On the Economic Consequences of Warfare in
Early Modern Northwest Europe:
Four Conceptual and Empirical Contributions

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General Introduction

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1.1 Motivation

One of the central questions of economic history pertains to the origins and distribution of economic growth. Generations of economic historians have sought to explain the surge of economic growth and individual economic prosperity associated with the Industrial Revolution, the long periods of growth, stagnation and decline that went before it, and the vast disparities in economic fortunes across time, space and people. It is widely recognized that the structure of the economy—that is, the arrangements of production and of the exchange of commodities and production factors (North 1981)—is of fundamental importance to explain these patterns. The structure of the economy is not determined in a vacuum but embedded in social structure. As such, economic history is closely connected to a wider tradition in the social sciences that seeks to explain patterns of social structure—or 'political order'—as well as economic structure. Together, the structure of economy and society shape individual prosperity, personal security, freedom of action and, importantly, how inequalities therein emerge and recursively affect the organization of economy and society.

This dissertation analyzes the interaction between economic structure and political order by examining the economic consequences of warfare in early modern Northwest Europe. Although early modern warfare was highly disruptive and destructive, there are several ways in which these negative effects could have led to positive changes in the structure of the economy or political order. This introductory first Chapter outlines the capacity of warfare to produce such changes. In doing so, this Chapter serves to discuss the dissertation's main theme, it's aim and to outline its contributions. However, the proposition that studying warfare is helpful in understanding general patterns of economic growth and political order might be somewhat counterintuitive. Hence, I will use the first Section of this introductory Chapter for a short digression of what is arguably the currently dominant explanation of patterns of growth and political order and, subsequently, connect this to early modern warfare and its capacity to affect economic structure and political order.

The dominant explanation of the origins of economic growth posits that economic outcomes are driven by economic institutions.[1] Institutions are the rules, both formal and informal, that govern transactions among people and economic institutions are those rules that pertain to economic exchange (North 1990; Acemoglu and Johnson

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[1] There are several competing theories on the origins of economic growth. However, a full discussion of that literature is beyond the scope of this Chapter. For recent surveys of the literature, with a focus on institutional explanations for growth, see Alesina and Guiliano (2015) and Spolaore and Wacziarg (2013)—and for furter discussion, see Glaeser, La Porta, Lopez-de-Silanes and Shleifer (2004); Rodrik, Subramanian and Trebbi (2004) and Acemoglu, Johnson and Robinson (2005a).

2005; Greif 2006). Any economic exchange entails risks to the trading parties of being cheated or robbed (Hirshleifer 1988). Consequently, rules that protect individuals against such risks—by securing 'property rights'—facilitate more transactions, thereby stimulating investments, trade, and specialization and thus economic growth (North 1981; Greif, Milgrom and Weingast 1994; Acemoglu et al. 2001; 2005b).

If appropriate economic institutions explain economic growth, then the subsequent problem is to explain where these economic institutions come from—how they are designed and enforced—and why, if some economic institutions are better than others, they would differ across time and space. One explanation is that each society has a political elite that opportunistically designs and enforces economic institutions to its own benefit. The relative power of the elite then determines how beneficial or extractive, and thus how economically stimulating or harmful, economic institutions will be (North and Weingast 1989; Acemoglu and Robinson 2001; 2006a; 2008). After all, those with the power to protect property rights are usually also as capable of exploiting their powers to violate property rights (Greif 2006). Whereas this explanation views the political sphere as driving economic outcomes, others have argued that economic outcomes ultimately determine political outcomes (Glaeser et al. 2004; Abramson and Boix 2015). This alternative view stresses that economic exchange always exists, also in absence of enforced rules (Greif 1989; 1993; Milgrom, North and Weingast 1990) and that outcomes of such exchange subsequently determine the structure of political power (Boix 2015).[2] Irrespective of their differences, both approaches converge on a concept of 'political order' determined by the distribution of violence in society and the organization of extraction and protection (North 1981; Boix 2015).

In a recent contribution, North, Wallis and Weingast (2009) argue that the interaction between economy and political order can only be understood through the lens of violence. In their framework, political order is shaped by those with the capacity to organize large-scale violence. Economic structure, too, is an essential ingredient in their framework for it is a key driver of violence capacity and of stability within the political order. And, importantly, violence determines the security of economic transactions and property, whether it is used opportunistically to cheat or rob, or used to enforce economic institutions (North et al. 2009; see also Greif et al. 1994; Bates, Greif and Singh 2002). The framework of North et al. (2009) comprises a dynamic co-development of political order and economic organization rather than a framework that is ultimately based on one-directional causation. This results in an application

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[2] Most of the empirical studies investigating the direction of causality, concentrate on the question whether democratization causes economic growth or the other way around. See for evidence of the first Acemoglu, Johnson, Robinson and Yared (2008; 2009), and for the reverse Przeworski, Alvarez, Cheibub and Limongi (2000) and Boix (2015).

of institutions as endogenously determined, which is arguably more convincing than competing theories wherein institutions derive from intentional design or deterministically follow from existing geographical or biological differences across societies—for a similar, although more micro-level oriented approach, see Greif (2006) and Greif and Laitin (2004).

According to North et al. (2009), there are three archetypal constellations of violence, political order and economic structure that are stable. The first is the 'foraging order' that was the default form of social structure before the Neolithic Revolution and which, essentially, entails very little structure. The second order is the 'natural state'—or 'limited access order'—wherein the capacity for large-scale organized violence is concentrated in the hands of a small number of individuals who can use that capacity opportunistically. The natural state solves the problem of endemic violence by forming a political coalition of such violence specialists instead. By joining the coalition, violence specialists commit to respect each others' economic privileges and to collectively protect these against outsiders. Accordingly, the natural state creates incentives for violence specialists to limit their use of violence against other violence specialists, because the collective rents created by the coalition are more valuable than what could be gained by a violence specialist that exploits his violence capacity individually. And given that only violence specialists have the capacity to exert substantial violence, the coalition thus decreases the use of violence in society at large. However, to maintain this incentive, the coalition erects strict barriers in the political and economic sphere in order to maximize the economic rents extracted from the rest of society (North et al. 2009).

The political order in natural states is fundamentally unstable. Violence specialists can opportunistically exploit their capacity for violence and only manage to reduce violence collectively by way of an armed peace within the elite coalition. Small shifts in the distribution of violence capacity or in the relative contribution of individual members thus require readjustments within the coalition of elites to maintain a balance between the individual power of violence specialists and their rewards from maintaining the coalition (North et al. 2009: 20–21). Although natural states vary in the stability of the dominant coalition by virtue of the organizational structure surrounding the state—North et al. (2009: 6–18) stress the importance of impersonal, perpetual organizations that, although dominated by the elite coalition, standardize many interactions between violence specialists—true progress in stability only comes with the third order. This third order—the 'open access order'—emerged only gradually in history, during the nineteenth century in England, and still exists only in a minority of predominantly Western countries. Open access orders are characterized by the

constant flux of economic competition and a democratic state with a monopoly over organized, large-scale violence. The state subsequently guarantees open competition in the economic and political sphere. Hence, representation and consent of the ruled form the basis of political order in open access orders, rather than the distribution of violence capacity concentrated in individuals. Following this framework of North et al. (2009), to understand the origins of economic growth and stable political order is to understand why societies move from limited access to open access orders. And for this, we have to understand developments in early modern Northwest Europe (North et al. 2009: 240–50).

Although it is certainly not the case that all important developments in economic growth or political order happened in Northwest Europe, it is clear that these developments went furthest there, eventually resulting in modern economic growth (Broadberry and Gupta 2006; Allen 2009; Allen, Bassino, Ma, Moll-Murata and Van Zanden 2011; Bolt and Van Zanden 2014; Broadberry, Guan and Li 2018). In addition, Northwest Europe developed a practice of political order based on consent and representation of clearly-defined groups within society (Stasavage 2016). As such, it developed more complex forms of natural states with a strong organizational core and eventually developed open access orders (North et al. 2009). Although several scholars would argue that the roots of these developments are to be found in the Middle Ages (Epstein 2000; Van Zanden 2009; Campbell 2016), it took until the eighteenth or nineteenth century before the Industrial Revolution and the beginnings of broad-based democracy were established. Furthermore, these developments were concentrated in the urbanized core of Western Europe, running from Northern Italy through the Rhine valley into the Low Countries and Southeast England (Tilly 1992; Stasavage 2011). Although the frontier of these developments shifted from Northern Italy to the North during the early modern period, it was in this central core that urban communities developed their own states based on a degree of consent and representation (Blockmans 1989; Tilly 1992; Stasavage 2011; Prak 2018) and produced economic growth (DeLong and Shleifer 1993; Mokyr 2002; Acemoglu et al. 2005b; Cantoni and Yuchtman 2014).

Spanning the entire period from the Middle Ages through to the early modern period, Western Europe, and in particular the numerous small city-states in its urban core, experienced almost continuous and intense warfare (Tilly 1992; O'Brien 2011; Stasavage 2011; Dincecco and Onorato 2016).[3] The pattern of warfare in early modern

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[3] For example, Chapter 2 of this dissertation shows that there were only 18 years without military activity in the Low Countries throughout the entire seventeenth century. Furthermore, several of those 18 years occurred in periods wherein the Dutch Republic and the Spanish Netherlands were actually at war—with each other or with other political entities—but without fighting occurring in the Low Countries itself. Whereas the Low Countries formed the main battleground of the seventeenth century,

Europe is argued to have been quite specific as it was based on competition among a plethora of small political entities with relatively weak state structures. Although warfare arguably existed in all world regions and occurred at high frequency there too (Stasavage 2016; Dincecco and Onorato 2017), the political fragmentation that existed in Europe after the fall of the Roman Empire is said to have resulted in a more intense type of military conflict between political entities (Rosenthal and Wong 2011; Hoffman 2015; Dincecco and Onorato 2017). This was the case because military competition gave an edge to those political entities that raised revenue efficiently, either through taxation or borrowing. In this way, military competition stimulated European political entities to develop efficient fiscal systems and state structures (Tilly 1992; Ertman 1997). Importantly, military pressure often forced them to grant consent and representation to those being taxed and, particularly, to potential creditors (North and Weingast 1989; O'Brien 2011; Stasavage 2011).

The European pattern of warfare stimulated the development of political entities with capable state structures that, in turn, could have stimulated economic growth (Besley and Persson 2009; Dincecco and Prado 2012). However, it often corrupted state structures in favor of creditors as well (Stasavage 2011). In addition, it could drive small but economically efficient political entities out of competition in favor of larger ones (Tilly 1992). Of course, early modern warfare had devastating short-term effects on economic activity and sometimes resulted in high death tolls (Hale 1985; Alfani 2013a). Even these destructive effects, in theory, could turn out positive if they forced people to seek refuge in cities thereby strengthening Europe's urban core (Rosenthal and Wong 2011; Dincecco and Onorato 2016; 2017). Warfare thus was at least intimately connected to Northwest Europe's rise to modern economic growth and political development. It shaped political order by favoring some political entities over others, and it affected the structure of the economy through destruction and subsequent realignments. On a more abstract level, warfare is intimately related to political order in the sense that it is a demonstration of the control of violence and the power of extraction and protection of political and economic interests. It is for these reasons that an examination of the economic consequences of warfare is helpful for understanding the interaction between economic structure and political order.

The central theme of this dissertation is the capacity of warfare to explain variation in economic structure across regions and over time within Northwest Europe. However, this topic is evidently of such magnitude that a comprehensive and complete analysis of this theme is beyond the scope of a single dissertation. Accordingly, the

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it shared that role with Northern Italy during the sixteenth century (Tilly 1992; Dincecco and Onorato 2017).

aim of this dissertation is more modest. It provides four focused contributions to the literature, wherein the central theme is assaulted from different directions. In doing so, I will concentrate on the effects of warfare on the structure of the economy in the context of early modern Northwest Europe. In accordance with the above discussion, I will take into account throughout that these effects are shaped by the interaction between warfare, political order and existing variations in economic structure.

Before moving on to these contributions, the next Section provides a more detailed overview of the existing literature. It provides a focused discussion that highlights in particular the two ways in which early modern warfare could, according to the literature, have had an effect on long-term economic growth; through changes in the structure of the economy, or through war-induced changes in political order and subsequent economic effects. This is followed in Section 1.3 by an outline of the possibilities to contribute to the existing literature. Furthermore, Section 1.3 details the approach taken in the rest of this dissertation and summarizes the four main contributions of the later Chapters of this dissertation. The final Section of this Chapter provides a general conclusion of this dissertation.

1.2   State of the debate

The omnipresence of warfare in early modern Western Europe is, perhaps unsurprisingly, paralleled by a vast literature on the topic. This Section provides a stylized overview of the literature with a clear focus on those parts of the literature that are relevant for the topics treated in this dissertation. That is, it will focus on those strands of literature that provide insights in the potential of warfare to explain variations and change in economic structure over time and across space. To indeed explain variation, I will argue that the consequences of warfare need to be systematic and long-lasting. And, for warfare to explain variation and change in economic structure, either the pattern of warfare needs to vary or the consequences of warfare have to depend on existing differences in economic structure or political order. In doing so, this Section is skewed towards literature on the Northwestern part of early modern Europe, and particularly on the Low Countries—which form the setting of the empirical parts of this dissertation. It prioritizes systematic empirical literature and favors recent advances over a discussion of the entire historiography. The first part of the Section concentrates on the direct relation between warfare and economic structure, while the second part brings political order into the equation as well.

To start with, it seems evident that a discussion of the economic consequences of warfare has to come to terms with the enormity of the destruction and human misery

brought about by warfare. There exists a large literature emphasizing precisely that effect of warfare, as one of the biblical 'horsemen of the apocalypse' (Cunningham and Grell 2000; Alfani 2013a). A picture of massive population losses and economic collapse emerges from the literature on the most spectacular episodes of war, such as the Hundred Years' War (1337 to 1453) between France and England (Duby 1974; Bois 1984; Campbell 2016) or the Thirty Years' War (1618 to 1648) in Germany (Rabb 1962; Parker 1984; Wilson 2009). Such negative depictions are almost as often directly qualified by precise empirical work, often by the same scholars. For instance, while Campbell (2016) stresses the collapse in trade during the Hundred Year's War in his recent synthesis of the late medieval crisis (Campbell 2016: 12), he also emphasizes the benign effects of other episodes of war on the access of several European political entities to trade with the Levant (Campbell 2016: 267–76). A closer inspection of the effects of the Thirty Year's War, likewise, has resulted in regionally varying accounts with some literature estimating population losses of thirty to forty per cent in several German regions while hardly any population changes are found in the nearby and equally war-affected region of Liège (Gutmann 1978; 1980).

Regarding the Low Countries, much of the literature is oriented towards the Eighty Years' War (1568 to 1648) during which the Dutch Republic formed and gained independence from Spain.[4] The effects of that war differed vastly over time and space. The first decades of the conflict, when the Protestant rebels tried to establish a solid foothold in the Low Countries, were notoriously violent. Fighting occurred throughout the Low Countries and led to large population losses in wealthy and densely populated regions such as Flanders and Brabant (Parker 1975) and heavy repression by the rebels as well as Spanish forces (Van Nierop 1999). During later periods, on the other hand, treatment of civilians was often remarkably restrained (Parker 1972: 18; Israel 1982: 97–101; De Cauwer 2008). These effects differed across regions as well. Regions that fell outside direct control of either the emerging Dutch Republic or Spain bore the brunt of taxation, repression and violence (Adriaenssen 2007). Furthermore, such differences in the actual execution of war have been shown to lead to large differences in economic outcomes. For instance, the population decline in Flanders and Brabant alluded to before coincided with rapid population increases in several cities in Holland, Amsterdam in particular (Kuijpers 2005). Similarly, the fall of Antwerp in the 1580s and the subsequent naval blockade of the city led to an exodus of wealthy merchants. Many of these merchants ended up in Amsterdam which became the new trade center of the North Sea and, eventually, the central node in global trade during the seventeenth century (Gelderblom 2000; 2013; Lesger 2006).

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[4] See 't Hart (2014) for a recent synthesis of the literature on the Eighty Years' War.

