1 Introduction

D'Maris Coffman and Larry Neal

'Credible commitment' has become one of the most widely used phrases in the literature of both economics and political science. A search of *EconLit*, an electronic bibliography of economics articles, brings up 1,932 hits for the phrase in texts published between 1976 and 2012. Of these, 1,894 occur after the publication of North and Weingast's classic 1989 article 'Constitutions and Commitment: The Evolution of Institutions Governing Public Choice in Seventeenth-Century England'. Clearly the attraction of the phrase derives largely from the appealing story they tell: institutional change arising from England's 1688/89 Glorious Revolution created, for the first time, a 'credible commitment' that the government would not default on its debt in the future. 'Whig historians', from Henry Hallam and James Mackintosh forward, had also pointed to the importance of the Glorious Revolution for the increasing military success of the English mercantile state after 1688, in contrast with its inconsistent performance in the three Anglo-Dutch Wars from 1652 to 1674. The ability of William III and Marlborough to challenge the forces of Louis XIV from 1689 to 1713 presaged a century of rising economic and political power. North and Weingast, however, put their emphasis on the economic consequences of the political changes that occurred in 1688. They argued that the contrast between the lacklustre performance of the English economy before 1688 and the increasing strength of its overseas trade afterwards resulted directly from the change in political institutions.

Their narrative fits well with the implications of economic models of control systems being developed in the late 1980s.These found that credible commitment mechanisms had to be in place to keep political authorities from making time-inconsistent policy decisions. Beginning with Lucas's critique of macroeconomic models based on existing patterns of economic behaviour by the private sector (1976), economists realised that private agents would have an incentive to change their behaviour in response to changes in economic policy. For example, a persistent increase in inflation rates generated by permissive monetary policy would not lead to a permanent decrease in unemployment. The

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Lucas critique was followed by Kydland and Prescott's demonstration (1977) that initially optimal plans would turn out to be suboptimal even if planners changed policies in response to new behaviour by private agents. The resulting 'time inconsistency' of public policy would, for instance, lead private decision-makers to respond by restricting investment in light of the resulting uncertainty. Economic growth would be permanently lowered. The result of the persistent disruptions to the international and domestic economies after the oil shocks of the 1970s and the rise of globalisation after 1971, and again after 1990, has led macroeconomists to develop ever more sophisticated models to help policy-makers devise appropriate responses to exogenous shocks. A major effort has been the search for micro foundations of macroeconomic models that would establish incentive-compatible contracts between, for example, central banks and the financial sector, or governments and debt holders.

In this context Persson and Tabellini, policy-oriented economists deeply involved in the formation of a new international economic order, beginning with the founding of the European Central Bank in 1998, found the North and Weingast story very appealing. They remark that 'delegating much of the power over fiscal-policy decisions to the Parliament . . . relaxed the previous incentive constraints and increased the government's credibility for repayment' (Persson and Tabellini 1994, 1: 21). More recently, Daron Acemoglu (an economist) and James Robinson (a political scientist) state boldly that 'The Glorious Revolution was the foundation for creating a pluralistic society, and it built on and accelerated a process of political centralization. It created the world's first set of inclusive political institutions' (Acemoglu and Robinson 2012, 102).

The thrust of analysis of the failures of economic policymaking, repeatedly evident since the end of the 'golden age of economic growth' in the early 1970s, has been for both economists and political scientists to explore the interactions of economics and politics. Lacking experimental evidence from laboratories, theorists seeking validation of their arguments must turn to the material generated by political and economic historians. Too often, however, the evidence useful for economists has to be teased out from the pages and pages of contextual descriptions that are so characteristic of the history profession. The North and Weingast article did exactly that for both economists and political scientists. From the massive collection of details presented in Dickson's classic work on the financial revolution in England (1967), North and Weingast extracted the pertinent details that illustrate the importance of institutions for determining economic outcomes. It was Dickson's demonstration that the rise of the capital market for British government debt followed the accession of William and Mary in 1689 that North and Weingast took as

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'the single most important piece of evidence' that the necessary condition of securing contracts across time and space, and therefore enabling impersonal exchange, had been fulfilled. That breakthrough, they argue, followed (albeit gradually and with setbacks) from Britain's continued economic and military success, and led to victory in the wars against France, and eventually to the Industrial Revolution.

