

Late medieval and early modern towns as focal points of market power. An interview met Stephan Epstein¹

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Stephan R. ('Larry') Epstein – not to be confused with his Texan namesake Steve A. Epstein, also a mediaevalist specialized on Italy – was born in Germany in 1960 and grew up in Switzerland. He read history at the University of Siena, Italy, and took his doctorate at the University of Cambridge. He has been a Fellow of Trinity College, Cambridge, and has taught at the Universities of Cambridge and Siena. He is currently Reader in Economic History at the London School of Economics and Political Science.

Publication of his article 'Cities, regions and the late medieval crisis: Sicily and Tuscany compared' in the 1991 issue of *Past and Present* instantly established his reputation as an innovative economic historian with a refreshing insight into the late medieval and early modern economy and society. This was even before his magnificent monograph on Sicily, *An island for itself. Economic development and social change in late medieval Sicily* (Cambridge etc. 1992), came out. It was closely followed by several other important articles in major journals such as the *Economic History Review* and the *Journal of Medieval History*. This fall two new books will appear, one entitled *Freedom and growth. The rise of states and markets in Europe, 1300-1750*, the other (a volume of articles edited by Epstein) *Town and country in Europe, 1300-1800*. Less well known outside Italy is Epstein's prolific work in Italian; in fact his first book – *Alle origini della fattoria toscana* (Florence 1986) – was in Italian. Through the years Epstein's work has remained sharply focused on various broad theme's of late medieval economic history: urban hierarchies and market structures, town-country relations, and urban industries. It is always well embedded into relevant historiographical debates, whether it be on the nature of the late medieval economic 'crisis', on the 'Mezzogiorno' problem (the intriguing question where

¹ This interview was conducted on 18 February 2000. We would like to thank Larry Epstein for his comprehensive help with the editing of the original rough transcript.

to look for historical roots of the relative backwardness of the Italian south) or on the profitability of the toscan ‘mezzadria’ (sharecropping) system.

Were you trained as an historian or as an economist?

No, as an historian. I moved to economic history really by chance while working as an undergraduate on poverty and charity in Siena. Actually, I never had any formal education as an economist. This is a disadvantage. Economics is a formal language, a sort of code, that can say very complicated things with only a few words. Also, a working knowledge of economics enables one to speak to people who are extremely clever in thinking about things that historians are not taught to think about. On the other hand, historians have other advantages. They develop a sensitivity to context that is extremely important but which is very difficult to learn quickly. If you have to learn something late, it is better to learn economics late than history. It takes time to become a good historian, to master the detail, to learn to read documents for what is important and what not, to see the hierarchy of things.

Within the field of economic history you are known as an institutionalist. Do you agree with this characterisation?

I do and I don’t agree. If it is meant as a broad characterisation of a concern with the formal constraints or parameters of economic activities, then yes, I am an institutional economic historian—but that’s pretty much what most economic historians do anyway. In recent years, it is true, I have been concerned specifically with the arguments developed by New Institutional Economics (NIE), particularly in the version developed by Douglass North.² North has been hugely influential, as the award of a Nobel prize in economics testifies, and has played an important role in the current convergence—which I see as one of the most exciting intellectual developments of the past decade—between economics, political economy, historical sociology and history. The reason it has been so influential is that it has seemed to offer a theory, couched in neo-classical language, for why institutions matter for economic performance; and because he

² Especially Douglass C. North, *Institutions, institutional change and economic performance* (Cambridge etc. 1990).

was also able to offer, on the basis of that theory, a provocative explanation for the rise of West.³ His argument that incentive structures matter is hardly new. However, he was able to couch it in a language and a theory that captured the imagination not only of economic historians but also of political scientists and economists. The fact that North's work was being recognised just at the time of the fall of the Berlin Wall also helped, because his theories were seen as relevant to the transition from command to market economies; North has acted as advisor to the Czech government and in Central America.