To a large extent, differences in the economic effects of warfare can be tied to patterns of warfare itself. If we define war as violent conflict between political entities or groups within them, it follows that the exact nature of war could differ quite substantially. In particular, wars differed in their duration, the size of armies involved, their geographical spread, the intensity of actual fighting between armed groups, and the use of violence towards non-combatants. These differences could be very local, depending on such things as the locations of garrison towns (Vermeesch 2009) or whether a band of soldiers raising 'contributions'—a euphemism for extraordinary taxes extorted under the threat of violence—were regularly paid and not drunk (Parker 1972; Lynn 1993). Although the issue of local coping capacity has received little systematic research, there is some evidence that differences in the economic outcomes of warfare derive from variation in the capacity of localities or regions to deal with the consequences of warfare rather than from differences in the actual form of warfare (Gutmann 1978; Soens 2018).

Testing whether variation in the economic consequences of warfare are due to variation in the capacity of different regions to cope with warfare is quite difficult. For instance, Adriaenssen (2007) finds that almost all villages and towns in Brabant suffered heavily during the Eighty Year's War, but that a small number of villages—Tilburg in particular—managed much better. However, how to exclude the possibility that Tilburg had simply been more lucky during the war? In some cases, a clear rationale can be provided. Caferro (2008), for instance, argues that Italian towns which had armament industries benefited from the Italian Wars as demand for their products grew rapidly. In a careful study on the consequences of the Italian Wars in Lombardy, Di Tullio (2016; 2018) shows that communal management and distribution of the burden of war determined the distribution of economic consequences within and between localities. Even in those studies, however, it remains difficult to establish whether the burden of war faced by different communities or regions was similar.

That war had economic consequences is clear. Whether and why these consequences varied is harder to establish, because it is difficult to disentangle the role of variations in the experience of warfare itself from differences in the capacity to recover. Accordingly, it is quite difficult to generalize or aggregate findings from particular wars or places. Importantly, if the outcomes of warfare are not systematic, with similar impacts of war randomly leading to vastly different outcomes, then there is little to say about the economic consequences of warfare in a general sense. This, however, is quite unlikely given the discussion above—for instance, the close association of variation in economic outcomes with variation in local economic opportunities (Gutmann 1978) or local communal management (Di Tullio 2016). In addition, even something like the

discipline of soldiers, or the distribution and intensity of taxation are hardly random but driven by the organization of the military and the state instead (Parker 1972; Tilly 1992; O'Brien 2011; Brandon 2015). The same can be said for more indirect economic consequences of warfare such as patterns of migration and trade. For instance, the capacity of one city to benefit greatly from the temporary breakdown of trade in another region is arguably related to that city's economic prospects—as with Amsterdam's rise to prominence (Gelderblom 2013) discussed above. Put differently, these are examples wherein the consequences of warfare vary across places because they depend on existing economic structure, which varies across places—and on political order, discussed below.

Another important question is whether warfare's economic consequences are enduring. For instance, will destroyed physical capital not simply be replaced? And are people fleeing from warfare not simply returning at war's end? If that is the case, then it might not even be relevant to find out how places are affected or whether some places are more affected by warfare than others. Importantly, there is ample theoretical and empirical literature which suggests that economies normally rebound to their previous structure following a shock—whether that shock is a natural disaster, an economic crisis or warfare—and that variation in outcomes due to differences in impact of the shock quickly disappear. For example, this was the case in two modern cases of extremely destructive episodes of warfare: post-World War II Japan and post-war Vietnam (Davis and Weinstein 2002; Miguel and Rolland 2011). Furthermore, Khan (2015) shows that the heavy economic damages following the American Civil War (1861 to 1865) had no lasting effects because war-induced misallocations were reversed as soon as the war ended. Juhasz (2018), in contrast, finds that temporary misallocations due to trade blockades during the Napoleonic Wars (1803 to 1815) did have persistent effects. She finds that the blockade of trade with the British Empire forced local French communities to industrialize. Subsequently, those regions where the blockade was more effectively imposed experienced positive economic consequences compared to regions where British commodities could be smuggled in. Similarly, O'Rourke (2006) shows that the economic disturbances caused by the Napoleonic Wars had substantial effects on global trade with long-lasting consequences. Clearly, there is quite some some evidence to suggest that warfare had persistent economic consequences in some cases but not others.[5]

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[5] Blattman and Miguel (2010) provide a survey of the literature on modern-day warfare, and civil wars in particular. There is quite some literature in economics specifically looking at effects of warfare on growth using some source of exogenous variation. Although many studies establish some persistent effects, the magnitude and type of effects remain difficult to establish (Abadie and Gardeazabal 2003; Bosker, Brakman, Garretsen and Schramm 2007; 2008; Glick and Taylor 2010). In a recent study, Feigenbaum, Lee and Mezzanotti (2018) provide similar results for the small but, along some

Systematic empirical analyses of the economic consequences of warfare in early modern Europe are relatively scarce. However, Voigtländer and Voth (2013) theoretically deal with some of the issues discussed here in an important recent contribution. Using the stark setting of a Malthusian model with two sectors—a productive urban sector and a necessary rural sector—with labor as the only flexible production factor, they suggest that the economic consequences of warfare in Europe were persistent because they resulted in more war. According to Voigtländer and Voth (2013), the main effect of warfare was that it spread epidemic disease and thereby killed substantial numbers of people. Furthermore, it predominantly killed people in Europe's unhealthy cities. In addition, they argue that capital was unimportant at the time and easily replaced. The consequences of war in the model of Voigtländer and Voth (2013) are as follows. First, it would result in high mortality pushing up real wages. Second, high real wages would increase the demand in urban products stimulating people to work in the cities—where these products would be produced and wages would be higher. At the same time, urbanization also increases mortality rates as a consequence of the unhealthiness of the cities. Third, high real wages would allow more effective taxation, the proceeds of which would be spend on more warfare. This cycle of warfare, urbanization, high wages, and more warfare would have set Europe on a path to economic growth (Voigtländer and Voth 2013).

The approach of Voigtländer and Voth (2013) to persistence is quite striking. In particular, their Malthusian model is in itself fundamentally incompatible with the idea of persistent shocks. Malthusian models posit an economy with a fixed resource base to which a flexible population will always adjust. Hence, if warfare causes mortality, this would simply lead to temporarily high wages, followed by population growth that would eventually reproduce the initial equilibrium. Instead, Voigtländer and Voth (2013) show that war-induced changes in economic structure can break this Malthusian cycle. Their model has less to say about the variation in economic development, however. In principle, their model posits the same outcome for all places and periods. To explain outcomes between world regions, they argue that European cities were more unhealthy than cities in China; to explain the onset of war-driven growth, they use the Black Death as an initial shock—the Black Death killed so many people that it set in motion the cycle of high wage, urbanization and warfare—and the severity of the Black Death to explain variation in economic growth within Europe (Voigtländer and Voth 2013).

Using a similar approach to persistence but a more elaborate approach to variation

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economic dimensions, lasting negative consequences of war-induced destruction of rural capital during the American Civil War.

in patterns of warfare, Dincecco and Onorato (2016; 2017) connect warfare in early modern Europe to urbanization and subsequent economic growth too. They argue that people in early modern Europe were much better protected against warfare in cities than in the countryside, with the consequence that warfare induced rural-to-urban migration—in this, they follow Rosenthal and Wong (2011) who suggest that, in combination with endemic warfare, Europe's unsafe countryside explains its fast urbanization and subsequent economic growth relative to China. In addition, Dincecco and Onorato (2017) argue that episodes of warfare regularly lasted several years and that it was not always evident for migrants to return to their villages after the war. Furthermore, urban environments provided several productive benefits to migrants—in the form of economic privileges to citizens, better access to technology, human capital formation, and general agglomeration effects—providing migrants with yet another reason to stay in the city (Dincecco and Onorato 2017). Dincecco and Onorato (2016) provide empirical evidence associating the spatial pattern of major military conflicts in late medieval and early modern Europe with the growth of European cities. Somewhat more tentatively, they show that European regions with a histories of more intense warfare are on average richer than regions that experienced less warfare (Dincecco and Onorato 2017).

The contributions by Voigtländer and Voth (2013) and Dincecco and Onorato (2016; 2017) posit clear rationales for the capacity of warfare to affect the structure of economies. In particular, in their view warfare moves people from the countryside into cities. Besides a shift of production factors, both Voigtländer and Voth (2013) and Dincecco and Onorato (2016; 2017) imply that this changes the production process of the economy because urban production differs from rural production—urban production is more productive and produces different commodities. For Voigtländer and Voth (2013), the driving force is that warfare kills people and, in particular, people in cities—followed by subsequent rural-to-urban migration. To some extent, this is difficult to reconcile with previously discussed literature stressing the negative consequences of war-induced mortality (Rabb 1962; Adriaenssen 2007; Alfani 2013a). In particular, Campbell (2016) argues that mortality shocks often resulted in a contraction of trade, which would arguably hurt economic activity in cities much more than in the countryside. Furthermore, the direct mortality consequences of warfare are usually found in the countryside rather than the city (Parker 1975; Gutmann 1978). The connection between war-induced urbanization and economic growth posited by Dincecco and Onorato (2016; 2017) begs similar questions. The productive benefits they attach to economic activity in cities might not be accessible to migrants, for instance (Ogilvie 2006). More importantly, these productive benefits might be undermined by war itself

because of trade disruptions, or destruction of complementary economic activity in the countryside (Rosenthal and Wong 2011).

This assessment of the direct economic consequences of war-induced migration underscores the findings discussed before. First, it is clear that warfare could have substantial economic consequences. It could trigger realignments in the structure of the economy by killing people, inducing migration, obstructing trade flows and destroying capital. Second, identifying whether warfare-induced realignments in the structure of the economy are systematic is much harder. Most existing literature that finds differences in the local or regional economic consequences of warfare does not correct for differences in the initial impact of warfare itself—as it is obviously difficult to do so—even though variations in wars, such as intensity and duration, can be quite local. This makes it difficult to establish whether particular episodes of war or particular regions were simply unlucky in their experience with war. Or, alternatively, that the different economic consequences are due to underlying structural differences between regions that shape the impact and recovery of warfare. Third, it is not obvious that even large, short-term effects of warfare entail lasting consequences because temporary misallocations in the economy are often quickly reversed. Accordingly, existing explanations in the variation in the economic consequences of warfare consistently point to underlying conditions that can explain the relative resilience of some places or variation in the patterns of warfare themselves. But does warfare explain variation in these underlying conditions?

1.2.1 Warfare, political order and economic outcomes

In the first Section of this Chapter, I introduced political order as the distribution of violence and the organization of extraction and protection. In addition, I outlined the important developments in political order that occurred during the medieval and early modern period in Northwest Europe. There, communities and political entities developed formal practices of political representation and influence on decision-making as alternative routes to political power besides the exploitation of violence capacity. This stimulated growth-enhancing economic institutions and gradually paved the way for open access orders (North and Weingast 1989; Acemoglu and Robinson 2000; North et al. 2009; Stasavage 2011; Prak 2018). Can positive changes in political order explain the relative resilience of some places or variation in the patterns of warfare? And if so, did warfare influence these changes in political order? To address these questions, I will first discuss existing evidence on war-induced changes to political order and the varying ways in which these changes affected economic outcomes.

Political order is, of course, closely associated with the state. That warfare could

seriously affect political order in early modern Europe has a lot to do with the relatively ambiguous structure of power there. Following Tilly (1992), the state is often defined as a coercion-wielding organization with clear priority over other organizations, that holds a quasi-monopoly of violence within a substantial territory. All three main aspects of this definition—organizational structure, monopoly of violence, and a defined territory—reveal the relative weakness of early modern European states. By way of illustration, consider the Low Countries during the first half of the sixteenth century. All the principalities in the Low Countries were formally under control of Charles V from 1506 onward, either as part of his inheritance as duke of Burgundy or, as in the case of the Bishoprics of Utrecht and Liège, under clear Burgundian tutelage (Blockmans 1999). Besides, Charles V would inherit the Spanish as well as the Imperial crown, making him the most powerful monarch of his time in Europe. Still, it took him until 1543 to temper one unruly prince within the Burgundian Low Countries—the duke of Guelders. While fighting his final war against the duke of Guelders in 1542–43, Charles used his title as duke of Brabant—one of the other principalities of the Low Countries (Tracy 1990; see also Chapter 3)—suggesting that Charles V's title as overlord of the Low Countries granted him little useful additional power or organizational capacity—see Blockmans (1999) for a general argument in that direction. In addition, the conflict between Charles V and the duke of Guelders involved not only the Duchy of Guelders but much of the Low Countries. The duke of Guelders used his influence to stir and support military rebellion throughout the Low Countries and launched raids into other principalities (Bonney 1991: 112–13). Accordingly, Charles V lost de facto control over much of the Burgundian Low Countries at several moments during the first four decades of the sixteenth century (Struick 1960; Tracy 1990). In fact, the struggles of Charles V to consolidate his power in the Low Countries are exemplary for the lack of state capacity of early modern European states (Bonney 1991; 1995; 1999; Rosenthal and Wong 2011; Gennaioli and Voth 2015; Hoffman 2015). A monopoly of violence over clearly defined territories was hardly attainable for early modern European states.

Early modern European states were relatively weak, because they lacked a stable organizational structure that could reliably organize the main functions of the state (North et al. 2009; Hoffman 2015). It is not difficult to envisage that warfare potentially affected such weak states given that warfare produced substantial direct economic consequences and that assembling, organizing, feeding and commanding armies to fight wars required quite some organization (Parker 1972; Brandon 2015). The weakness of the state also highlights the other component of political order, specifically the distribution of violence capacity. Given the lack of a clear concentration of violence capacity in the organization constituting the state, the arrangement of taxation and of

protection were all contentious. Subsequently, they were to a large extent determined by conflict among other specialists in violence, such as nobles, wealthy urban citizens or both (Blockmans 1989; Tilly 1985; 1992; 't Hart 1993; North et al. 2009). Besides imposing stringent demands on the organizational capacity of the state and other violence specialists, warfare could also directly affect the distribution of violence capacity. In addition, warfare produced such effects within political entities but also between them—changing the relative organizational capacity of competing states and the distribution of violence between them.

Starting with the effect of warfare on internal political order, warfare forced political entities to organize their violence capacity and to generate finances to pay for war—to hire soldiers, pay for weapons and provisions (Parker 1972). Early modern states spent the large majority of their revenues on warfare (Tilly 1992; 't Hart 1993; Hoffman 2015). This had several direct consequences on internal political order. On the one hand, it has been shown that warfare stimulated development of the fiscal apparatus of states, thereby strengthening the organizational structure of the state itself (Bonney 1995; 1999; Dincecco 2009; O'Brien 2011) and changing patterns of extraction—in particular, states increasingly relied on taxation rather than on the personal resources of their elites ('t Hart 1995; Bonney 1999).

It is regularly argued that the reliance of elites on taxation-generating state structures was in several cases accompanied by elites granting representation to those being taxed (North and Weingast 1989; Tilly 1992; 't Hart 1995; Stasavage 2011). That is, citizens were often in the position to demand concessions, in the form of access to political decision making, in return for paying taxes—see especially Blockmans (1989; 1995) and Stasavage (2010; 2011), and for the relation with parliaments Van Zanden, Bosker and Buringh (2012). Accordingly, the financial demands imposed on states by warfare forced states to improve their organizational strength, whether to tax more or to clearly define political decision-making processes. Note, however, that the conflict over taxation could as easily stimulate autocratic state formation, with those in control of the state enforcing ever higher and more efficient forms of taxation without consent of those being taxed (O'Brien 2011). For instance, Stasavage (2011) provides evidence that the demands of financing wars allowed merchant-creditors to subvert state structures in city-states towards oligarchy. Which groups in society managed to assume control over taxation and the state of course depended on distributions of violence capacity and vice versa. At the same time, warfare could directly affect the capacity of the state as well as the distribution of violence—for instance through redistribution of wealth—although evidence for the early modern period is scarce (Scheve and Stasavage 2010; 2012; Piketty 2014; Scheidel 2017). Most of the evidence collected by these authors—and

certainly the most convincing evidence—stems from later periods, the nineteenth and twentieth centuries in particular. In addition, these authors stress massive destruction, and mass mobilization during periods of democracy as the main drivers behind war-induced equalization. These factors were far less relevant during the early modern period.