Among historians, however, the 'credible commitment' thesis has been subjected to both criticisms and elaborations. The critics picked away at the historical evidence North and Weingast used to support their argument that the constitutional break in 1688 was definitive for England. North and Weingast acknowledged then, and in all subsequent writings by each of them, that constitutional change is subject to historical contingency, which means it is not sufficient for establishing credible commitment mechanisms, and is potentially reversible. North (2005), for example, emphasises the complexity of the interplay of beliefs, institutions, implicit incentives, and political systems for enforcing the formal and informal rules that make it so difficult to displace dysfunctional institutions with more effective alternatives. He argues that the ultimate test of new institutions is their adaptive efficiency in responding to new economic challenges. Weingast (2005) takes up the question of why the constitutional commitment made in 1689 was self-enforcing. He argues that the prior disposal of James II as monarch made it clear, from then on, that Parliament could depose the king, the ultimate sanction to ensure that future monarchs would keep their commitments to maintain the new constitution. At the end of that essay, moreover, he concludes that 'Both crises and ongoing constitutional adjustments seem central to the creation of self-enforcing constitutions that are stable for multiple generations' (Weingast 2005, 106).

Both North and Weingast had to clarify and sharpen their original argument about the sources and results of England's constitutional commitment in light of earlier challenges by economic historians. O'Brien and Hunt (1997) argue that the evolution of British public finance began much earlier, at least during the English Civil War, and some improvements were made during the Stuart Restoration. Epstein (2000) notes that interest rates on British government debt remained well above those already achieved in the Netherlands and various Italian city-states well into the eighteenth century. Sussman and Yafeh (2006) take up the interest-rate argument to show that market interest rates remained high on most British government debt for several decades after 1688. They refer to Quinn (2001), who showed that private interest rates remained high due to wartime demands for government finance during the reign of William III. Quinn, however, does note that the huge increase in the size

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of government debt provided a useful reserve asset for private bankers, whose business increased as well. The enormous quantity of government debt issued after 1688, which continued to increase in each war thereafter, remains the most impressive evidence in support of the original North and Weingast thesis, a point made repeatedly by Weingast in later work (Weingast 1997; Weingast 2005).

Examining the importance of constitutional provisions in general, Clark (1996) argues that property rights were already well protected in English law. He found no changes in yields on land mortgages in response to increases in Parliament's authority. DeLong and Schleifer (1993) had earlier demonstrated that European cities with absolutist institutions grew more slowly in population than cities within nonabsolutist regimes between 1000 and 1800. Acemoglu, Johnson, and Robinson (2005) demonstrate that constraints on executives helped cities to grow larger on the Atlantic seaboard. Stasavage (2007) shows that European cities governed by merchant elites found it easier to borrow, and at lower interest rates, especially if the cities had constitutional constraints on their rulers. He finds less support, however, for the argument that constitutional checks by themselves increased the credibility of debt commitments for either city-states or territorial states. Stasavage (2011) finds that merchant elites in control of cities whose geography allowed them to remain compact and defensible were the most successful in achieving long-term credible commitments for their sovereign debts. Acemoglu and Robinson (2012) emphasise the importance of historical contingency for institutional change in general. They also make the point that, to adapt effectively to changing economic circumstances, constitutions must provide newcomers with access to power.

Most recently Cox (2012) argued that the Glorious Revolution was indeed a 'constitutional watershed', but for more precise reasons than simply controlling the king's access to long-term finance. For him, private property rights were not at issue in England under either Tudor or Stuart rule, so Clark's evidence on the stability of private returns, especially for land throughout the sixteenth to eighteenth centuries, is simply irrelevant to the constitutional question. Rather, the real issue for the Stuarts was how to assert absolutist rule in place of representative assemblies, as the Bourbon monarchs had achieved in France. To accomplish this goal, Stuart rulers had variously tried to rule without convening Parliament, to sidestep Parliament by expanding use of royal prerogatives, or simply to buy parliamentary support. He then shows that the post-1688 Parliament took meaningful steps to counter each possibility. It insisted on regular elections (initially every three years), limited revenue

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sources to fixed and ever-shorter terms for specific purposes, and, most effectively, controlled the appointments of the king's ministers responsible for collecting and dispensing the revenue. In this way, he emphasises a point, made earlier by Getzler (1996): in light of the irrationality of traditional English property law, which limited the possibility of more efficient use of property by re-allocation through market processes, an effective legislative power was needed to direct resources towards more profitable endeavours. 'In other words, the costs of irrational private law were balanced by the benefits of a legitimate and effective system of public taxation. A stronger governmental participation in the definition and control of private property rights at the microeconomic level might have undermined the legitimacy and success of the larger fiscal system' (Getzler 1996, 650). Several of the contributions in this volume highlight the growing importance of excise taxes as the basis for funding government debt, while land taxes remained stagnant and irregular, as did customs revenues after 1688 (Chandaman 1975; Braddick 1996).