My new book – to be published in September – will engage with testing the claims of NIE, with which I have become increasingly dissatisfied. But my original interest in institutions came from Marxism, which is a theory of progress based on the interaction between institutions and technology, and I still feel that Marxism is a necessary point of departure. Glib dismissals stem from ignorance: 80 per cent of Marxism has now become common sense among political and social scientists, although nobody seems to realise this anymore. North himself (and Robert Fogel, his joint Nobel prize-winner) was originally a Marxist, and indeed his own theory could be described as a watered down version of Marxism with the technology and class struggle left out.

How would you define institutions? In North's work, for example, it is often difficult to grasp what he means by them.

North has refined his definition over time. Perhaps his most useful definition is based on the distinction between economic organisation and economic institution, where the former is the normative system binding a group of people engaged in a common economic activity, and the latter are the broader 'rules of the game'. Such rules can be both formal laws, backed by coercion, and informal norms that are more internalised, codes of behaviour that define what is acceptable and what is not and are backed by social and cultural sanctions. These distinctions are useful additions to the palette of rather crude primary colours that economic historians have traditionally worked with. In his interpretative work, however, North has proved curiously colour blind, having mainly focused on the effects of constitutional structure on economic performance. More recently he has responded to the challenge of cultural historians and anthropologists, who say: 'how do you make sense of culture', by attempting to

³ D.C. North and R.P. Thomas, *The rise of the western world* (Cambridge 1973).

endogenise culture through cognitive science. North hasn't been very specific about this, and it strikes me in any case as an epistemological dead end. Not just because the cognitive sciences are still in their infancy as far as explaining individual behaviour goes, but because the kind of evidence historians play with is simply not up to the challenge. Avner Greif has made a very clever attempt to take up North's agenda by contrasting 'collectivist' and 'individualist' behaviour among medieval Maghribi and Genoese traders respectively. But his claim that the different patterns of behaviour are down to cultural factors relies on 'holding all else constant', that is, on proving that other, non-cultural factors made no difference. In fact, Greif fails to take into account the different political framework within which the merchants operated. Behind Genoese merchants there was a 'state', which provided the sort of collective benefits and public goods (defence, law and order etc.) that stateless Maghribi traders had to provide themselves. In other words, individualism is the counterpoint to the state, not an independently generated cultural option.

Let us get back to your career, and to the influence of Marx and North on the development of your ideas on history.

I was dissatisfied, as a Marxist, with certain of the institutional assumptions underlying not Marxism as a general theory (which is on the whole rather vague), but specific neo-Marxist narratives of how premodern economies worked and of the transition to capitalism. These narratives were too narrow. In a nutshell, they either resorted to exogenous factors like overseas trade (which not only vitiated any claim about the primacy of technological progress and property rights, but had little empirical foundation), or focused the explanation of the transition to capitalism entirely on the nature of property rights to land. But property rights to land—in the sense of who owns the land ownership—is not an accurate predictor of economic performance. Who owns the land cannot tell us which developmental path either agriculture or the broader economy is going to follow.

So you deny that the social division of property rights to land was an essential determinant of economic development – or rather, underdevelopment - in premodern agrarian societies?

It depends on what you mean by ‘social division of property rights to land’. If you restrict the definition to land title, as Brenner does, then yes, I deny that it matters whether the land is owned by the cultivator or by a landlord. Premodern Italian history offers a classic paradox, ignored by the literature, where bourgeois property is absolutely dominant by 1500, but the country’s agricultural performance actually gets worse in relative terms after 1500. If performance depended simply on ownership, Italy should have been the first capitalist economy in Europe; but the transition doesn’t occur because of the presence of other factors—which include restrictions on trade, exploitative taxation of the countryside, and constraints on rural protoindustry—that disincentivise investment and innovation. What matters is the broader institutional context. Now, if you want to call that complex bundle of fiscal, legal and ownership rights that allocate agricultural rent streams (the ‘surplus’) as ‘property rights to land’, I’m happy to go along with you. But then we’re playing with words.

This stance situates you in square opposition to Robert Brenner.