Warfare could affect political order between early modern European political entities by introducing a form of competition. Those political entities that were better at organizing and financing their military apparatus are thought to have gradually driven the inefficient ones out of competition (Tilly 1992; Gennaioli and Voth 2015). This process of military competition was further stimulated by advances in military technologies—often alluded to as 'the military revolution' (Bean 1973; Tilly 1992; Downing 1992; Parker 1996). In his influential *Coercion, Capital and European States*, Tilly (1992) suggested that European political entities followed different paths to increase their military capacity. In this view, some, predominantly city-states, relied on their economic prowess and access to financial credit in particular. Larger political entities used their access to natural resources and to manpower, with the result that these followed a more autocratic path towards military capacity. The most effective option according to Tilly (1992), however, was to use both channels as happened in the Dutch Republic and, particularly, in England.

According to some, military competition particularly favored large political entities with the capacity to accumulate violence capacity in strong, centralized state structures (Tilly 1992; Gennaioli and Voth 2015). In contrast, Abramson (2017) finds that small city-states survived the onslaught of military competition during the early modern period surprisingly well—in line with Spruyt (2017) who argues that there was a variety of political orders efficient enough to survive early modern Europe's competition. Evidently, what type of political orders were systematically favored by war is not clear (O'Brien 2011). More important for this study, whether the political entities favored by military competition actually also had political orders conducive for economic activity is even more difficult to establish.[6]

Using the effects of warfare on the city as an example, the challenges in establishing the relationship between warfare, political order and subsequent economic consequences become apparent. To start with, there is a long-standing idea that the

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[6] Theories focusing on the bellicose origins of capable European—or 'Western' in general—states remain quite popular among economists and development scholars seeking to explain state failure in other parts of the world (Herbst 2000; Bates 2001; Besley and Persson 2009; 2010; Dincecco and Prado 2012; Besley and Reynal-Querol 2014; Dincecco and Onorato 2016). However, state failure was an important part of the story in early modern Europe too and not infrequently driven by warfare (Spruyt 2017).

most beneficial economic institutions in early modern Europe were to be found in (semi-)autonomous cities. In its stylized form, this argument is perhaps most clearly found in DeLong and Shleifer (1993). The idea is based on a conception of the city in early modern Western Europe as semi-autonomous centers of economic exchange, wherein political and economic power were shared relatively equally. The city, in this view, is contrasted to political entities with autocratic, centralized states—'territorial states'—where rulers opportunistically preyed on economic activity and thus hindered economic growth (DeLong and Shleifer 1993).

Dincecco and Onorato (2017) in particular follow this benign urban-model quite closely in their explanation for city-driven growth. According to them, people were more productive in urban environments because they had better access to human capital and technology. Furthermore, they argue that citizens of urban centers were better protected from exploitation and expropriation by elites and often even enjoyed a certain degree of access to political decision making. That some cities were indeed engines of economic activity in early modern Europe is quite clear, but it is also clear that their economic prowess ultimately depended on their economic institutions (Bosker, Buringh and Van Zanden 2013; Cantoni and Yuchtman 2014; De la Croix, Doepke and Mokyr 2017). Consequently, war-induced urbanization, as argued for by Voigtländer and Voth (2013) and Dincecco and Onorato (2016; 2017), would stimulate economic activity when it stimulated the growth of cities with good economic institutions and as long as it enforced rather than undermined those institutions. The real question thus is, did it?

Importantly, the stylized representation of cities as the benign forces in political order obscures intensive scholarly debate on urban economic institutions. For instance, cities were often dominated by oligarchic elites that stifled economic activity (Greif et al. 1994; Ogilvie 2006; 2007; Puga and Trefler 2014). Stasavage (2011) provides strong evidence that those city-states that survived military competition often became more oligarchic in the process. In addition, even cities with internally benign economic institutions regularly imposed highly extractive economic institutions on their surrounding countryside (Epstein 1991). The potential stifling of rural regions is of significance, given that even the most urbanized parts of early modern Europe were essentially still agrarian economies where advances in production and productivity took place in the countryside or, at least, were heavily dependent on agrarian commodities (Van Bavel 2010). Accordingly, the question is not only whether warfare improved internal urban institutions, but also how it affected the position of the countryside as a consequence. In effect, it is not difficult to complicate the picture further by including the role of cities within the politics of territorial states (Blockmans 1989; 't Hart 1995; Angelucci,

Meraglia and Voigtländer 2018; Blondé, Hanus and Ryckbosch 2018; Prak 2018; Dittmar and Meisenzahl 2018) or by stressing the possibilities for market integration and trade, and thus economic growth in Europe's larger early modern territorial states (Epstein 2000; Rosenthal and Wong 2011).

Following this short digression, it is clear that the effects of warfare on political order are indeed conditional on the original political order. After all, war-induced strengthening of the political power of cities could lead to a more egalitarian distribution of violence between city, countryside and territorial state or to more unequal relations depending on the position of the city in the initial political order. Similarly, the effects of war-induced changes to political order on economic institutions are contingent on political order itself. Considering the relationship between warfare and political order, it becomes evident that economic structure cannot be treated exclusively as an outcome either. Instead, many of the arguments put forward here ultimately depend on economic structure. In particular, whether and how warfare changes political order internally, depends among other on the availability of taxable resources and how these resources are distributed. The consequences of military competition on political order, too, depend on which type of political entities and what state structures were better at generating resources. Is it possible then to determine some form of ultimate causality wherein either variation in war, political order, or economic structure is at the root of the subsequent co-development of all these factors?

Note, first of all, that a definitive analysis of the causal chain between warfare, political order, and economic structure is obviously complicated—and perhaps impossible as a general theory. This problem is aggravated by the available empirical material, because the historical source material itself dries up as one moves further back in time. Nonetheless, several influential attempts have been made. Some scholars have argued that geographic diversity in Europe stimulated the emergence of a central belt of cities, which in turn caused a fragmented distribution of violence capacity (Rokkan 1975; Tilly 1992; Spruyt 1994; Abramson 2017). Others argue that political fragmentation was the result of particular events such as the the partitioning of the Carolingian Empire (Stasavage 2011; 2016; Hoffman 2015; Dincecco and Onorato 2017) or the Black Death (Voigtländer and Voth 2013). Independent of the original explanation for political fragmentation, it is clear that political order thus shaped warfare and, in return, that warfare could reinforce or undermine the initial political order. Similarly, as military competition arguably stimulated state formation it is often argued that more capable states in turn made more war, at least in early modern Europe (Tilly 1985; 1992; Alesina and Spolaore 2005; Gennaioli and Voth 2015; Hoffman 2015). If that is the case, war-driven state formation—even in case it had internally positive

effects on economic institutions—would hinder long-term economic growth, simply because more war would lead to more destruction and deaths (Tilly 1975; Spruyt 2017). Accordingly, a case could be made for warfare, political order, or economic structure as the ultimate source of causation.

Even though warfare had the potential to produce systemic consequences, these consequences depended on initial differences across regions which, in return, could also be shaped by warfare itself. As discussed in this Section, warfare could do so by affecting political order—affecting the distribution of violence capacity, the capacity of state structures, and which groups in society could control the state—and economic structure. However, as the last part of this Section has shown, warfare itself is as much shaped by political order and economic structure. As evidenced by the first part of this Section, the intimate relationship between political order and patterns of warfare impacts the direct economic consequences of warfare. It did so not only through processes of state formation and institutions, but also by shaping patterns of destruction, recovery, and extraction. Decisions on where wars were fought, what groups in society were supported or hindered in the process of war, and which groups in society were to pay taxes could cause substantial variation in economic outcomes, as already alluded to before.

In addition, the realization that the economic consequences of warfare and the effect of warfare on political order are, in turn, conditioned by existing political order and economic structure, suggests caution in drawing general statements on the consequences of warfare. Unsurprisingly, the process of war-driven state formation is still widely researched and has, arguably, yielded more variation in outcomes and processes than statements with general applicability—for recent overviews, see Yun-Casalilla and O'Brien (2012), Fynn-Paul (2014), and Kapsersen and Strandsbjerg (2017). Besides the question of state capacity, the economic implications of war-driven changes to the distribution of violence capacity are equally unclear in terms of the consequences on economic institutions. Whereas some states were forced to grant concessions to a wider range of people, other states turned more extractive (Stasavage 2011; Spruyt 2017).

Ultimately, there is still little consensus as to what the economic consequences of warfare were in early modern Europe. Finding convincing answers depends on how the complex relationship between all the relevant factors are conceptualized and measured. Studies addressing this conundrum take one of three different approaches. The first approach is to find comparative case-studies wherein as many relevant factors are constant across the units of analysis. This is the approach of Di Tullio (2018)

in his study on the role of communal management in explaining local variation in the capacity of towns to cope with the burden of war. This approach is conceptually appealing but not frequently allowed by the available historical data. The second approach is to use plausible and clearly identifiable sources of exogenous variation in some of the variables at play here, and to use this variation to identify causal effects on other variables. However, the number of variables at play is large while data for the early modern period do not often allow for clear identification of the sources of variation.[7] The third approach is to use theoretical models to separate and identify different elements of the complicated relationship between economic structure, political order, and warfare as in, for instance Voigtländer and Voth (2013), Gennaioli and Voth (2015), and Hoffman (2015). The strength of such models crucially depends on whether they succeed in capturing the main elements conceptually. All these approaches thus have their advantages and drawbacks. In the next Section, I will discuss how I use these approaches to provide new insights on the economic consequences of warfare in this dissertation.

1.3 Contributions

As outlined throughout Section 1.2, it is clear that warfare produced substantial economic consequences in early modern Europe. It did so in a variety of direct and indirect ways, all of which varied over time and across space. Although not all these consequences were long-lasting, there are several channels through which the consequences of war could persist. Furthermore, how these effects varied over time and across space is not at all random, nor can it be taken as exogenously produced variation. Instead, how and where wars were fought and with what consequences on economic institutions, direct damages, and possibilities for recovery are all endogenously shaped by the relationship between economic structure and political order, and how they subsequently interact with patterns of warfare.

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[7] For instance, Juhasz (2018) uses variation in the efficiency with which the Napoleonic blockade was imposed to test long-term effects of war-driven trade disruptions. Becker, Ferrara, Melander and Pascali (2018) use variation in the noble status of local nobles as exogenous variation for the spatial pattern of warfare, in order to establish the causal effect of warfare on local institutions. Arguably, though, noble status of local elites is not independent of local institutions, nor is it independent of economic structure. Similarly, regional variation in the efficiency of the Napoleonic blockade could very well depend on initial variation in political order, which affects economic structure too. Studies using modern data are often better able to control for the complexity of these interactions, simply by controlling for such complicating interactions—see, for example Bellows and Miguel (2009), Miguel and Rolland (2011), Dube and Vargas (2013), Bazzi and Blattman (2014), Voors and Bulte (2014), and Sanchez de la Sierra (2018). In addition, several studies on the consequences of modern-day conflict resort to field experiments, which are obviously unavailable to historians—see Voors et al. (2012), and Cassar, Grosjean and Whitt (2013).

This dissertation consists of four focused analyses of the economic consequences of warfare. Rather than aiming to produce a comprehensive framework to solve all the empirical and conceptual problems associated with the economic consequences of warfare, which would far exceed the scope of this dissertation, it aims to deal with these problems in specific settings. That is, all the Chapters empirically test or theoretically conceptualize particular aspects of the economic consequences of warfare where they highlight the most relevant aspects of war, political order, and economic structure for the questions at hand. To do so, the Chapters aim to abstract from aspects that, given the particular empirical or theoretical setting, can be held constant. Consequently, particular aspects of the complex relationship between warfare, economic structure and political order can be isolated and subsequently analyzed.

The Chapters in this dissertation are all written as independent pieces. They combine different methodological approaches, such as historical case-material, quantitative empirical methods, and theoretical modeling. Although these methods are used to isolate specific elements of the relationship between war, economic structure, and political order, none of the Chapters is centered on one-directional causal identification. This is a deliberate choice. As should be evident from the review of the literature in the previous Section, warfare, political order and economic structure are intimately related. That warfare has economic consequences, or that political order affects the economy and patters of war should not be surprising. Still, identification of case-specific causal effects, based on plausible exogenous variation, can provide valuable new insights on these relations, and especially on their magnitude and sense of persistence. However, given the strong interdependence and co-development of the variables of interest, such causal findings are entirely conditional on the specific context wherein they are tested. In this dissertation, in contrast, I concentrate on developing an understanding of the joint role of warfare, economic structure, and political order in shaping economic outcomes. In doing so, the empirical context forms an essential ingredient of our understanding whereas, in a one-directional causal sense, it would constitute a nuisance limiting the external validity of the causal claim.

The early modern Low Countries form the empirical context for most of the material in this dissertation. In many ways, the Low Countries provide an attractive test case. As part of the western edge of the 'urban belt' of Europe, it was at the forefront of economic development, urbanization, and warfare during the early modern period (O'Brien 2011; Dincecco and Onorato 2016; 2017). In addition, the Low Countries featured an interesting variation in economic structure and political order, developing from a loose collection of principalities into a heavily urban-based Republic in the north and a constituent part of the Spanish Crown in the south (Stasavage 2011; 't Hart

2014; Prak 2018). And although the Low Countries had high urbanization rates, it also comprised flourishing rural economies based on highly competitive land and lease markets (Van Bavel 2009). In contrast to much of the recent literature, this dissertation takes the economic consequences of warfare on the countryside explicitly into account. It should be noted, however, that the Low Countries provide a somewhat extreme case-study, as the most urbanized part of Europe and due to its highly developed rural economies.

1.3.1 Death at a distance? Warfare and civilian mortality in the seventeenth-century Low Countries

Chapter 2, co-authored with Daniel Curtis, studies the direct economic consequences of warfare through its effects on civilian mortality. It does so by way of systematic econometric analysis of a large new database comprising annual burial registers of localities in the seventeenth-century Low Countries—ranging from small rural villages to large cities—and a detailed reconstruction of annual patterns of warfare. By focusing on short-term fluctuations and very localized patterns of warfare in a geographically restricted space, the Chapter establishes the direct mortality consequences of warfare at the local level. That is, even though variation in political order or economic structure were quite substantial in the Low Countries, the analysis isolates the short-term mortality effects from such variation. This is feasible because, even though the location of 'warfare' at the macro-level depended on conflict between and within political entities, the exact location of 'military activity' on a local level was much more random—especially in the Low Countries with its dense settlement and transportation networks, in combination with the flatness of the terrain, imposed few restrictions on army movements (Parker 1972: 12–18; Childs 1991: 32–33). Similarly, demographic trends of course varied across regions and, arguably, related to variation in economic structure and political order. On the other hand, the short-term effects of nearby military activity measured as deviations from those trends are by construction independent from these demographic trends.

The capacity of warfare to induce urbanization arguably is the most tangible empirical prediction in the existing literature on war-induced changes to the structure of the economy—as discussed in Section 1.2. Given a sharp distinction between urban and rural places with different economic functions and different demands for labor and capital, a shift in relative population between the two types of places is a clear change in the structure of the economy. Our focus on short-term mortality effects associated with nearby warfare can be contrasted to Dincecco and Onorato (2016; 2017) and

Voigländer and Voth (2013) who concentrate on longer term population changes, which they assume to be driven by rural-to-urban flight or disease-driven mortality in cities. Instead, our focus is on directly assessing the local and short-term mortality effects associated with warfare.

To measure the mortality effects of warfare, we construct a novel dataset covering annual mortality in 442 places in the seventeenth-century Low Countries. The mortality data are based on burial records from cities and rural villages throughout the Low Countries. Lacking accurate population figures for the vast majority of these localities, we construct an index of trend mortality per local. Subsequently, we calculate for each place and each year for which data is available the deviation of mortality from trend, providing a normalized index of mortality that is comparable across places. Note that this measure does not capture migration directly. Because we do not observe local population levels, any place that attracts migrants will grow in size and, even if the mortality rate in this place would remain the same, the total number of burials will go up, thus showing as an increase of mortality over the normal rate in our measure. Accordingly, and as further detailed below, our measure of mortality picks up both direct mortality and immigration which, according to Voigtländer and Voth (2013) and Dincecco and Onorato (2016; 2017) particularly affected cities.