Clearly it is the details of history that matter for determining the success of any institutional change, and examination of those details is the particular province of historians. Looking at the process of creating credible commitments between monarch and Parliament, and then between Parliament and the British public, scholars contributing to this volume find multiple instances of historical contingency creating watershed moments, as well as multiple mechanisms for creating commitments. A much more nuanced and complicated narrative emerges, which suggests not that North and Weingast were wrong in 1989, but that their story was incomplete.

Much of the attraction of their narrative was the apparent simplicity of the government's role in the economy at the time, unencumbered as it was by the expenditure demands of modern welfare states. Economic policy-makers today have to confront the multiple demands of an enfranchised public who desire full employment and access to health care and education, as well as public order and prosperity. In contrast, Britain's central government authorities in the seventeenth and eighteenth centuries confronted a much narrower, but equally pressing challenge: to maintain the British Isles free from foreign domination, whether military, dynastic, or religious. The international context was the increasing scale and expense of the warfare necessary to maintain both the authority of central governments and the territorial integrity of the emerging nation-states of continental Europe, in a period when the military revolution, which began in the fourteenth century, led to continued advances in technology, logistics, and finance.

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Not all European powers followed the same paths. Some evolved solutions that North and Weingast label 'absolutist', whereas others developed a 'constitutional' model. North and Weingast's formulation of this dichotomy is consistent with, and to some degree an inspiration for, the subsequent use of the terms by historical sociologists, who approach absolutism by employing it as part of a set of typologies that delineate the possible solutions to the problem of rule for the composite monarchies of Early Modern Europe (Gorski 2003, 3–15). These typologies are useful because they suggest a range of possibilities, but they say nothing about what happened in practice.

As with most late-twentieth-century debates, Marxist and non-Marxist variants were advanced. In *Coercion, Capital, and European States, 990–1990*, Tilly sees three divergent paths of political development: the 'capital-intensive' city-states of Northern Italy, Switzerland, and southern Scandinavia; the 'coercion-intensive' hinterlands of Russia, Poland, Hungary, and northern Scandinavia; and the 'centrally-ruled' national states of France, England, and eventually Prussia (Tilly 1990, 7–20). In his account, Tilly is heavily indebted to Wallerstein's neo-Marxist world-systems theory, which recasts class relations as exchange relations within a capitalist world system (Tilly 1990, 61). Unfortunately the Marxist version does not help historians to distinguish between elements within each set. As Abrams points out, 'Marxist theory needs the state as an abstract-formal object in order to explain the integration of class societies', and thus takes the state as a relatively unproblematic locus for the exercise of power (Abrams 1988, 70). The only way to get a different outcome in the Marxist account is through a different alignment of class or (now) exchange interest. Thus, for Marxists, absolutism (or constitutionalism) is not, strictly speaking, a modality of the exercise of power because, given a specific set of material realities and social relations, Marxist theory admits no competitive solutions. Variations in the forms of absolutism and styles of absolutist rule are of only passing interest. What made 'absolutism' absolutist was the nature of the monopoly on coercive power that it purportedly encouraged.

Downing (1992) and Parker (1996) see the military revolution as the impetus for political change. Downing claims that 'military-bureaucratic absolutism' emerged in Prussia and France because the ruling regimes there could mobilise vast domestic resources to finance their respective participation in the Thirty Years War. Downing then contrasts these regimes with those that he believes saw liberalism evolve from medieval constitutionalism. Ertman's account in *Birth of Leviathan: Building States and Regimes in Medieval and Early Modern Europe* (1997) is more

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sophisticated, offering a two-dimensional typology with two variables: organisation of local government at the time of state formation and timing of 'sustained geo-military competition'. From this, Ertman adduces four variants: patrimonial absolutism (France, Spain), bureaucratic absolutism (Prussia), bureaucratic constitutionalism (Britain), and patrimonial constitutionalism (Hungary, Poland). In this account absolutism versus constitutionalism denotes state structure, whereas bureaucratic versus patrimonial denotes regime structure. He explains the 'absolutist' outcome, usually in the 'core' states, in terms of a fledging *Ständestaat* divided among itself, unable to resist the ambitions of 'imperial' princes. In order to sidestep charges of overly schematic conclusions, Ertman introduces an element of contingency. Owing to the strength of her parliaments, Britain might have become patrimonial and absolutist under the Stuarts, but instead evolved a state bureaucracy under William. The chief advantage of Ertman's account is that it de-links bureaucratisation from absolutism, thereby making it possible to distinguish between France, Prussia, and Britain.