Yes. Brenner made a very forceful case in the 70’s that was historiographically very useful.⁴ He challenged the prevailing neo-Malthusian (not neo-Ricardian) model, and brought the neo-Marxist agenda, which had been marginalized since the Dobb-Sweezy debate of the 1950’s⁵, back to the centre of debate. But his argument is derivative, largely pieced together from Dobb, Bloch, Barrington Moore. It is also factually wrong on two essential points concerning premodern technological change and the capacity by owner-cultivators to innovate. This may explain why Brenner is far more cited by non-specialists than by historians, because he seemed to make sense of what rival theories could clearly not make sense of. But looking back, what strikes me most is how similar to the neo-Malthusian orthodoxy Brenner’s theory actually was: the only significant

⁴ Brenner’s famous *Past and Present* article of 1976 (‘Agrarian class structure and economic development in pre-industrial Europe’) has been reprinted, together with the main critiques and Brenner’s reply of 1982 in the volume *The Brenner Debate. Agrarian class structure and economic development in Pre-Industrial Europe*. T.H. Aston, C.H.E. Philpin [eds.] (Cambridge etc. 1985).

⁵ The main contributions have been collected in the volume *The transition from feudalism to capitalism* [introduced by Rodney Hilton] (London/New York 1976).

difference was the emphasis on property rights to land rather than demographic pressure.

How did your ideas on history develop? Did you first have the ideas, and then went to look for the material to test them, or did you work the other way round? In other words, how on earth did you get in Sicily?

As an undergraduate I was in Italy, and worked on Tuscany. After I graduated there was little point in staying; the academic prospects were not good. I also wanted to avoid becoming a regional historian, a historian of Tuscany. In the world of Italian history at the time there was a clear division of labour, firstly between historians of the North and historians of the South, and then on a regional basis. This is now changing: historians are becoming more comparative and 'European'; but it is a rather recent development and has still to come to full fruition.

What do you think then of French historians of Italy, like Pierre Toubert, Jean-Pierre Delumeau and Henri Bresc? They all went for the in-depth regional study, and have published impressive regional monographs.⁶ How in particular did your work on Sicily relate to Henri Bresc's?

The work by the historians you mention, and by others, is first class. It draws on the tradition of the regional *grand thèse* established by Fèbvre and the school of French historical geographers; its ambition became the *histoire totale*. That was also Bresc's ambition: after all, he began his work in the mid-'60s, at the height of the prestige and influence of Braudel and the *Annales*. But I believe that the current *Annalistes* themselves are rather disillusioned with that tradition, perhaps because they began to uncover the theoretical underpinnings of those regional histories and found them poorly constructed. That is, in a sense, what happened to me with Bresc's work. Obviously I know him, and indeed when I was beginning my research he invited me to his home in Paris to read his *thèse*,

⁶ I.e. Pierre Toubert, *Les structures du Latium médiévale: le Latium méridional et la Sabine du IXe à la fin du XIIIe siècle*. 2 Vols. (Rome 1973). Henri Bresc, *Un monde méditerranéen. Économie et société en Sicile, 1300-1450*. 2 Vols. (Rome 1986). Jean-Pierre Delumeau, *Arezzo: espace et sociétés 715-1230. Recherches sur Arezzo et son contado du VIIIe au début du XIIIe siècle*. 2 Vols. (Rome 1996).

which was then still unpublished and which took me five days of concentrated reading to digest. He had done a huge amount of work, quite unprecedented for Sicily: he put late medieval Sicily on the historiographic map. His interest—perhaps not at first, but developed over the years he spent researching—was in situating Sicily within the ‘world-systems’ framework of Braudel and Wallerstein (remember how much Braudel emphasises Sicily in the *Méditerranée*), in combination with a, paradoxically very conservative, southern Italian rhetoric of colonialism. My original interest was similarly in the origins of southern Italian underdevelopment, which was central to the post-war Italian political and economic debates. But being in Cambridge gave me the distance and perspective that I needed to break out of that mould. Had I stayed in Italy, my work would have been completely different.