The mortality data are combined with a detailed annual reconstruction of war events in the same region—and including war events around the Low Countries as well. That is, we aim to capture the spatial spread of hostile military activity per year throughout the Low Countries. This allows us to estimate the distance to the nearest front of military activity for each place—with mortality data—and each year.

Using panel regression analysis, we estimate the spatial effect of warfare on local increases in mortality. We find that places with nearby warfare experience significantly higher mortality than places further away from warfare. This raised mortality effect holds for places located within roughly 60 kilometers distance from war activity both in the same year or in the previous year. The magnitude of this effect is rather small, suggesting on average an increase in mortality from roughly 5 people dying per 100 inhabitants to 6 or 7. Although this effect is relatively minor, the average locality in the data experienced many years of nearby warfare throughout the seventeenth century. This implies that the mortality effects of warfare could, over time, accumulate to substantial demographic losses.

Importantly, we do not find any differences between small villages and cities. This is particularly surprising given that both rural-to-urban migration as well as disease-driven mortality should result in higher urban mortality outcomes in our measure. This result could possibly be explained by exceptionally high mortality effects of warfare

on the countryside—with the result that, even if people migrated, the remaining people died at such a rate that the number of burials still increased. In our additional tests, however, we find little evidence for such extensive rural damages, at least in a general sense. Instead, the more likely implication of our finding is that rural-to-urban flight and disease-driven mortality occurred but at modest rates and that these effects spread out thinly over a large number of cities.

1.3.2 Sharecropping as a kick-start contract: Coping with warfare in the central Low Countries, ca. 1500–1550

Whereas Chapter 2 aims to isolate the direct consequences of warfare from other factors, Chapter 3 studies how these consequences interact with local economic structure. The Chapter is based on a detailed archival study of the lease activities of a major landowning organization—Mariënweerd Abbey—in the Duchy of Guelders during the first half of the sixteenth century. The Abbey was relatively independent in its economic activities and had little influence on the patterns of warfare. And although warfare affected the overarching political order of the Duchy of Guelders, these political changes had little direct relevance for the local economy around the Abbey—at least given the relatively short time-frame of this study. At most, changes in the political order due to war were reflected in increased taxation. Hence, this historical case study focuses on the direct effects of warfare and how these consequences were dealt with in the local economy, and in isolation from political order. More specifically, it analyzes how war-induced damages affected and were solved through lease markets.

Much of the recent literature on the economic consequences of warfare, as discussed in Section 1.2, emphasizes the poverty of the countryside relative to cities and the inability of the countryside to cope with the destructive effects of warfare—see Rosenthal and Wong (2011) and Dincecco and Onorato (2016; 2017) in particular. However, the omnipresence of warfare in early modern Europe and the simple fact that it was still predominantly an agricultural economy would suggest that rural economies were at least somewhat resilient to warfare, or the entire early modern economy would have collapsed, cities included—see also the more regional literature cited in Section 1.2, Gutmann (1980) and Di Tullio (2018) for instance. Chapter 2 analyzes how and why a rural economy could cope with the damaged imposed by war, using the extensive archives of Mariënweerd Abbey.

The Abbey provides an insightful case-study for several reasons. It was located in the central river area of the Northern Low Countries in the Duchy of Guelders in a rural economy that was geared towards large-scale commercial livestock farming.

Thus, in contrast to much of the recent literature analyzing the economic consequences of warfare in early modern Europe (Rosenthal and Wong 2011; Voigtländer and Voth 2013; Dincecco and Onorato 2017), capital was an important production factor in this rural economy implying, too, that the local economy could be substantially affected through destruction of capital. Furthermore, feudal impositions on land and people had long disappeared in the region and property rights for land were clearly defined, stimulating competitive lease markets based on short-term contracts that allow for close analysis of economic behavior (Van Bavel 2009). Mariënweerd Abbey was one of the foremost landowners in the region and leased most of its landed estate—2,500 *morgen* or roughly 2,125 hectares in close proximity to the Abbey—through these competitive lease markets where fixed-rent cash contracts were the norm (Van Bavel 1993). Through its lease practices, the Abbey had a serious influence on the local rural economy. Finally, the region around Mariënweerd Abbey was confronted with regular bouts of warfare throughout the sixteenth century due to the Guelders Wars (1502 to 1543) and the Dutch Revolt (1566 to 1648).[8]

Based on the extensive lease accounts of the Abbey, I reconstruct the Abbey's lease practices between 1515 and 1550 with a particular focus on the period following the pillaging of the Abbey and intense fighting in the area in 1526 and 1527. In 1528, the Abbey and the surrounding economy were in disarray. Lease prices were low and the Abbey lacked the financial means to rebuild the productive capacity of its landed estate. In normal times and during minor crises, the Abbey stimulated recovery of its tenants by way of direct financial aid or through granting reductions on the lease prices, as it did following the short war-episode of 1542–43. In 1528 the Abbey could simply not afford such expenses but it needed rapid recovery in lease incomes to improve its own financial position nonetheless.

The Chapter shows that Mariënweerd Abbey managed to kick-start production on roughly a third of its landed estate by renting it to a handful of tenants with exceptionally large farms—up to 90 hectares in size—and aiding these tenants directly. Besides some directed financial aid to several of these tenants, the Abbey made extensive use of share contracts for these large tenants—interestingly, the Abbey used share contracts exclusively for these large tenants and only for about 12 years, or roughly two contractual terms. Share contracts imply that tenants pay a fixed share of their production to their landlord and, in addition, that landlords provide the same share of the non-labor inputs in the farm. Theoretically, share contracts provide little productive incentives and are

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[8] Of course, this particular labeling of war years hides a large variety in actual military activity and as much variety in the extent to which that activity affected the Abbey. The Abbey and the surrounding area were heavily affected by warfare between 1505-17, in 1527-28, in 1542-43 and during the first decades of the Dutch Revolt—although the focus of Chapter 3 is on the period before the Dutch Revolt.

commonly associated with poor tenants, economies with underdeveloped markets, and risky agricultural crops. In the case of Mariënweerd Abbey, however, they entailed a commitment of landlord and tenant to provide half of the livestock for the farm—with clear stipulations in the contract—in combination with a relatively accessible entrance for tenants in their large farms. The possible link between warfare and the adoption of share contracts in the Low Countries has been suggested by previous literature (Van Bavel 1993; 1999: Brusse 1999). However, Chapter 3 in this dissertation is the first to systematically reveal the link and to analyze how and why share contracts worked to limit the economic consequences of warfare.

The analysis of share contracts by Mariënweerd Abbey could be seen as an extreme example of the use of sharecropping. That is because the tenants with share contracts at Mariënweerd were rich—as evidenced by their capacity to work large farms—and the local economy was dynamic and capital-intensive (Van Bavel 1993). However, and in contrast to the recent literature emphasizing the relative poverty and low productivity of the countryside (Voigtländer and Voth 2013; Dincecco and Onorato 2017), the relatively dynamic and capital-intensive rural economy around Mariënweerd—and, similarly, its regular experience with warfare—was certainly not as extreme an example within the Low Countries (Van Bavel 2010), or even within Europe more generally (Hoffman 1996; Epstein 2000).

The effective but surprising application of share contracts by the Abbey—given that it was directed to the largest tenants and applied in an atypical manner—in a period of crisis in this Chapter alludes to two important findings. First, it shows the dynamism of this rural, capital-intensive economy in dealing with heavy damages. Second, it highlights the role of organizations such as Mariënweerd in the local economy in facilitating exchange and combining production factors. Together, these factors demonstrate that the economic consequences of warfare were heavily dependent on the existing economic structure. Besides these findings, the case of Mariënweerd also highlights that, even though the local economy was capable of dealing with large shocks such as the 1520s episode, warfare could in some cases completely undermine the local economy. This happened during the first decades of the Dutch Revolt, when the lease activities of the Abbey came to a complete standstill and the Abbey—a Catholic organization that was under intense pressure from Protestant forces—eventually lost most of its landed estate and was forced to cede its economic independence (Van Bavel 1993).

1.3.3 The economics of violence in natural states

Moving away from the direct economic consequences of warfare and bringing political order back in, Chapter 4, co-authored with Erik Ansink and Bas van Bavel, provides a stylized economic model of violent competition among violence specialists. The model abstracts from the particularities of war and adopts a stylized conceptualization of the state as an empty shell of organizational power. The actors in the model are violence specialists who either chose to join an elite coalition or to opportunistically exploit their violence capacity as warlords. The coalition of elites effectively forms the state, simply benefiting from the empty shell of organizational power. Elites tax the economy, taking into account that taxation entails negative productive incentives, and they collectively fight warlords. Warlords, in contrast, simply appropriate as much as they can. In a stylized way, this model captures how violent competition within or between societies is conditioned by the economy and, in turn, affects economic outcomes.

The stylized model developed in Chapter 4 builds on the literature relating violent political competition to development of the state and political order more generally, as discussed in Sections 1.1 and 1.2 above—in particular, Tilly (1992), Acemoglu and Robinson (2008) and North et al. (2009). In doing so, the model takes on board insights from the economic literature on conflict and appropriation although the focus of the model presented here is on large-scale conflict rather than the individual-level conflicts in the appropriation literature—for surveys of the appropriation literature, see Garfinkel and Skaperdas (2007) and Konrad (2009). Accordingly, the model of Chapter 4 integrates the role of economic structure into models of political order. In addition, and following North et al. (2009), the Chapter emphasizes the importance of competition among elites rather than exclusively focusing on hierarchic relations between a ruler—or monolithic state—and a relatively powerless population.

The main result of the model is that it suggests an apparent deadlock between economic development and the development of stable and growth-enhancing political order. Although this result confirms several existing theories, most of these theories derive this result from the unwillingness of an elite or ruler to adopt beneficial institutions (Acemoglu and Robinson 2006a; 2008), whereas elites in the model of Chapter 4 are not necessarily unwilling to limit extraction. Instead, it shows that violent competition entails high rates of extraction, while limits on taxation by the elite decrease the attractiveness of coalition-membership relative to warlordism, thus destabilizing the elite coalition and increasing conflict. Although the cooperative quality of the elite—or the organizational capacity of 'the state'—is exogenous in the model, the model provides suggestive support for the contention that increases in state capacity entail positive effects on the economy because it makes the elite more effective in

fighting off warlords. This limits total extraction in two ways, by reducing the role of warlords on the economy, and by providing more scope for elites to reduce their rates of taxation. In addition, state capacity could increase the stability of the coalition.

Whether or not increases in state-capacity—or, more precisely, the cooperative capacity of the elite coalition—stimulate the stability of the coalition relies on two further points. First, it depends on conflict over resources within the elite coalition. As state capacity increases the total amount of rents attracted by the elite, it decreases the attractiveness of 'warlordism' but it does increase incentives for elites to fight over the coalition's rents. The model shows that state-capacity, by increasing conflict over the rents within the coalition, could even undermine coalition stability and welfare levels—for violence specialists and ordinary producers.

Second, although the model abstracts from spatial considerations, it provides intuition on the role of point-based natural resources or clusters of production in economic and political outcomes. High levels of aggregate welfare in the model depend on a high elasticity of production,[9] strong organizational quality of the elite, and a limited decisiveness of conflict. The first of these three elements can be related to natural resources or productive urban centers, for instance. At the same time, these are precisely the factors that make the decisiveness of conflict more important— the violence specialist in control of a mine or city enjoys a clear advantage over his competitors. Although control over often well-defended cities is arguably more stable than control over point-based natural resources such as mines, cities themselves arguably decreased the stability of the elite coalition directly—recall that much of the literature discussed in Section 1.2 suggests that cities were effective centers of resistance against emerging states, thus nurturing the fragmented nature of early modern Europe (Blockmans 1989; Stasavage 2011; Prak 2018).

1.3.4 Understanding the economics of limited access orders: Incentives, organizations and the chronology of developments

Building on the insights from the stylized model developed in Chapter 4, Chapter 5, co-authored with Erik Ansink and Bas van Bavel, provides a conceptual historical reconstruction of the process of political and economic development in pre-modern Europe. It concentrates on the gradual evolution of some parts of Europe that developed from limited access orders towards open access orders—see also North et al. (2009) who argue that the development of elite organizations reduced individual violence capacity and, instead, stimulated the centralization of violence in the state. Building

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[9] The output elasticity determines how much differences in productive inputs matter for output.

on recent literature, and in contrast to North et al. (2009), Chapter 5 foregrounds the many organizations founded by ordinary people in medieval and early modern Europe such as guilds, town communities, and charitable organizations. Importantly, these organizations often remained outside the scope and control of the state and its elites. Warfare is brought forward to explain the evolution of state and economy, but in a different fashion than the literature on war-induced state formation—for example, in Tilly (1992) and subsequent work, see Section 1.2 above. Instead, the consequences of warfare are narrowed to its effect on organizations, and to the role of organizations in shaping war.

The starting point of this Chapter is to complement and further develop the insights from the previous Chapter and North et al. (2009) about progress in natural states with recent literature that has emphasized the distinct role of organizations of ordinary people in the political order and in the economic structure of early modern— and medieval—Northwest Europe (Epstein 1998; Olgivie 2006; Van Zanden 2009; Cantoni and Yuchtman 2014; Prak 2018). In this Chapter, we use that literature and the model of Chapter 4 to scrutinize the political order and economic structure in early modern Europe, emphasizing changes over time and the role of warfare and elite conflict therein. Producer organizations allowed ordinary people to coordinate their economic activities, jointly accumulate resources, and to organize political influence or even resistance within the scope of their organizations (Blockmans 1989; 1995). Besides stimulating economic activity, these organizations thus provided ordinary people collectively with a means to better protect themselves from appropriation—whether in the form of taxation by the state or as pure appropriation by warlords. In doing so, the members of these organizations had substantial influence on conflict among violence specialists—or of elites and their coalition. In several cases, organizations even allowed ordinary people to develop the capacity to wield large-scale violence themselves, thus keeping encroaching state-building elites at bay or even building their own political entities (Blockmans 1989; Stasavage 2011; Prak 2018).

Building on these premises in this Chapter, we use both elite organizations and the organizations of ordinary people as important markers of political order, as they shape as well as evidence the distribution of violence capacity and the organization of extraction and protection. Expanding the stylized model of the previous Chapter, Chapter 5 shows that organizations are central to the structure of extraction and protection in society because they shape the distribution of violence specialists between the coalition and warlords and they, especially the producer organizations, limit the total scope of extraction by violence specialists. Accordingly, and in contrast to North et al. (2009), Chapter 5 points out that not all these organizations increased the capacity of the

state. Instead, many organizations remained rivals of the state and of the elite that controlled the state—see also the discussion in Section 1.2 above. Organizations are an important element of economic structure too, as they facilitate exchange and allow the bundling of resources (Cantoni and Yuchtman 2014; De la Croix et al. 2017)—see also Chapter 3. Consequently, by establishing the effect of warfare on organizations—the number of organizations, the spread of access to and control over organizations, and the functioning of organizations—the consequences of warfare on economic structure and political order are made quite tangible in Chapter 5.

Following this line of reasoning, we argue that the role of warfare in the development of Northwest Europe was certainly not positive in a general sense. Although the result of conflicts—whether violent, thus war, or in a more peaceful manner—between elites as well as between elites and organized groups of ordinary people could go in any direction, the Chapter argues that warfare was often used by elites to overwhelm organizations of ordinary people. This happened not just as internal conflict—for instance, with local nobility clamping down on organizations—but also across states, or in a few cases even drawing elites from different states together to collectively assault autonomous cities or regions—for many examples, see Blickle (1997). Elites certainly did not always triumph in such wars, but where they did the consequences for economic development and political order were generally negative. In contrast, warfare between elites could have the exact opposite effect, by creating a power vacuum that increased the scope for ordinary people to organize themselves.

Ultimately, the early modern period brought mixed fortunes to non-elite organizations. Driven by the military revolution, states and their elites were able to gradually take the upper hand—see also Tilly (1992) and Prak (2018). In several regions, however, organizations of ordinary people managed to obtain a stake in the state itself. Although this did not always result in positive economic outcomes—as evidenced by Ogilvie (2006) who provides a case where the interaction between the state and producer organizations actually stimulated economic repression—it was only in those states that economic growth and developments toward open access orders occurred (Tilly 1992; Prak 2018).