Ertman did not have the last word. Gorski's bold new synthesis, in *The Disciplinary Revolution* (2003), combines Michel Foucault, Max Weber, Norbert Elias, and Gerhard Ostreich to develop a 'comprehensive theory' of social discipline, one that includes self-, corrective, communal, and judicial discipline. His aim is not to explain absolutism, but rather to describe the Early Modern nonabsolutist state par excellence, the Dutch Republic. For him, the 'Reformation unleashed a profound and far-reaching process of disciplinary revolution – that greatly enhanced the power of the Early Modern states, and the effects of this revolution were deepest and most profound in the Calvinist parts of Europe'. Nevertheless his model can be applied to absolutist regimes as well, for he redefines 'state capacity' as 'a function, not only of administrative rationalisation, but of the strength of social infrastructure, and the rationality of sociopolitical ethics'. His model holds equally well in his analysis of the nonabsolutist Dutch and absolutist Prussian cases. This leads him to a revised state theory, under which states are 'not only administrative, policing, and military organisations, but also pedagogical, corrective and ideological' (Gorski 2003, 32, 38, 165). They operate as frequently through cooptation as through coercion; state power is not always dependent on state structure (but also on human resources, organisational fidelities, and infrastructure). State formation is as often a bottom-up as a top-down process. Early modern rulers, whether absolutist or constitutional, 'had multiple and potentially conflicting interests – ideal and material, geopolitical and eudaemonistic, personal and dynastic, long-run and short-run,

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and so on – and their efforts to prioritise and reconcile them were invariably influenced by individual *habitus* and collective valuations' (Gorski 2003, 168).

How well did these theoretical formulations of historical sociologists correspond to reality? England avoided most seventeenth-century wars, in part due to its defeat of the Spanish Armada in 1588, so its contribution to the military revolution really began at the conclusion of the Thirty Years War (1618–48). The Treaty of Westphalia (1648) forced a permanent retrenchment of the Holy Roman Empire, as the German states were divided along religious lines. Further, the Habsburg dynasty yielded the northern provinces of the Low Countries to a republic, which, along with the confederation of the mountain cantons of Switzerland, was recognised by the traditional sovereign powers of Europe as an equal power when they implemented the terms of the Treaty of Westphalia. For the Dutch Republic the treaty marked the successful end of an Eighty Years War for independence from Spain, and a new phase of political governance that focused on maintaining the prosperity of the Dutch mercantile elite under the political guidance of the long-established merchant guilds in control of the major port cities (Lesger 2006). For France, however, the Westphalia agreement merely meant that Cardinal Mazarin could now focus on continued war with Habsburg Spain, which ended with the Peace of the Pyrenees in 1669. The signing of that treaty completed the outlines of the borders of modern France, as well as the ascendancy of Louis XIV over the aristocratic revolt of the Fronde led by the Prince de Condé, which had threatened control over state revenues by parliament similar to that in the English and Dutch examples. By 1651, however, the revolt was subdued, and France could focus on the war with Spain.

The Peace of Westphalia mattered little for England, a country in the final throes of a civil war which climaxed the following year with the regicide of Charles I. The military victory of the New Model Army led by Oliver Cromwell and financed on a continuous basis by monthly levies of the land tax in 1650 was followed by the Navigation Act of 1651, which restricted coastal trade and fisheries to English and Welsh vessels. Presumably intended to thwart foreign incursions in support of Charles II, who had fled to Scotland, the Navigation Act provoked a short-lived and decisive naval war with the Dutch, who had been taken by surprise. The upshots were a flourishing English merchant marine, which was bolstered by 1,000 vessels captured from the Dutch, and regular amendments, over the next two centuries, of the Navigation Acts, which were finally expunged from British law in 1849.