What, as a *Mezzogiorno* specialist, do you think of Putnam’s book, which appeared when you were working on your Sicily book?⁷ Putnam is also a sort of institutionalist.

I think it is a terrible book, so obviously using history in an instrumental way to make a rhetorical point. Although he’s a sociologist, Putnam does not seem to understand how societies and institutions work in the long run.

He still won an awful lot of prizes.

He may well be right about the twentieth century. But tracing back present-day anomie to specific contemporary causes is one thing; tracing its origins back to the communal era in the North and the Normans in the South respectively is quite another. The arguments presume totally different patterns of causation, but Putnam presents them as if they were equivalent. It’s a bit like saying that present-day Russian problems are a tradition of oppression originating with the Tartar yoke: it’s a nice line for a television sound-bite. Similarly, many Italians may be sympathetic to Putnam’s historical argument, but for ideological reasons associated with current political debates, not because of his historical expertise.

⁷ Robert D. Putnam [with Robert Leonardi and Raffaella Y. Nanetti], *Making democracy work. Civic tradition in modern Italy* (Princeton 1993).

The question still is, whether history can be of help in explaining the Mezzogiorno problem. In any case, in your contribution you worked from the other direction; from the deep, Norman past into early modern times.

I came to the topic believing the traditional story.⁸ After I went to Cambridge in 1985, I read everything noteworthy on the subject, I immersed myself in the literature. It is the best way to get a sense of the historiography, to grasp how historians working within certain traditions select their evidence, to get a clear idea of what is said, what is left out, and what the biases are. But very soon, already before reading Bresc's thesis, I realised that the evidence did not fit the theory. The hard bit was then to develop an alternative 'grand narrative', which took me quite some time. Things started with my thesis on the economic administration of a large Sienese hospital. It turned into a study of a market within the market. The hospital managed its properties like a modern multinational, with a number of independent granges whose operation was coordinated from the centre; the system was both parallel to and integrated with the regional and urban markets. Through this I became fascinated with debates on the late medieval crisis, which went to the heart of what premodern markets were and of how they operated.

In these debates towns and markets take a large place. The masterclass you had been invited to address is about urban history. Is urban history a separate field, with a right to stand apart in historiography?

Yes instrumentally, but no methodologically. Instrumentally it has played a very useful role in creating a network of people who work on issues related with urban history; useful also in providing a focus, and an institutional framework with conferences, journals and so on. This has hugely increased our knowledge, refined methods, made historians more open to social science methodology. But methodologically it has no separate identity: I have studied urban history, but

⁸ This tells that the relative economic backwardness of southern Italy goes back to the late medieval crisis. Instead, Epstein (with the French historian Maurice Aymard, but for different reasons) sees the crisis starting in the second half of the seventeenth century as the decisive breaking point. See especially *An island for itself*, 402-412.

wouldn't call myself an urban historian. Urban history is simply history through an urban prism.

But as an institutionalist you must have observed that medieval towns were special places, embedded in special legal arrangements, which set them apart and made them 'work' in a way.

Yes, but again I would emphasise that you can only make sense of towns in a broader context, which is not only the relations with the countryside, but also with the state, or the feudal system as a whole. Setting towns apart is to hypostatize them, to reify them. This was of course standard practice until the 1970s, the early 1980s. One talked about 'the town', 'the city'. And of course all towns share certain similarities, such as their functional role in the division of labour; but underlying these common functional aspects are the differences, which are more interesting. After all what we are trying to explain is: why are towns the way they are? To understand this we do need to make comparisons, these 'what, if' questions. My interest in the institutionalist paradigm is as a comparative historian interested in explaining differences. If comparative history is where the discipline is going, which I firmly believe it is, then we as historians need to reflect more than we normally do on counterfactuals. The institutionalist paradigm just offers a slightly more rigorous framework than others to address the counterfactuals.

Did urban functions change in the later Middle Ages according to you?