1.4   By way of conclusion

In analyzing the capacity of warfare to systematically shape in economic outcomes, this dissertation aims to contribute new insights into the joint development of economy and political order in early modern Northwest Europe. Of course, even a brief sketch of the existing literature is enough to demonstrate that warfare did affect economies.

However, whether warfare produced systematic and lasting effects that can explain variation in economic structure is less evident. This is because the consequences of warfare were conditioned by existing variations in political order and economic structure, and by variation in patters of warfare which too, were shaped by political order and economic structure. Ultimately, whether warfare can be said to have produced substantive economic effects depends on the capacity of warfare to have influenced this deeper layer of variation in economic structure and political order. Instead, if it did not affect this deeper layer, warfare might have been no more than a mere byproduct of existing developments or, at best, a factor that reinforced existing development trends.

Using different methodologies and approaches, Chapters 2 to 5 analyze particular aspects of this question. Chapters 2 and 3 present empirical studies of the economic consequences of warfare. Both of these Chapters find effects but also illustrate that these effects were usually limited. Chapter 2 shows that hostile military activity increased civilian mortality in nearby localities. With the exception of the occasional year wherein warfare spread deadly epidemic diseases—which in most cases bore little relation to warfare—the mortality effects were quite limited in magnitude. Furthermore, the mortality effects were quite similar over time and across space. Chapter 3, in addition, highlights the remarkable resilience of a capital intensive rural economy in dealing with the destructive consequences of warfare.

The findings of Chapters 2 and 3 speak to the existing literature arguing that warfare stimulated urbanization and thereby economic growth. Even though there are many reasons to doubt the direct relation between urbanization and economic growth— see Section 1.2.1 above—Chapters 2 and 3 cast doubt on the link between warfare and urbanization itself. The Chapters show that rural regions could cope with the consequences of warfare relatively well in the early modern Low Countries. Generally, the findings in this dissertation, as well as the literature discussed in this Chapter, show that warfare produced some economic effects but that these effects were generally not that large nor necessarily systematic. Still, the findings in this dissertation suggest that when war did have serious consequences, these consequences had to do with the impact of warfare on organizations.

This dissertation foregrounds the role of organizations in political order and economic structure—Chapters 3 and 5 in particular. Organizations are impersonal containers of resources and facilitators of exchange. Thus, they had a distinct economic function as also evidenced by the role of Mariënweerd Abbey in Chapter 3. The emphasis on the economic function of organizations in early modern Europe is not novel in itself—see, for instance Epstein (1998), Ogilvie (2006), Cantoni and Yuchtman (2014) and De la Croix et al. (2017). Similarly, organizations are more often discussed

in studies of political order in early modern Europe—in particular, see North et al. (2009) and for producer organizations Blockmans (1989), Greif et al. (1994) and Prak (2018). The analysis of organizations developed and controlled by the elite already bridges two distinct approaches to political order, that of the state as a bureaucratic apparatus with independent agency, or the state as the sphere of elite competition (North 1981; North et al. 2009; Boix 2015). By combining the behavior of elites with the organizations that they can establish—and which, in return, shape the behavior of the same elites—one can not only describe but also analyze the distribution of violence capacity and the organization of extraction and protection—see especially North et al. (2009).

In this dissertation, I contrast elite-organizations with the organizations founded by ordinary people—or 'producer organizations'. Besides their economic function, producer organizations provided ordinary people with a coordination mechanism to voice resistance and, in some cases, to muster the capacity to project large-scale violence (Blockmans 1989; Blickle 1997; Prak 2018). In doing so, these organizations often protected ordinary people from extraction—even though the same organizations were sometimes used to suppress outsiders too (Epstein 1998; Ogilvie 2006)—and thus stimulated economic activity even further but not equally across regions or people. Whereas elite organizations can be viewed as stabilizing relations among elites and as increasing state capacity, the organizations of ordinary people were often in direct competition with the state and its elites. By combining the role of organizations in economic structure with their role in political order, and by including organizations of ordinary producers besides elite organizations, this dissertation presents a tangible conceptualization of political order and economic structure—and, therefore, of the variety of effects of warfare on political order and economic structure.

Did warfare then produce substantial economic consequences, not directly, but through its effect on organizations? Were organizations established because of war? Did warfare, for instance, systematically favor elite organizations over organizations of ordinary people? Did warfare increase competition between the state and organizations of ordinary people? Evidently, not all these questions can be answered here. What is clear is that organizations, as the backbone of local economies and of political order, form an essential and tangible component of the co-development of political order and economic structure. Accordingly, warfare could do real damage to economies if it undermined organizations—as shown in Chapter 3—or if it tilted the distribution of violence capacity away from growth-stimulating organizations—as discussed in Chapter 5. Warfare may have in some cases forced elites to incorporate organizations of ordinary producers—with their own capacity of violence—into the state. This certainly

did not happen everywhere, but it seemed to have happened in the Dutch Republic (Prak 2018) and, to some extent, in England (Angelucci et al. 2018), whereas it corrupted the political organizations of city states in Northern Italy (Stasavage 2011). The findings in this dissertation, however, highlight the importance of studying the existence and behavior of powerful individuals, and organized groups of individuals, in their relation with other organizations. In doing so, the joint development of economic structure and political order comes into view. Much further research is needed to systematically study the consequences of warfare on organizations. Yet, based on this dissertation, it can be argued that although warfare might have produced positive effects in particular cases, this dissertation provides no support for the general contention that warfare systematically explains positive changes to economic structure in early modern Europe.

Chapter 2

Death at a Distance? Warfare and Mortality in the Seventeenth-Century Low Countries

[1] Bram van Besouw and Daniel Curtis

Abstract: Using newly collected data from burial records and war events in the seventeenth-century Low Countries, we test the intuitive links between warfare and civilian mortality. We show that war episodes were spatially linked to raised mortality but the effects were limited. Although recent literature has linked warfare, rural-urban migration, and high urban mortality through epidemic disease, we show that warfare raised mortality within rural localities at least as much as in urban ones. Early modern urban mortality patterns may still be linked to favorable economic outcomes, but it had little to do with warfare per se.

2.1 Introduction

Northwest European communities regularly experienced episodes of warfare during the early modern period that could have substantial economic effects in the short term by killing people and destroying capital and infrastructure. The omnipresence and significance of these effects have stimulated a large literature analyzing the long-term economic consequences of European warfare. A number of scholars have argued that there are positive links from the destructive consequences of warfare to long-term economic growth in early modern Europe, whereby spatial patterns in warfare partially explain divergences in economic development. One such link connects war-induced migration to urbanization (Rosenthal and Wong 2011) and subsequent productivity gains of displaced migrants in the cities (Dincecco and Onorato 2016; 2017). In addition, based on the view that epidemic mortality was exacerbated in crowded cities (Clark and Cummins 2009), Voigtländer and Voth (2013) employed a model to show that the links between recurrent warfare, repeat mortality shocks, and rural-urban migration, could explain a large part of economic growth in early modern Northwest Europe.[2]

There is ample narrative evidence of warfare resulting in substantial civilian mortality (Lynn 1993; Outram 2002; Landers 2005). However, there is little systematic evidence to corroborate the notion of links between warfare and economic development through demographic channels. Gutmann (1980) demonstrates that demographic behavior was clearly influenced by periods of warfare, although these results are based on a detailed but localized study from which it is hard to generalize. Recent work has expanded the spatial and temporal scope, as Dincecco and Onorato (2016; 2017) show that pre-industrial warfare is spatially associated with city growth in Europe, which they explain as rural migrants fleeing towards the safety of cities. Similarly, Voigtländer and Voth (2013) find a correlation between the number of years that pre-industrial states were at war and their urbanization rates, which is consistent with their argument that warfare creates a mortality regime that eventually stimulates urbanization and high per capita incomes. However, the evidence provided by these studies is based on low-resolution macro data using large geographic or temporal units of analysis.

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[2] The logic behind a positive relationship between war-induced destruction and long-term economic development builds on a literature that links demographic shocks with realignments in production factors (Clark 2007; Pamuk 2007; Malanima 2018) and easing of Malthusian pressures (Galor and Weil 2000). Alfani (2013a; 2013b), Alfani and Murphy (2017) and Campbell (2016) challenge the notion of benign effects from large-scale demographic crises and show, in contrast, how such episodes can account for stagnation or reversal of economic development. Other literature linking war to positive economic development focus on channels that are not necessarily dependent on the destruction caused by war, such as increases in state capacity (Tilly 1992; 't Hart 1993; O'Brien 1996; Fritschy 2003; Hoffman 2011; Gennaioli and Voth 2015).

Consequently, our understanding as to how warfare affects demographic outcomes and whether there are systematic urban-rural differences in impact remains intuitive rather than empirically demonstrated.

In this paper, we provide new evidence on the relationship between warfare and civilian mortality using new data from the seventeenth-century Low Countries. To achieve this, the data are specifically compiled to provide both high-resolution material and systematic coverage. The mortality data are annual and comprise of 511 series of burial records from 442 localities, ranging from small rural localities to large urban ones and covering the entire Low Countries—although few individual series cover the entire 100 years. Because accurate population figures for individual localities are sparse, we calculate annual locality-specific increases over the normal mortality rate as the deviation of observed mortality from the locality-specific trend mortality.[3] Note that this implies that migration is not directly captured in our data. Instead, localities receiving migrants will have inflated deviations from trend mortality because, as their actual population increases, the same actual mortality rate still results in more burials compared to previous years.

A simple aggregation of the data is presented in Figure 2.1, which graphs a composite of locality-specific deviations from local trend mortality over time. The shaded segments of the graph are war-years; the vertical dashed lines identify aggregate mortality spikes in the Low Countries. All the major mortality crises occur during periods of war, whereas the one mortality crisis in a peace year—the dotted-dashed line for 1669—is not at all very large compared to the peaks during war years and, in effect, follows one year after the end of the War of Devolution (1667-68)—although 1668 saw no actual fighting in or near the Low Countries.[4]

Figure 2.1 presents a crude aggregation of information, overlooking spatial connections between mortality and the actual execution of war, and over represents regions with many observations, or localities with more variable mortality rates. Furthermore, given that the vast majority of years of the seventeenth century were war years, it is unsurprising that the main mortality spikes are in those years. It is thus possible that the

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[3] Trend mortality is calculated as a seven-year centered moving average, dropping the highest and lowest observation and the center observation within those seven years. See below for more detail on the burials data and the calculated mortality rates. Appendix B provides a description of data and sources. The population estimates allow us to distinguish between smaller and larger localities but are not frequent enough to use for calculating mortality rates.

[4] The textbook-wars affecting the Low Countries: the 1600-09 phase of the Dutch Revolt (up to the Twelve Years' Truce); the Thirty Years' War of 1618-48, extended to 1659 as the Franco-Spanish war; the 1664-68 episode covering the Second Anglo-Dutch War, the First Münster War and the War of Devolution; the Franco-Dutch War of 1672-78; the War of Reunions 1683-84; and the Nine Years' War of 1688-97. We discuss alternative measures of war below.

Figure 2.1: Average deviation from trend mortality in the seventeenth-century Low Countries
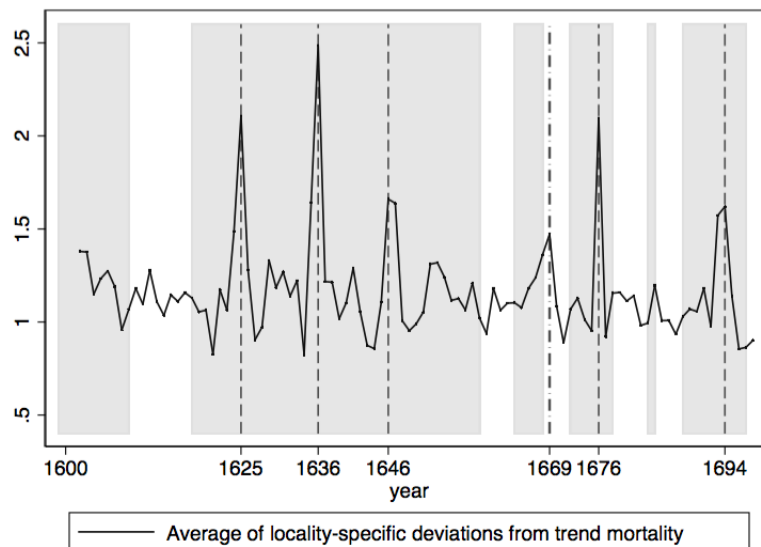
Sources: See Appendix B for a description of data and sources. *Note*: The main line presents for each year an unweighted average of locality-specific deviation from trend mortality, for all localities in the database that have an observation for that year. Local deviations from trend are based on a simple ratio of actual mortality over trend mortality per locality—see below. The shaded parts depict known war-years and the vertical lines date the main aggregate mortality spikes.

mortality spikes seen in the figure are causally unrelated to warfare. It is certainly clear from the figure that warfare does not inevitably lead to increased mortality, let alone mortality spikes. Indeed, this unclear relationship may be a reflection of the fact that periods of warfare could have wide-ranging effects on communities, as illustrated by an example from Hoge Mierde, Lage Mierde and Hulsel, three small villages in Brabant during the Dutch Wars of Independence. In a series of formal petitions, the villagers argue that they are unable to pay taxes due to successive calamities, including the billeting and lodging of soldiers, damage to capital goods and infrastructure, 'contributions' paid to armies, depopulation caused by epidemic disease and their general indebtedness and poverty. In addition to the direct effects of war, the petitioners mention consecutive failure of local harvests.[5]

The complaints of the three Brabantine villages resonate with the direct and destructive consequences of warfare. But to what extent is the experience of these villages representative for early modern communities' experience with warfare in general? Moreover, even when representative, differences between small rural localities

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[5] Based on a series of formal requests and letters bundled together in Brabants Historisch Informatie Centrum, Den Bosch, Raad en Rentmeester-Generaal Domeinen, 9, no. 18 [13/3/1638], [19/2/1639], [25/3/1639].

and towns were likely. Whereas the burdens of normal taxation and the billeting of allied soldiers can be expected to have been similar between countryside and towns, the ravages of bypassing armies, raising contributions, demanding lodging and, potentially, turning violent on people or capital likely diverged sharply between the open countryside and, often fortified, towns (Gutmann 1980; Lynn 1993; 't Hart 2014). Similarly, to what extent was the hardship expressed by the Brabantine villages actually dependent on warfare, and not on general circumstances of poverty? Put differently, are there clear spatial and temporal links between warfare and local deprivation and, in particular for this study, between warfare and civilian mortality?

Applying a simple fixed-effects model to establish the spatial association between warfare and raised mortality, we document three main findings. First, we find clear local and lagged effects of war events on civilian mortality. The contemporaneous effect extends to around 120 kilometers from the war event, although the magnitude of the effect declines rapidly after the first 30 kilometers—from an increase over normal mortality of about one-third initially, to about 10 percent thereafter. Lagged mortality effects are visible only within approximately 60 kilometers from war events but these effects dominate the contemporaneous effect. This lagged effect provides indirect evidence that epidemic diseases were an important factor in spreading war-driven mortality, though our results suggest that the reach of such diseases was often spatially limited.[6]

Second, we compare the mortality effects of different episodes of war. In doing so, we show that distance is essential when connecting war periods to higher mortality. Simply defining a war year as a year wherein a known war was fought in the Low Countries is not a precise measure, even though the Low Countries are of relatively small size by European standards—let alone by global standards. Some war episodes affected large parts of the Low Countries directly and thus connect to raised mortality throughout. Many other wars were much more local in nature. In addition, several conventional war years saw no fighting at all. Correcting for these issues, we show that all war episodes were significantly associated with high mortality—compared to peace years—but that the size and spread of this effect varied substantially across war episodes.

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[6] Our empirical findings thus lend indirect support to the plausible assumption that warfare and outbreaks of epidemic disease were often connected. Social disruption, ruination of crops, and the unhygienic conditions inside early modern armies were conducive to disease outbreaks (Parker 1972; Outram 2002; Landers 2005; Alfani 2013b; Curtis 2016b). These epidemic diseases could spread to the civilian population as well, as was sometimes explicitly noted in the burial records. In 1658 at Geraardsbergen someone was noted as 'qui a militibus fuit infectus' (who was infected from the military). Similarly, half the population of Montenaken is said to have died from water polluted by bodies of military casualties of the Battle of Landen (or Neerwinden) in 1693 (Gutman 1980, 165).