So lay the future of European mercantilism in the play of Great Power politics among Spain, France, Britain, and the Dutch Republic over the

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period from 1648 to 1815. Each state consolidated its political position domestically, while exploring the possibilities for expanding trade and treasure from abroad. The early examples of treasure streaming into Spanish Seville on the annual fleets from Vera Cruz and Cartagena, and of trade profits from pepper sold at persistently high prices by the Portuguese state monopoly, excited the imitative efforts of all three follower countries. Already the Dutch and English East India companies were proving successful in undercutting the existing spice and silk trades, which had been dominated in earlier periods first by Italians and later by Portuguese merchants. Under Jean-Baptiste Colbert the French East India Company attempted to mimic Anglo-Dutch success, with state support and control. The overall context of the economic policies of all three mercantile powers was the endeavour first to define, and then to maintain, the geographical limits of the emerging nation-state. This was the historical setting within which a series of natural experiments were conducted by the three powers to determine, essentially, how to wage and win large-scale wars. In the process major innovations in war finance were necessary, while coming up against the limits of what modern macroeconomists call the 'trilemma of open economies'. Economics students today learn, along with newly appointed finance ministers, that no country can long sustain all three desirable economic policies of independent monetary policy, fixed exchange rates, and open capital markets.

France, as the largest kingdom in Europe after 1648, naturally attempted to expand its trade overseas while protecting its domestic industries from competition by Italian and Dutch city-states. While attempting at times to import capital, especially under the guidance of John Law during the Regency period of 1715, and then to keep fixed exchange rates with the rest of Europe after 1726, the absolutist regime of France was persistent in pursuing independent monetary policies. By the end of the Thirty Years War the Dutch realised that they needed to encourage foreign capital, but could not hope to maintain independent monetary policies among their several fiscally separate provinces, so the Dutch Republic's focus was always on maintaining fixed exchange rates, basically to sustain its commodity trade with the rest of Europe. Britain variously flirted with independent monetary policies within Scotland, Ireland, or northern England, and occasionally was forced into floating exchange rates as a result, but always persisted in maintaining access to foreign capital.

From this broader historical perspective, then, the ultimate success of Britain in learning how both to finance and to wage war effectively – starting with its attempts in the seventeenth century and culminating with its success by 1815 – is a natural experiment already run. Within

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it, economists and political scientists may find evidence to test their assumptions about the nature of the essential determinants for creating credible commitment mechanisms. The historical context in which the British state focused on building a fiscal–military regime to compete with the other European mercantile powers, but paid scant attention to public education, health, or employment and retirement security, moreover, makes this natural experiment neater for analytical purposes. Just as accounts of the Industrial Revolution have furnished models of industrialisation and prescriptions for developmental economists (Rostow 1960; Gerschenkron 1962), North and Weingast's thesis has furnished similar inspiration to those who aspire to pair financial capitalism with economic growth (Levine 2001; Graff 2003). Consequently, the thesis has proven influential in an extraordinary range of disciplines, from financial and economic history to sociology, development studies, and legal and political theory, and in modern public policy circles. It is possible to see in their historical model support for the Washington Consensus.

The essays collected in this volume grew out of a conference entitled 'Questioning Credible Commitment: Re-thinking the Glorious Revolution and the Rise of Financial Capitalism', held at the Centre for Financial History at Newnham College of the University of Cambridge in March 2010. They by no means represent the first attempts by historians to critique the North and Weingast formulation on empirical grounds. Yet, as editors, we feel they represent fairly the principal lines of approach, with one caveat. If fiscal institutions were neither based on the Dutch model nor formed as a result of regime change in 1688, similar claims may be made for monetary institutions, which were noticeably absent from North and Weingast's discussion, despite their centrality to debates about time-inconsistent policy making (Persson and Tabellini 1994). Mayhew argues in a recent edited collection (Munro 2012) that the Williamite Recoinage came not as a result of predation by Stuart monarchs after the Restoration, but rather from the Crown's insistence on maintaining a 'high value silver coinage of unchanging worth, despite major shifts in the international value of precious metals'. Over the course of the seventeenth and eighteenth centuries, resistance to adjusting the mint price of silver to align it with the market price was so entrenched that clipping became commonplace. Quinn's now-classic study of the operation of the bills–bullion arbitrage market demonstrates vividly the opportunities and costs associated with this policy (Quinn 1996). Subsequently, Isaac Newton's fixed peg of one gold guinea to twenty-one silver shillings in 1717 had the not-unsurprising consequence that bimetallism soon became a dead letter, and a de facto gold standard emerged over the eighteenth century (Redish 1990). Whether these developments had anything to do with

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