No I don't think they did in an abstract sense. The main difference was of course the rise of the state, and the increased centralisation that went with it, which expanded the potential, if not always actual, administrative, political, and even economic functions of towns.

Could you explain then why the later Middle Ages have been so crucial for later economic development?

As I said before, I really got interested in economic history through the debate on the late medieval 'crisis'. But how was the debate framed, when I began trying to make sense of it twenty years ago? It was about agriculture, about

diminishing returns, it was the Ricardian story. But I was working on markets, and markets were the missing factor in the equation. It is not that historians denied that trade existed; it's just that it didn't matter. Michael Postan and Robert Brenner are quite agreed on the following syllogism: medieval peasants aimed for security, security could be best achieved with subsistence farming, therefore medieval peasants did not trade. In my story, by contrast, markets - which are extensively documented even in older work that denies that they were economically significant - are *the* explanatory factor in the transition. One way of describing the transition is as a movement from a less commercialised to a more commercialised society; but when I began work there was no model on offer that could explain how one got from point A to point B. And I think that it is in the changing structure of markets - of that bundle of rights that I described before as the 'context' for the operation of property rights to land - that we can find the explanation, firstly for the transition to capitalism, and secondly for economic divergence between regions. That's where my contribution to the debate lies, namely looking at the role of markets, and how markets changed, became more competitive and more integrated over time. I see the late medieval crisis as critical in weakening or abolishing barriers to trade, and sources of rent seeking that existed previously.

Then the next question would be what were markets? Classical economics refers to ideal-type markets with perfect competition and so on. Of course, those markets did not exist in the fourteenth century. So, what sort of markets are you referring to?

You're right, and, well, I am referring to markets in which prices were set by demand and supply, but in which market power (monopolistic, monopsonistic, oligopolistic powers) arising from asymmetric information, economies of scale, political power, and what have you was widespread; but there is now a large quantity of modern economic theory that takes it on board and seeks to model it. Until very recently economic theory did not have developmental theories, dynamic theories that could incorporate non-competitive markets (or multiple equilibria, in their jargon). That is now changing very rapidly, and that's where the institutionalist contribution has made an impact, because they insisted, in

language economists took seriously, that we need to take these non-economic forces into account to explain how markets develop.

Using new price data and data on interest rates that allows me to measure market integration, I argue that although we observe a very long run trend towards market integration as far back as the 13th century (when the first evidence becomes available), there is a break point in the trend, a structural break after which markets integrate more rapidly than the previous trend would predict, if you see what I mean.

And this happened in the fifteenth century?

This happened in different regions of Europe to a different extent at different points in time between 1350 and 1450.

Isn't it strange that you got more integrated markets in a more empty world, a world that had suffered from heavy and protracted depopulation? One would imagine that the market economy would be severely disrupted.

That is certainly the general assumption in the literature. My answer would be that when so many people die there are really two things that can happen *a priori*. Either you get autarky, as Postan argued, because fewer peasants can subsist on more land, or you get diminished pressure on resources at the margin and greater freedom to experiment. I argue that the latter happened, because it was combined with two additional factors. First, state centralisation (I mean integration of separate jurisdictions, introduction of common measurements, abolition of tolls, and so on, all of which have to do with the rent-seeking abilities of different social groups which are negotiating with states over these) accelerated in this period, and systematically reduced transaction costs; second, individual mobility in this period increased dramatically. During the late Middle Ages, people became more mobile, partly for exogenous factors—warfare—partly also for endogenous reasons, the opportunity to improve one's lot by moving to a neighbouring village or region, which was perhaps the main cause for the disappearance of serfdom in England. An important effect of increased mobility was the speedier diffusion of technical knowledge, for which there is a lot of evidence.

What you forget is that labour got more expensive, so the per capita purchasing power of substantial parts of late medieval populations rose. Wasn't that the main driving force behind intensified market activities?