Third, we look to what extent the link between warfare and mortality is comparable between cities and countryside. This issue has received little systematic study, despite the fact that interactions between urban and rural environments play an important role in the explanations of the direct demographic and economic effects of warfare (Rosenthal and Wong 2011; Voigtländer and Voth 2013; Dincecco and Onorato 2017). Although our measure of deviations over trend mortality responds to both disease-driven mortality and to immigration in urban environments, our results show that warfare affected the countryside as much as cities.

2.2   The war and mortality data

The dataset used in this paper contains 442 geo-referenced localities of different population sizes, where we distinguish rural localities with less than 2,000 inhabitants from small, medium and large urban localities—2,000 to 4,999, 5,000 to 9,999 and over 10,000 inhabitants respectively. A detailed description of data and sources van be found in Appendix B—for mortality, local population estimates, and warfare.[7] Population estimates are irregularly spaced over time, usually with large intervals in between. Therefore, we analyze deviations in mortality over normal mortality per locality as an approximation of the mortality rate—as explained in the next subsection. The effects of war events on deviations over trend mortality are evaluated across different episodes of war. In addition, we compare changes in mortality within each locality related to the distance from war events. In all cases, we focus on differences between rural and urban impact. Applying a fixed-effects model allows us to capture locality-specific differences in mortality regimes—and, more likely, variation in burial registration practices and burial institutions—that are stable over time. Similarly, aggregate shocks are filtered from the analysis—using time fixed-effects.

To depict the spread of warfare, we provide new detailed geo-referenced data on war events in the seventeenth-century Low Countries based on a systematic reconstruction of military events. Although warfare was omnipresent in early modern Northwest Europe, military activity was geographically and spatially concentrated even within war years. In addition, the burden of warfare on communities came in several forms, such as taxation, billeting of soldiers, contributions, violence, and disease, as mentioned before through the petitions of the three Brabantine villages. Not all these forms are equally visible in the historical record as, for instance, raiding activities performed by small

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[7] The population estimates for the individual localities derive from several sources, which are listed in Appendix B. Estimates are available for over two-thirds of the localities in the dataset. However, the localities for which no population estimates are found are generally small hamlets and are, therefore, classified as rural in the dataset.

military units are not normally documented. In addition, the levying of contributions or the billeting of soldiers have been studied for some regions and periods, as in Gutmann (1980) on the region around Liège during the late seventeenth- and early eighteenth century but not for all regions in the Low Countries. Missing some of the smaller raids or the geographical scope of billeting or contributions would constitute a relatively minor statistical problem if those data are missing randomly. Systematic over- or under representation of some regions or periods, due to gaps in the literature or an uneven distribution of source material covering raids, billeting or contributions would bias empirical results when source coverage is related to spatial patterns in mortality or war activity—which is quite likely.

In order to avoid the issue of systematic misrepresentation, we focus on war events that are covered extensively and consistently in the literature; namely battles, sieges, and hostile occupations of localities, while we ignore the events that are less or unevenly covered—raids, billeting and contributions—altogether. Battles and especially sieges are relevant for the seventeenth-century Low Countries where, due to the high density of fortifications, wars were predominantly based on sieges while the movement of armies was relatively limited (Parker 1972, 12-18; Childs 1991, 32-33). Accordingly battles and sieges provide a systematic depiction of military movements. Based on an extensive survey of the literature, we document 755 war events distributed throughout the seventeenth-century Low Countries and its direct surroundings.[8] Combined with the mortality data, the database provides high-resolution empirical material on a substantial part of northwestern Europe, covering several wars and a variety of regions. The area and period under study are of particular relevance since the major polities in Western Europe were almost continuously engaged in war during this century, and the Low Countries often served as the main battlefield (Parker 1972; Israel 1997; Lynn 1999). Significant spatial and temporal variation existed in war activity, with different episodes of war affecting different parts of the Low Countries.

However, the focus on this systematic data comes with some caveats. First, the burials data do not consistently provide information on the cause of death of those registered. Second, detailed information on migration during the pre-industrial period remains elusive. Consequently, our results cannot be conclusive in determining an exact causal link between war and mortality and do not provide direct evidence on the mechanisms involved.[9] Plausible mechanisms for demographic effects are, death

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[8] This list of battles and sieges substantially expands on existing encyclopedic references that are often used in the literature (such as Clodfelter 2002). See Appendix B for a description of data and sources.

[9] Endogeneity issues are a concern too, especially concerning measurement of the data as we detail in the text. Note, however, that the local dimension of the mortality data strongly reduces reverse

through violence, through disease, or through famine—where famine generally kills people through heightening their sensitivity to disease (Gutmann 1980; Alfani and Ó Gráda 2017). However, although there are many examples of atrocities in early modern warfare, there is little evidence that warfare systematically led to violent civilian deaths in the seventeenth-century Low Countries (Parker 1972; Gutmann 1980; de Cauwer 2010; 't Hart 2014).[10] Furthermore, if violence caused death, then the effect should have been very local. Widespread famine, in addition, is generally disregarded as a major cause of death in the seventeenth-century Low Countries. Food staples were imported in large bulk and distribution was generally smooth (Curtis and Dijkman 2017; Curtis, Dijkman, Lambrecht and Vanhaute 2017). Of course, warfare could disrupt food distribution or result in local food shortages but given the direct link between localized warfare and localized famine, mortality effects along this channel are picked up in our analysis.[11] Migration, however, could affect our ability to measure local mortality accurately by changing population levels. More problematically, migration patterns are likely endogenous to warfare, inducing rural-to-urban flight (Rosenthal and Wong 2011). Consequently, the effects of warfare on urban localities presented below possibly overestimate urban mortality because an increase in burials over trend might reflect an increase in population rather than an increase in the mortality rate—and vice versa for rural localities.

2.2.1 The burial records

The mortality data in this paper derives from a newly-compiled database of church burial records we collected for the period 1600-99 in the Low Countries, providing

causality issues: these towns have little influence on the actual course, spatial spread, and duration of the wars fought during the seventeenth century. The variation in the spread of warfare was high, with some episodes focusing on the south (for example, the Franco-Spanish war, 1635-59), others on the east (for example, the Franco-Dutch war, 1672-78), and others on the central parts of the Low Countries (for example, the Dutch Revolt up to 1609 and again from 1618 to 1648).

[10] This doesn't imply an absolute absence of military violence towards civilians. There are several mentions of military personnel killing civilians in our sources; for instance from Maulde (Tournai) in 1659 where soldiers killed civilians in the church. Or in Goor (Overijssel) in 1665, and in Termunten (Groningen) and Zuidlaren (Drenthe) in 1672 where civilians were killed while fleeing a siege; and Oostwinkel (Vlaanderen) in 1678 where a citizen was robbed and killed by soldiers despite having a 'safeguard'—a paid-for free passage. Nor should the seventeenth-century Low Countries be generalized to other places or periods. The early years of the Dutch Revolt in the second half of the sixteenth century involved some particularly brutal episodes of warfare (Van Nierop 1999; Adriaenssen 2007). Similarly, seventeenth-century warfare in Germany was particularly brutal towards civilians (Parker 1984).

[11] The lack of a direct connection between harvest failure and mortality is quite specific for the Low Countries (Alfani and Ó Gráda 2017). The string of harvest failures in the 1690s, during the Nine Years' War resulted in widespread famine and starvation in France, for example, and was further exacerbated by import restrictions related to the war (Childs 1991, 250-1).

counts of the people buried per calender year from 442 localities, and yielding 522 individual series, since several localities, especially large cities, had multiple burial institutions—again, see Appendix B for a description of data and sources. Of these, we excluded series with too few observations or series from hospitals and plague houses, as these were likely to respond differently during high mortality periods and because they tended to admit soldiers external to the locality in question.[12] Of course, there are many missing observations with several series providing just a few years of burial registration. In addition, we drop start- and endpoints of the series other than 1600 and 1699, because it is usually unclear whether the burials recorded in such years span the entire year or just a part of it. More importantly, sudden stops or starts of the recording of burials might be correlated with the occurrence of mortality spikes. The real value of the burial records is that they are an index for mortality from the same standardized source that was in wide use over a long period of time (Curtis 2016b), notwithstanding the possibility that scribal practice may deviate over time, between regions, and between religious denominations.

Lacking high resolution population data, we approximate local mortality rates by focusing on deviations in local mortality. First, we calculate burial series-specific mortality trends as seven-year moving averages wherein the highest and lowest observation, as well as the center of the seven years are dropped. Deviations from mortality trends can be compared within a locality over time but also across localities. An additional advantage is that these deviations from trend mortality results automatically in trend-stationary data—whereas dropping center and highest and lowest observations limits further unit-root concerns.[13] Deviations from trend mortality—or 'increases over trend mortality' in the case of the regression results below—are calculated as the ratio of observed local mortality in year t over trend mortality—that is $y_{i,t} = \text{mortality}_{i,t} / \text{trend mortality}_{i,t}$. Given a normal mortality rate of 3 to 5 deaths per 100 inhabitants estimated for the seventeenth-century Low Countries (Van Zanden and Prak 2012, 241; Schroor 2014, 79), $y_{i,t}$ can be related to this mortality rate—with for instance $y_{i,t} = 1$ implying

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[12] A locality could have two or more burial institutions as a result of having different churches and hospitals. We deal with this by reconstructing an individual city's burials on a separate institution-by-institution basis rather than consolidating them all together. In doing so, we avoid incorrectly interpreting a sudden spike in the deviation from trend mortality, caused by the mere addition of a new institution in the accounts, as a mortality crisis. One limitation for cities that have more than one burial institution, however, is that the institutions may interact with each other. For some cities it is not possible to split the burials according by institution but for all these cities we were able to verify from what point in time the number of burial institutions remained stable—and included the consolidated series only from those dates. Nonetheless, this variety creates some noise and variation across cities. However, this should be absorbed in the fixed effects approach.

[13] Stationarity of the data is confirmed by the standard unit-root tests, although these tests tend to be weak for panels with large time and cross-sectional dimensions.

'normal mortality' and $y_{i,t} = 1.3$ implying a 30 percent increase over the normal rate. To economize on the number of observations available, we allow mortality trends—and thus deviations from mortality trends—to be calculated when there are at least four out of the six possible observations available in the seven-year time span of the moving average. Dropping the highest and lowest observation in the calculation of the moving averages results in more stable trends that are less sensitive to short-term deviations of the trend, such as mortality spikes (Alfani 2013a).[14]

One of the possible limitations of the burial records is that not all deaths were equally recorded—a detailed discussion is provided in Appendix B. Relatives of the deceased did not always have the finances for a church or cemetery burial, especially during famine or warfare when a number of family members had suddenly died together during an epidemic. However, there usually were different types of burials ranging from expensive to cheap. In addition, burials of the poor were often financed by the local poor tables. During epidemics, some of the deceased were buried in sites connected to hospitals or plague houses, which were sometimes recorded—but not always. During the worst epidemics, some people were buried together in undesignated pits (Noordegraaf and Valk 1988, 204). However, even when the dead were indiscriminately buried together, there is no evidence to suggest considerable under-recording—in Leiden even the dead simply thrown into the city ramparts were listed in the burial records, although indiscriminately listed. Some of the church burial records did not list the deaths of children but, even if they did not, records within localities were quite consistent in the coverage—that is, they either never or always recorded children. In terms of our calculations, the biggest assumption is that registering practices within a particular institution did not vary substantially during the century.[15] Although this is a strong assumption, there are few examples in the data that suggest substantial changes in registration practices.

Although burial records do often mention causes of death, this is unfortunately rarely consistent over time or between localities, or even within the same locality. Accordingly, we sometimes have information on whether someone died of military violence or from a disease, and are given an indication of what contemporaries thought the disease was or a list of its symptoms. However, at the same time, there were also many people in the burial records who died of diseases for which there is no indication

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[14] The annual organization of the data is not problematic since military activity and most epidemic diseases in the Low Countries were concentrated in the summer and autumn. Temporal gaps between death and burial are generally limited. Most burial records give both specific date of death and date of burial. Furthermore, urban governments used ordinances to urge citizens to bury plague dead within a matter of 48 hours (Noordegraaf and Valk 1988).

[15] Variation could be possible given the turnover of priests across the century.

of how they died.[16] Consequently, we cannot accurately determine causes of death in this study. The burial records did usually distinguish between civilian and military deaths, and this is important since we are looking to assess the effect of warfare on civilian mortality. Overall, soldiers could be buried in local church institutions of towns and villages. However, burials cost money and communities were understandably reluctant to use collective funds to finance the privilege of burial for random soldiers. Instead, larger amounts of soldier deaths were found in plague houses or hospital records, which are excluded from the data for that reason.[17]

Overall, we have burials data from 442 localities in the Low Countries and distributed over 511 time series, as can be seen in Table 2.1. Of those series, 366 are from rural localities, 43 from small urban localities, 27 from medium-sized urban localities, and 86 from large urban localities—the large number of series from large urban localities is due to the many large cities that have several burial institutions. These yield 22,347 mortality observations out of a potential total of 52,722—42 percent— and 20,143 calculations of increases over trend mortality—38 percent. The spatial distribution of the mortality data is shown in the left panel of Figure 2.2. Generally, the number of burial series is higher for the western provinces such as Holland and Vlaanderen, and much less for the eastern-most provinces. Although this coincides with known patterns of population in the early modern Low Countries, it is clear that the spatial distribution is not perfect but covers the Low Countries quite densely.[18] Figure 2.3 provides detail on the temporal coverage of the burial records. The left panel shows the coverage over the seventeenth century, broken down by regions.[19] The right panel shows the number of localities for which we have burial information, per year and broken down between urban and rural places.

Most localities produced burial records for only a part of the seventeenth century, generally starting somewhere during the century—burial registration was formally required in the Southern Netherlands from 1614 onward (Lambrecht 2014). Although

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[16] For example, in Leiden in 1604 we have 1,717 burials out of a total 3,311 (51.9 percent) explicitly attributed to plague (marked with a 'p'). Yet, during the bigger epidemics of 1624 (7,709 burials) and 1635 (16,336 burials), there is not a single reference to plague or any other disease.

[17] For example, the Onze-Lieve-Vrouw gasthuis (hospital) in Mechelen recorded 358 out of 853 burials as soldiers in the year 1692-7 (42 percent) in the worst phases of the Nine Years' War (Curtis and Dijkman 2018, 18).

[18] Several localities fall outside the official borders of the Low Countries—for instance Leer in the northeast (Dutch name is *Lieroord*)—these localities are assigned to the nearest province.

[19] Compared to Figure 2.2, Brabant includes Cambrai, Hainaut and Mechelen; Liège includes Landen van Overmaas, Limburg, Namur and Luxemburg; Vlaanderen includes Artois, Tournai and Zeeland; Friesland includes Groningen, Overijssel and Drenthe; and Gelre includes Utrecht. Provincial information within the Low Countries is in this paper only used with some statistical checks.

Table 2.1: Summary statistics of the main variables

	(1) Mean	(2) St. dev.	(3) Between	(4) Within	(5) Dummy count	(6) Observations
Mortality per locality						
Burials	68.24	252.58	170.99	146.02		22,347
Increase over trend mortality	1.15	0.98	0.24	0.96		20,143
Number of years with war event within 30 kilometers[α]						
Rural	10.88	6.58			364[β]	52,722
Small urban (2,000-4,999)	10.24	6.58			45	52,722
Medium urban (5,000-9,999)	8.93	4.62			27	52,722
Large urban (> 10,000)	9.20	6.67			86	52,722
Distance bins (in kilometers)						
Overall						
0-30	0.10	0.30	0.06	0.30	5,452	52,722
30-60	0.14	0.35	0.07	0.34	7,599	52,722
60-90	0.13	0.33	0.05	0.33	6,765	52,722
90-120	0.10	0.30	0.05	0.30	5,292	52,722
120-150	0.08	0.26	0.04	0.26	3,963	52,722
Conditional on locality-year having an observation for 'increase over trend mortality'						
0-30	0.11	0.32	0.12	0.30	2,275	20,143
30-60	0.16	0.37	0.14	0.35	3,212	20,143
60-90	0.13	0.34	0.10	0.32	2,602	20,143
90-120	0.09	0.29	0.09	0.28	1,904	20,143
120-150	0.08	0.27	0.08	0.26	1,538	20,143

Sources: See Appendix B for a description of data and sources.
[α] Using the number of years that localities are within 60 kilometers of a war event more than doubles the mean. Rural localities have 25.83 such years (s.d. 11.17), small urban 24.49 (s.d. 12.83), medium urban 22.78 (s.d. 10.84) and large urban 22.43 (s.d. 13.95).
[β] Indicates the total number of rural series in the database.

the amount of data is quite substantial, it is clear that it is much better for the second half of the century, and not as good for the northeastern parts of the Low Countries—especially for Friesland, Groningen, Drenthe and Overijssel.

2.2.2 War events in the seventeenth-century Low Countries

To generate an annual depiction of the spread of hostile military activity, we extensively collected data on war events throughout the seventeenth century, including battles, sieges and hostile occupations of localities per year within or close to the Low Countries—where we focus on war events that are systematically represented in the sources as explained above. To do so, we start from oft-used encyclopedic studies locating battles and sieges (Clodfelter 2002; Jacques 2007). Although these sources provide rich accounts of battles and sieges for many centuries and much larger geographies, extending these encyclopedic sources is necessary in our view. These encyclopedic lists are biased towards more spectacular events and to more data rich and economically more relevant regions. Hence, we supplement these with systematic

Figure 2.2: Spatial distribution of the burial records throughout the Low Countries
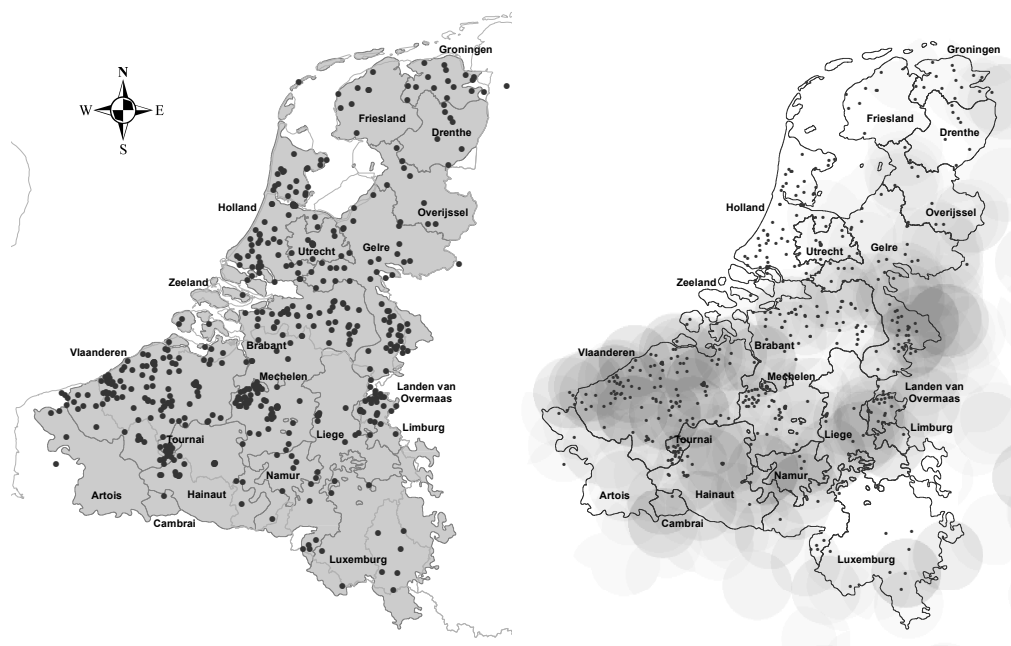
Sources: The shapefiles for this map were provided by Iason Jongepier, GIStorical Antwerp (UAntwerpen/Hercules Foundation). The left panel presents the locations of the burial records on a provincial map of the seventeenth-century Low Countries. The right panel presents the spatial distribution of the war events as explained in the text. See Appendix B for a description of data and sources. A full-page version of both maps can be found in Appendix A.

annual reconstructions of war campaigns using more precise secondary literature focused on the seventeenth-century Low Countries—an extensive description of data and sources can be found in Appendix B.

The nature of pre-industrial European warfare enables a thorough reconstruction of one or more annual sequences of events. This is because military campaigns in Northwest Europe generally started at the end of spring and ended by late autumn or early winter, due to weather conditions and the prospect of fresh harvests. This provides a natural point to extend the inquiry from high-profile events to annual military campaigns, by focusing on literature for particular periods of war or specific generals. As a result, we obtain sequences of military events within years, which provide an accurate overview of the areas affected by military activity. For completeness, we incorporate war sequences from regions bordering the Low Countries. This is a straightforward extension because the wars in the Low Countries were closely connected to warfare in western Germany and the northern and northeastern France and, therefore, the literature describing them strongly overlaps.[20]

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[20] We focus on military events performed by state armies, since warfare in the Low Countries by the

Figure 2.3: Number of localities with mortality data per year
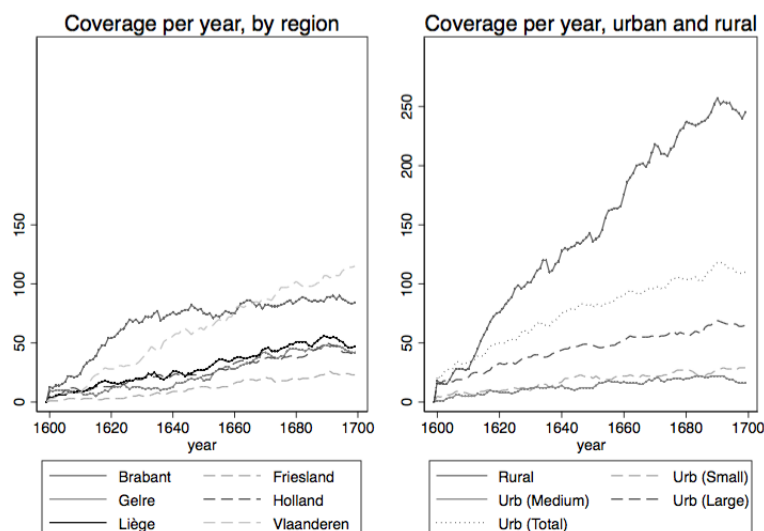
Sources: See Appendix B for a description of data and sources. *Note*: Small, Medium and Large refer to localities with 2,000 to 4,999; 5,000 to 9,999; and 10,000 and more inhabitants.

The dataset contains 755 geo-referenced war events in and around the Low Countries between 1599 and 1699.[21] We construct for each burial series, for each year of the seventeenth century, the distance to the war event nearest to the location of the burial series. These distances are translated into distance categories, where we effectively draw concentric rings of 30 kilometers radius around each burial series' location, coding in which 'distance bin' the closest war event of that year is found. The right panel of Figure 2.2 shows the spatial distribution of war events, organized by the number of years that places are within 30 kilometers of a war event—the darker, the more years of war events within 30 kilometers. Figure 2.2 shows a remarkable variation in war exposure across the Low Countries between the northern and southern halves,

seventeenth centuries was largely conducted by professional armies. Accordingly, we do not focus on banditry and organized crime, which certainly did take place in the seventeenth-century Low Countries (Egmond 1993), though perhaps at a reduced level than elsewhere across Europe at this time (Israel 1995, 678). We also do not focus on localized small-scale violent acts, which did occur but are difficult to discern in the data. Much of this violence was often related to deprivation at the local level, and therefore, upsurges in local violence may still be connected to warfare through its effect on general deprivation and hardship. During the height of the Nine Years' War and associated famine conditions in the rural hinterlands of Den Bosch, Brabant, for example, criminal offences increased 153 percent from the trend in 1694, with repeat acts of aggression such as violent coercion of those with tithe and rent arrears (Curtis and Dijkman 2018).

[21] Clodfelter (2002), arguably the most detailed of the war encyclopedias, lists around 800 war events for Europe at large between the years 800 and 1799—see Dincecco and Onorato (2016).

but also within the southern and northern halves.[22] Table 2.1 provides an overview of the war data and shows that the average locality had about 10 years with a war event less than 30 kilometers away during the seventeenth century. The bottom block of Table 2.1 shows the distribution of war events over distance bins but only for those locality-year observations for which increase over trend mortality is also available. Importantly, the differences between the third and fourth block are negligible, implying that the probability that a given locality-year pair has an observation of increase over trend mortality is unrelated to the distribution of war events. The same picture emerges from Figure 2.4, which shows the proportion of localities that have a war event within 30 kilometers or 30 to 60 kilometers distance over time. The left panel of Figure 2.4 presents this relation for the full sample of locality-year pairs while the right panel only uses locality-year pairs for which increases over trend mortality are available. The panels are similar in magnitude and the relative behavior of the two distance bins, except for the first 15 years of the seventeenth century. As these years are also the years for which mortality data are least available we exclude these years of the data from the analysis.

Although it is impossible to verify to what extent the list of war events is exhaustive, the wide range of detailed secondary literature available and the possibility of systematically reconstructing annual sequences yields a detailed depiction of the spatial spread of military activities, but perhaps not of all activities. In addition, given the large number of events and the geographic density of coverage within sequences—most sequences of war generate many war events within close range of each other—the translation into distance categories further increases precision in measuring distance to war for each locality in any particular year. An additional advantage of the distance categories is that they randomize possible omissions in the distance data because the size of the distance categories is orthogonal to noise in the data (Rosenthal and Strange 2003; 2008). Accordingly, possible omissions of individual war events are unlikely to affect our results.

There are two concerns to our approach which connect to potential differences in data quality across regions or across war episodes, and across regions within one individual war episode. First, if the literature on warfare is regionally unbalanced or if the literature is unbalanced across different wars with different regional patterns, then

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[22] Note that Holland, the main province of the emerging Dutch Republic, experienced almost no direct war events. This is driven by military policies of the Republic to safeguard Holland from invasion ('t Hart 1993; 2014). Fear for invasions remained real in Holland, however, as invasions had been real in the late sixteenth century and amphibious assaults from England were likely during several phases of the seventeenth century (Van Nimwegen 2010). Our main results are robust to excluding all burial series from Holland as is shown in the Appendix A.

Figure 2.4: Relative number of localities within certain distances to war events
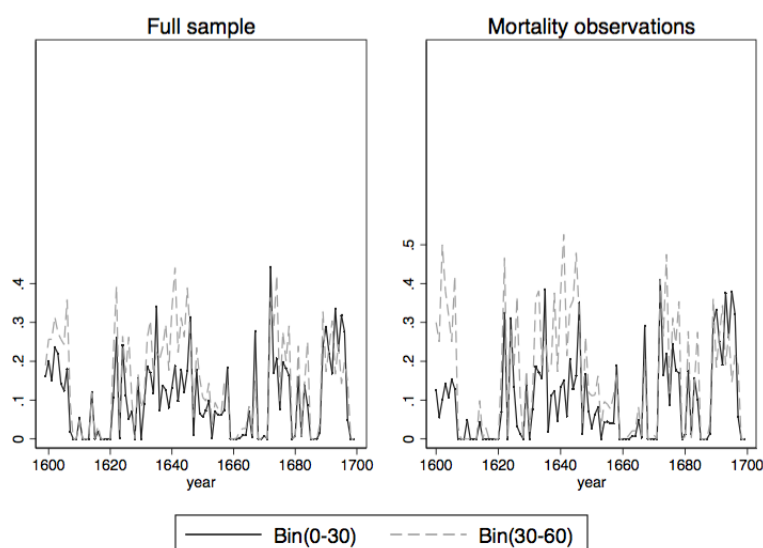
Sources: See Appendix B for a description of data and sources. *Note*: The left panel shows, over time, the proportion of localities within a certain range to a war-event, per year. The right panel shows this only for those localities for which we have mortality data for that same year.

there is a larger probability of missing out sequences of war events for some places than others. Second, the seventeenth century saw substantial changes in war practice, from siege warfare at the beginning of the century, to the emergence of large strategically maneuvered armies that dominated the final part of century. The number of war events listed in the literature is clearly larger for the former form of warfare than for the latter. The limited geographical and temporal scope of this paper as well as the abundant secondary literature, in our view, makes our approach feasible.

Arguably, relying on simple distance measures and indicator variables to grasp the exposure to war of individual localities is somewhat crude. However, developing alternative measures of war intensity is problematic. Without detailed data on the actual intensity of each war event—such as their duration or inflicted damages, information that is unavailable for most events—constructing such measures would necessarily be ad hoc. The evidence that is systematically available such as the number of war events per year is not necessarily a good proxy for the intensity of war on civilian communities or economy. In addition, the effect of missing out one event in a dense sequence is hardly problematic in the current set up, as explained above, but would be when constructing war intensity measures based on the number of war events near a locality.

2.3 Methodology

The data described in the previous Section yields a panel with 511 burial locations followed over 100 years, with numerous missing observations. We analyze the spatial and temporal relationship between war and civilian mortality using two simple fixed-effects frameworks. First, we establish the relationship between war years and local mortality, and between war episodes and local mortality. Second, we detail the more specific mortality effects over distance and over time. The dependent variable is the local increase in mortality over trend. The estimates presented in the next Section are average mortality effects for all series 'treated' by war. The long time dimension of the data almost inevitably includes aggregate shocks. Hence, we estimate all models using Driscoll-Kraay standard errors to account for cross-sectional dependence in the error terms (Driscoll and Kraay 1998; Vogelsang 2012).[23]

First, we investigate the aggregate effect of war on deviations in local mortality, as in Equation (1a), where we compare different definitions of war.

$$y_{i,t} = \alpha + \beta W_{(i,)t} + \gamma W_{(i,)t} * \text{Urban}(>5000)_i + \theta_i + \epsilon_{i,t}, \tag{1a}$$

where $y_{i,t}$ is the increase over trend mortality in burial location i at time t, α is a constant, θ_i the locality-specific fixed-effect, and $\epsilon_{i,t}$ the error term. The fixed-effects control for time-invariant differences across localities. Especially relevant here are differences across localities relating to burial and registration practices, and differences relating to the general health and disease environments in localities.

$W_{(i,)t}$ is an indicator variable signaling whether a particular year is a war year. We compare four different definitions of war; (a) coding a given year as a war year for all localities when that year is part of the textbook war episodes in the seventeenth-century Low Countries as in Figure 2.1 or, (b) coding a year as a general war year when there is at least one war event within 200 kilometers of one burial location.[24] These two definitions do not discriminate among burial locations, and do not rely on subscript i—which is given between brackets in Equation (1a) for that reason. The other two different definitions of war take value 1 for each year when locality i is (c) within 60 kilometers of a war event, or (d) when that locality is within 120 kilometers of

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[23] In addition, Driscoll-Kraay standard errors correct for standard heteroskedasticity and serial correlation yielding i.i.d. error terms and thus appropriate t-statistics.

[24] The main difference between the original war definition and the alternative one is that several official war years saw no actual fighting, whereas there are some years of minor wars with significant military activity fought in or just outside the main Low Countries—such as the Jülich Succession Wars in 1610 and 1614 or the French invasion of the Duchy of Luxembourg in the years preceding the War of Reunions.

a war event. Besides estimating the average effect of a war year on local mortality, Equation (1a) also estimates the marginal effect of the same relation for medium and large cities—i.e. localities with a population of at least 5,000 inhabitants—through the parameter γ.

Equation (1a) can be slightly adapted to estimate the relative severity of different war episodes, as in Equation (1b). Instead of estimating the effect of one indicator variable coding all war years, Equation (1b) has an indicator for each separate war episode. That is, Equation (1b) estimates a set D_w instead of W, for all the different war episodes, $w \in W$.

$$y_{i,t} = \alpha + \sum_{w \in W} \beta_w D_{i,t,w} + \sum_{w \in W} \gamma_w D_{i,t,w} \text{Urban}(>5000) + \theta_i + \upsilon_{i,t}. \tag{1b}$$

In a second set of empirical models, we estimate the spatial and chronological patterns of war-induced mortality. This is done using a two-way fixed effects model, adding year fixed-effects ϕ_t in addition to locality fixed-effects. The year fixed effects eliminate shocks to mortality that are common to all localities in a particular year, such as aggregate weather, epidemic, trade or harvest shocks.[25] The estimated effects are, therefore, more precise in estimating the distance effects per locality. In addition, these models incorporate a lag-structure to identify mortality effects playing out over several years.