As I said before, the presumption until the 1980s - led by Postan⁹ - was the other way round, that market activities decreased because peasants, faced with lower rents and more and better land could finally achieve their ambitions, retreat from the markets, avoid market risks, and follow a safety-first strategy. Peasants, according to this view, aren't willing to engage in production for the market. This was the paradigm adopted by neo-Marxists in their explanations of the crisis. For Wallerstein it was an underconsumption crisis for which you needed an exogenous factor, i.e. the overseas discoveries, to generate new demand. Brenner doesn't really have much to say about the late medieval crisis, but it doesn't seem like a good period; it's a sort of black hole between the Black Death and the mid-sixteenth century. From the late '80s there's been a rediscovery of late medieval consumption; it's something I also emphasised. But without denying the role of increased consumption in giving shape to the late medieval economy, I would now place more emphasis on the supply side.

So you are saying that peasants at this time were indeed voluntarily seeking the market?

Yes. The evidence of growing protoindustrial activities seems indisputable. In the old model this would be inconceivable. There is a story of how John Hatcher, now a distinguished professor at Cambridge University, who as a graduate student of Postan's did find evidence of such industrial activities in late medieval Cornwall on which he was writing his thesis. So he went with them to

⁹ One could think of Postan's general co-editorship of the very influential Cambridge Economic History of Europe, which appeared from 1942 onwards. Postan also edited two parts on medieval economic history. And further: M.M. Postan, *The medieval economy and society: an economic history of Britain 1100-1500* (Berkeley 1972), and *Essays on medieval agriculture and general problems of the medieval economy* (Cambridge 1973). There is a useful – as well as a bit malicious – piece on Postan in Norman F. Cantor, *Inventing the Middle Ages. The lives, works, and ideas of the great medievalists of the twentieth century* (New York 1991), chapter 10.

Postan, and Postan just said, this can't be. So Hatcher published his book¹⁰, and described the activities, but didn't make much of the point because Postan said it was impossible! My point is simply that the evidence was available, and was even published, but the prevailing view that the late medieval economy was *less* commoditised was so powerful, and seemed to explain so much, that alternative views (such as that of Alan Bridbury at the London School of Economics¹¹) were ignored. And not only in England. Just look for instance at Guy Bois' book on Normandy¹²; it is all about grain production. Or Le Roy Ladurie's work on the Languedoc peasants.¹³ Neither of them ever considered the impact of markets: their theories told them that markets were irrelevant.

Then what do you think of Jan de Vries' idea of the industrious revolution?¹⁴

I do indeed draw a parallel between with what he argues for the seventeenth century and what happened in the late Middle Ages. As I said, I do put more emphasis on changes in the supply side. De Vries seems to suggest that the 'revolution' is down to a change in preferences, a desire to consume more, and to work more in order to consume more. I would add to that the greater ease with which rural industries - which are central to both De Vries's and my interpretations - were set up, largely because the coercive powers of towns over their hinterlands were significantly weakened during the two periods. This argument about the late medieval crisis is set out in a comparative study of cloth production in three Italian regions: Lombardy, Tuscany and Sicily, in a chapter of my new book. The most interesting contrast is between Tuscany and Lombardy. There is no proto-industry to speak of in Tuscany, but there is a lot in Lombardy. Lombard industry was based in small chartered towns which were able to set up new manufactures despite strong urban pressure to stop them, because they were protected by legal privileges and franchises, often very

¹⁰ John Hatcher, *Rural economy and society in the Duchy of Cornwall, 1300-1500* (Cambridge 1970).

¹¹ A.R. Bridbury, *Economic growth: England in the later Middle Ages* (London 1962).

¹² Guy Bois, *Crise du féodalisme: économie rurale et démographie en Normandie orientale du début du 14^e siècle au milieu du 16^e siècle* (Paris 1976).

¹³ Emmanuel Le Roy Ladurie, *Les paysans de Languedoc*. 2 Vols. (Paris 1966).

¹⁴ Jan de Vries, 'The Industrial Revolution and the Industrious Revolution', *Journal of Economic History* 54 (1994), 249-270.

ancient, that had been confirmed by the new territorial lords who sought the small towns' support. So in that sense, proto-industry was supported by the rise of territorial states, which pursued its own political strategy by negotiating political and economic rights. The same thing happened during the later medieval period in many parts of Germany, in Flanders, and also in Holland. It was a form of infant industry protection.

You have been referring to your coming book a couple of times now. What will it be about?

It will be about whether states mattered for economic growth in premodern Europe, and why.

Well, what is the answer?

My answer is that they did matter, but not for the reasons claimed by neo-institutionalists, particularly Douglass North and other North-American scholars, that different constitutional structures secured property rights differently. In their view, what explains different patterns of growth or stagnation in premodern Europe is the predatory nature of the state, and only where rulers were constrained by constitutional arrangements like what emerged from the Glorious Revolution in England or the Dutch Revolt, did property rights become sufficiently secure to create the incentives for long-run growth. So, the book tests this argument, and concludes that the evidence does *not* support North's case. For example, republican regimes, which from a nineteenth-century liberal point of view are more free than 'absolutist' monarchies, at least for the elite, the ruling group, are *ceteris paribus* less conducive to economic development than the so-called absolutist states.

So most of the book is about what aspect of state activities *did* matter. As I see it the institutional barrier to growth is not the power of the state but the weakness of the state. So-called absolutist states, which were of course the majority in early modern Europe, were in practice weak states that had to come to terms with a large number of domestic constituencies which claimed legal title to pre-existing rents, or which negotiated their support for the state in exchange for new rents. Seen from this angle, the history of European state

formation is the story of how states slowly eroded those ‘private’ rights and concentrated in what became the ‘public’ sphere. In that sense the late Middle Ages and the seventeenth century were crucial, because they both witnessed a rapid intensification of military activities, which was the only legitimate way with which states could extend the boundaries of their jurisdiction over taxation and law.

That is the general theory, which I test in the second part of the book where I examine how states could shape incentive or market structures. I approach political regimes not as ways to expropriate the taxpayer, as the North American public choice literature and New Institutional Economics do, but as ways to facilitate economic cooperation for mutual advantage. Markets are public goods based on cooperation, and cooperation requires enforcement. My main hypothesis is that the most important economic consequence of the rise of centralised states is that they made coordinated and competitive markets possible. I test this by examining how states in three Italian regions with different political structures influenced the development of grain markets, proto-industry, and urban hierarchies.

What in the end matters is not the type of regime, or the degree of urbanisation, but the efficiency with which an emerging state knows how to organise markets?

Well, I am not arguing that this was part of a *deliberate* strategy by states. There was a logic to state formation, namely the extension of sovereignty which was the prerequisite for being able to tax. In order to extend sovereignty states had to negotiate with their constituencies. State formation is the result of negotiating over constituencies’ rights - such as rights of feudal and urban jurisdiction, fiscal rights, corporate rights to corporate claims to rents - in exchange for other rights. More effective and coordinated markets were the unintended consequence of this process. I don’t mean that it was a smooth developmental process, either. State formation clearly entailed major redistribution of resources, which frequently gave rise to civil warfare. But when we compare states circa 1300 and circa 1815, one of the most important aspects from an economic point of view—possible *the* most important aspect—is market coordination. What the state does, is arrogate and concentrate the right to determine how markets are organised. Today it is the state, not me as an individual or member of a corporate body that

decides whether I can wield an economic right over something; we take this for granted. How we got to this point however is the result of a long drawn-out historical process. It is the unintended consequence of a logic of state formation which is driven both by the rulers who wish to tax more and increase their power, and by those among their constituents who stand to gain from a single, unified jurisdiction. For there are clear benefits to be had from having no tariffs within one's territory, of having no competitors claiming monopolies etc., in terms of lower transaction costs and more efficient allocation of resources. But to achieve those benefits often meant that agents had to give up the same rights themselves; this is what made the process of state formation so complicated and drawn out. But on the whole merchants were supportive of state centralisation, in so far as it reduced barriers to trade, lowered transaction costs within domestic markets, etcetera. They only opposed it if they thought the gains were outweighed in the medium term by the loss of their traditional rights.

This is the dangerous liaison between states and merchants that Charles Tilly is always talking about?¹⁵

Yes. *Vis à vis* Charles Tilly I would emphasise that merchants also had political stakes, that they also used political means to achieve economic ends; that this was not simply a conflict between coercive states and mobile capital. And also, as I just noted, that merchants were on the whole the strongest *allies* of centralising states, rather than their foes as Tilly argues.

How can this entire process be related to urbanisation? Did states with higher levels of urbanisation perform better in this respect?

Well, I think in principle yes, because societies with higher levels of urbanisation would seem to have overcome the problems with coordinating markets more effectively than societies with highly dispersed populations. Dispersed populations result in very high transaction costs; urban concentration arises because there are economies of scale to be captured. Historically however there was another force at work driving urbanisation, which was the extent to

¹⁵ E.g. Charles Tilly, *Coercion, capital, and European states, AD 990-1992* (Cambridge Mass./Oxford 1990), 58-61.

which cities could organise trade in their hinterland, organise economic relations between town and countryside. In so far as towns helped to coordinate exchange between dispersed producers and consumers, the relation between town and country was a positive one. But underlying these relations was a paradox, because premodern towns also needed powers of coercion to coordinate effectively, and those coercive powers could easily be used for inefficient rent seeking. That is why premodern towns opposed the rise of competing marketplaces and fairs in the countryside, even though these lowered rural transaction costs and increased aggregate welfare, made everyone better off. The towns - or influential segments of the urban elites - opposed the new marketplaces because they thought they would divert trade away from urban markets and lose valuable tax revenue.

To return to your general point, I suggest that the same institutional forces that sustain urbanisation by giving the powers to enforce cooperation in the rural hinterland, may turn into fetters to growth in so far as towns are able to defend their rights and privileges against the encroaching state. That is, towns could use their privileges to reorganise the economy more effectively and more productively, but when those powers turned into sources of rent they smothered further growth. This institutional sclerosis may be the explanation for the remarkable stability in levels of urbanisation in many parts of Europe between 1500 and 1800, despite underlying economic change—growth or decline—in this period. There are very few exceptions. The main exceptions are three areas where medieval towns had comparatively poor powers of coercion over rural activities, but where the central state was strong. These areas were England, Sweden and (to a lesser extent) the northern Netherlands. All three started from very low urban bases; Holland (though not the rest of the country) began to catch up with the more advanced European regions in the fifteenth century, the others caught up during the sixteenth and seventeenth centuries. The main reason for these countries' low rates of urbanisation *circa* 1400 is precisely because their towns were less coercive and could not enforce market cooperation very effectively. Once the trading links had been established, however, the economy could evolve relatively unencumbered by urban rent seeking. It is significant that Dutch economic and urban growth came to a halt when the Netherlandish towns became less co-operative after the end of the Revolt.

This argument makes the French Revolution even more interesting, since it marked the end to whatever remaining coercive powers of western European towns. What happened to urbanisation in the first half of the nineteenth century?

Well, it takes off. But so many things were going on at the same time - the transport revolution, political integration and so on - that it is very difficult to tease out the necessary and sufficient from the merely contingent factors. The French Revolution clearly was a watershed, although it marked the symbolic culmination of a centuries' long process - which from the latter half of the seventeenth century became increasingly a *deliberate* project - to overcome feudal particularism, rather than the beginning of something entirely new. This process had been accelerating during the eighteenth century, which explains why urbanisation began to increase again and to converge across the whole of Europe. But change was slow: often states found it easier to deal with particularism not by abolishing existing privileges, but by creating new ones that undermined the existing ones. It was an excruciating process, two steps forward one step back, whose effects worked themselves out very slowly. The French Revolution had been preceded by a century and a half of political debate about the pernicious role of particularism - even mercantilism can be seen as a project against particularism - and these concerns finally coalesced in the Revolution.