The effect of distance is measured through distance bins—'Bin$(X - Y)$'—which are indicator variables that code whether a locality was within a certain distance bracket, between X and Y kilometers, to its nearest war event. The main text reports the results of regressions using bins with 30 kilometer distances, with $X \in \{0, 30, 60, 90, 120\}$ and $Y \in \{30, 60, 90, 120, 150\}$. The lagged variables identify to what extent previous years still affect current mortality outcomes. In addition, we include interaction terms of the distance bins with the urban indicator variables, using all three urban categories. Equation (2) describes the estimation of this set of models, repeated for all the different bins;

$$y_{i,t} = \beta_0 + \sum_{X,Y} \left[\sum_{\tau=0}^{2} \beta_{1;t-\tau} \text{Bin}(X - Y)_{i,t-\tau} + \sum_{U} \left[\sum_{\tau=0}^{2} \beta_{2,U;t-\tau} \text{Bin}(X - Y)_{i,t-\tau} * \text{Urb(U)}_i \right] \right]$$
$$+ \theta_i + \phi_t + \upsilon_{i,t}. \tag{2}$$

As in Equations (1a) and (1b), Equation (2) estimates an average effect of distance from war events on increase over normal mortality per burial location—β_1—and marginal effects for localities of different size—$\beta_{2,U}$ where U refers to the different

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[25] Market and price integration in the Low Countries during the seventeenth century were very high, so that trade and harvest shocks should be broadly similar for all localities (Curtis et al. 2017). Large local differences are possible in the case of trade disruptions or destruction of crops connected to warfare. These effects are picked up by our explanatory variables when they cause mortality effects, as intended.

categories of urban size. Because U leaves out only the rural localities, β_1 essentially estimates the average effect on rural localities, while the $\beta_{2,U}$'s estimates the difference of the respective urban categories with respect to the rural effect.

2.4 Results

2.4.1 Mortality effects across wars

As already emphasized above, this paper focuses on war events rather than war in political or diplomatic terms. Table 2.2, following Equation (1a), shows the outcome from a set of simple regressions of the local deviations from local trend mortality on indicator variables that divide the seventeenth century into war years and peace years, excluding the first 15 years from the analysis. The first column in Table 2.2 uses the official duration of the wars fought in the seventeenth-century Low Countries, while an interaction term estimating the urban bias of mortality is added in column 2—using the larger than 5,000 urban-category. The estimates of the average effect of war is statistically significant at the one percent level and suggest that local mortality in war years is on average 11 percent higher than in peace years, and that this effect is similar for urban and rural localities—the interaction term in column 2 is statistically insignificant. That is, if the mortality rate in the seventeenth-century Low Countries in normal times is 3 to 5 percent (Van Zanden and Prak 2012, 241; Schroor 2014, 79), then this rate is about 3.33 to 5.55 percent during war years. When a war year is defined as a year in which there is a war event within 200 kilometers of at least one locality in our sample, the average effect is no longer significant, which points to the fact that the occurrence of a war event in or around the Low Countries is not a good indication for war pressure.[26]

Columns 1-4 treat the entire Low Countries as affected in the war years. The Low Countries, with the exception of the Prince-Bishopric of Liège, formed one officially recognized polity at the start of the seventeenth century and this remained the case, at least in legal terms, until 1648. This could serve as an argument for treating the entire Low Countries as affected by war—similar arguments could be the strong economic and legal integration of the Low Countries, the fact that in all wars all of the polities in Low Countries were engaged, and on the relatively small size of the Low Countries. However, there should be a spatial and temporal link between warfare and mortality for the argument that warfare results in high mortality to have empirical relevance.

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[26] Recall that the there are several more war years in the second definition of war. The first definition uses the years of the textbook-wars fought in the Low Countries.

Table 2.2: Aggregate mortality effect

Increase over trend mortality	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
War	0.1075***	0.1110***						
	(0.0363)	(0.0414)						
* Urban (>5000)		-0.0133						
		(0.0357)						
War (alt)			0.1088	0.0968				
			(0.0656)	(0.0713)				
* Urban (>5000)				0.0461				
				(0.0348)				
War (60)					0.2053***	0.2198***		
					(0.0504)	(0.0586)		
* Urban (>5000)						-0.0591		
						(0.0440)		
War (120)							0.1584***	0.1782***
							(0.0486)	(0.0551)
* Urban (>5000)								-0.0757*
								(0.0413)
Observations	19,460	19,460	19,460	19,460	19,460	19,460	19,460	19,460
Number of groups	506	506	506	506	506	506	506	506
Town FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	N	N	N	N	N	N	N	N
D-C	Y	Y	Y	Y	Y	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

Columns 5-8 thus use a definition of war that discriminates between localities: only those localities that are within 60 (columns 5 and 6) or 120 kilometers (columns 7 and 8) to a war event in a particular year out of those textbook wars experience that 'war'. This results in larger estimates of the mortality effect of war. Furthermore, column 8 suggest that rural areas experienced heavier wartime mortality effects, although the strength of this finding is very limited—and significant at the ten percent level and not at all in column 6.

In Tables 2.3 and 2.4, we show the differences in mortality effects associated with different war episodes, using Equation (1b). Table 2.3 shows these differences applying the third measure introduced in Table 2.2, coding for all years only those localities within 60 kilometers from a war event in that year. The first two columns show estimates when treating the extended Thirty Years' War period, 1618-59, as one war, whereas columns 3 and 4 break it down between the Thirty Years' War before active French intervention up to 1635, and the period after French involvement from 1635 up to 1659. Columns 1 and 3 estimate the overall average effect, whereas columns 2 and 4 include interaction terms to disentangle urban and rural mortality differences—using Equation (1b) instead of Equation (1a), and using the 5,000 inhabitants urban category. Significant increases over trend mortality are seen for all wars in Table 2.3 except the Nine Years' War. The size of the mortality effect differed between wars, however; the

Table 2.3: Aggregate mortality effect per war with 60 kilometer cutoff

Increase over trend mortality	(1)	(2)	(3)	(4)
Thirty Years-Ext (60)	0.2184***	0.2283**		
	(0.0794)	(0.0940)		
Thirty Years-Short (60)			0.3371**	0.3505**
			(0.1350)	(0.1722)
Thirty Years-French (60)			0.1580*	0.1647*
			(0.0837)	(0.0973)
Anglo, Munster, Devolution (60)	0.2665***	0.2504***	0.2641***	0.2479***
	(0.0420)	(0.0499)	(0.0412)	(0.0488)
Franco-Dutch (60)	0.2593**	0.3035*	0.2580**	0.3018*
	(0.1292)	(0.1555)	(0.1289)	(0.1552)
Reunions (60)	0.1329**	0.1093*	0.1294*	0.1054*
	(0.0662)	(0.0570)	(0.0663)	(0.0571)
Nine Years (60)	0.1535	0.1634	0.1513	0.1607
	(0.0998)	(0.1064)	(0.0999)	(0.1064)
Thirty Years-Ext * Urban (60)		-0.0408		
		(0.0707)		
Thirty Years-Short * Urban (60)				-0.0587
				(0.1691)
Thirty Years-French * Urban (60)				-0.0269
				(0.0633)
Anglo, Munster, Devolution * Urban (60)		0.0703		0.0711
		(0.0484)		(0.0473)
Franco-Dutch * Urban (60)		-0.1778		-0.1764
		(0.1162)		(0.1160)
Reunions * Urban (60)		0.1279*		0.1294*
		(0.0661)		(0.0659)
Nine Years * Urban (60)		-0.0383		-0.0366
		(0.0550)		(0.0549)
Observations	19,460	19,460	19,460	19,460
Number of groups	506	506	506	506
Town FE	Y	Y	Y	Y
Year FE	N	N	N	N
D-C	Y	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

most severe ones proving to be the early Thirty Years' War period up to the French interventions, the Anglo-Münster-Devolution Wars, and the Franco-Dutch Wars, while mortality effects from the War of Reunions and the second phase of the Thirty Years' War were more modest. In general, there is little sign of urban-rural differences in mortality effect, except for the War of Reunions—which increased mortality more in cities than in the rural and small urban localities.

Using 120 kilometers as the threshold between being war affected or not, un-surprisingly, lowers the estimated mortality effects quite substantially for all the wars. Rather than the early phases of the Thirty Years' War, the most significant war episode becomes the Franco-Dutch War—suggesting a wider spread of mortality effects from that war. The connection between warfare and mortality for the War of Reunions is no

Table 2.4: Aggregate mortality effect per war with 120 kilometer cutoff

Increase iover trend mortality	(1)	(2)	(3)	(4)
Thirty Years-Ext (120)	0.1629**	0.1777**		
	(0.0662)	(0.0733)		
Thirty Years-Short (120)			0.2139**	0.2178*
			(0.0936)	(0.1173)
Thirty Years-French (120)			0.1401*	0.1604*
			(0.0789)	(0.0887)
Anglo, Munster, Devolution (120)	0.1788***	0.2044***	0.1783***	0.2040***
	(0.0546)	(0.0590)	(0.0542)	(0.0588)
Franco-Dutch (120)	0.2200*	0.2642*	0.2192*	0.2635*
	(0.1259)	(0.1509)	(0.1258)	(0.1509)
Reunions (120)	0.0877	0.0863	0.0861	0.0850
	(0.0610)	(0.0579)	(0.0611)	(0.0580)
Nine Years (120)	0.1129	0.1272	0.1118	0.1263
	(0.0862)	(0.0887)	(0.0862)	(0.0887)
Thirty Years-Ext * Urban (120)		-0.0551		
		(0.0432)		
Thirty Years-Short * Urban (120)				-0.0189
				(0.1001)
Thirty Years-French * Urban (120)				-0.0747
				(0.0461)
Anglo, Munster, Devolution * Urban (120)		-0.0939**		-0.0944**
		(0.0378)		(0.0377)
Franco-Dutch * Urban (120)		-0.1862		-0.1867
		(0.1194)		(0.1198)
Reunions * Urban (120)		0.0138		0.0130
		(0.0346)		(0.0346)
Nine Years * Urban (120)		-0.0567		-0.0571
		(0.0425)		(0.0424)
Observations	19,460	19,460	19,460	19,460
Number of groups	506	506	506	506
Town FE	Y	Y	Y	Y
Year FE	N	N	N	N
D-C	Y	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

longer statistically significant when accounting for localities up to 120 kilometers away. More surprising, however, are some changes to the estimated differences between urban and rural localities. The estimates for urban mortality in Table 2.4 are generally insignificant, as in Table 2.3, with the exception of the 1665-68 episode. Increases in urban mortality were about 9 percent lower than in rural and small urban localities—an increase over trend mortality of 11 percent versus 20 percent—when using the 120 kilometers threshold, whereas no such effect was found when using the 60 kilometers threshold. The reverse is true for the War of Reunions for which no effect is now found.

A comparison between Tables 2.3 and 2.4 adds further weight to our emphasis on distance to identify the actual impact of warfare. Unsurprisingly, the mortality effects estimated in Table 2.4 are generally smaller than those in Table 2.3: including

localities further removed from the actual war events means including many localities that experience a small or no mortality effect. On the other hand, there are a few important reversals, especially concerning the urban-rural comparisons. In general, the decline of the estimated coefficients when comparing Tables 2.3 and 2.4 is not the same for all wars or all urban-rural differentials, nor for changes in statistical significance. Some of the estimates in Table 2.4 are as precise as the same estimates in Table 2.3: in some cases, the standard errors are even lower in Table 2.4. Partially this is due to the higher number of observations that are treated as affected in Table 2.4—there are simply more burial locations within 120 kilometer of a war event than there are burial locations within 60 kilometers—thus increasing precision. At the same time, the expected magnitude of the mortality effect is smaller. These are not statistical artifacts but relevant information about these war episodes.[27]

2.4.2   Spatial and temporal effects within wars

We now turn to more precisely estimating the spatial and temporal effects of war on mortality within war years, applying Equation (2). Table 2.5 shows the estimates of that Equation using bins of 30 kilometers. The first column shows the contemporaneous relation between distance from war events and increases over trend mortality, while column 2 and 3 add lagged effects of war exposure in the previous year and the year before that, respectively. To discriminate between urban and rural localities, and between urban localities of different size, the spatial and temporal effects are estimated simultaneously with interaction terms of the urban dummies for all urban categories.

Several findings are clear from Table 2.5. First, the average spatial relation between war events and local mortality is quite substantial and estimated at 43 percent within the first 30 kilometers. This mortality effect diminishes somewhat at further distances, but a clear and significant mortality effect is still seen for localities up to 120 kilometers away from war events. Thus the spatial spread of mortality from wars is wide, although the magnitude of the mortality increase is limited. Second, the estimates for the lagged effects are also very substantial and statistically significant within the first 60 kilometers—similar or even larger in magnitude than same-year mortality. The magnitude of the contemporaneous effect diminishes slightly after introducing the lagged terms. Clearly, mortality is associated with past war events, although only visible up to 60 kilometers and only applicable to the first lagged year. The mortality effects of war events beyond 60 kilometers almost disappear while including the lagged terms.

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[27] The results presented in Tables 2.3 and 2.4 are robust to including the first 15 years of the century and to changes in the distance cutoff—see Appendix A for these results.

In addition, there are some insights to be drawn from the urban-rural disaggregation of the data. Recall that the average effect in the first block of Table 2.5 essentially represents the effect of war on rural mortality increases, whereas the second, third and fourth block estimate the difference between the small, medium and urban cities, compared to the rural effect. Table 2.5 provides little evidence for differences between urban and rural wartime mortality. When comparing the small urban localities to the rural localities, there is a complete absence of mortality differences, and yet this is not completely surprising given that these smaller 'urban' localities really did not have many 'urban' functions that made them distinctive from the rural localities, except for a higher population. Indeed, localities of around 2,000 inhabitants could feasibly be seen as large villages in most cases. When comparing the larger urban localities, there are some signs that mortality through warfare may have been slightly lower than in rural localities, although this evidence is fairly weak. These results are robust to several variations of the regressions presented in Table 2.5.[28] Ultimately, what cannot be discerned from these results is clear evidence for a connection between early modern warfare and a strong impact on specifically urban mortality.

2.4.3 Magnitudes and interpretation

Although the results presented above show a clear connection between war events and raised mortality, the magnitude and interpretation of these findings deserves further discussion. The results show that warfare did not on average result in large mortality spikes. Even within 30 kilometers from war events mortality increased by roughly one-third over the normal rate, implying a death rate of 4 to 6.7 deaths per 100 inhabitants compared to 3 to 5 people in normal years. Still, the occurrence of nearby war events was not uncommon in the seventeenth-century Low Countries as was noted in Table 2.1. On average, a locality experienced roughly 10 years with a war event within a distance of 30 kilometers. Consequently, the mortality effects of warfare may have been small but frequent.

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[28] The results are robust to changes in the size of the distance bins—such as using bins of 20 or 40 kilometers—or slightly different calculations of deviations from trend mortality—using a seven-year moving average and not dropping the center or the highest and lowest observations. In addition, the results are robust to dropping particular provinces or the inclusion of region-time fixed effects. These results can be found in Appendix A.

Table 2.5: Spatial and temporal variation in local mortality

Increase over trend mortality	(1)	(2)	(3)
Bin 0-30	0.4259***	0.3144***	0.3296***
	(0.0944)	(0.0788)	(0.0817)
L1		0.3225***	0.3437***
		(0.0897)	(0.0878)
L2			-0.1027
			(0.0646)
Bin 30-60	0.1588**	0.0813	0.0943*
	(0.0629)	(0.0521)	(0.0538)
L1		0.1711***	0.1834***
		(0.0649)	(0.0668)
L2			-0.0373
			(0.0622)
Bin 60-90	0.1062*	0.0487	0.0634
	(0.0594)	(0.0485)	(0.0525)
L1		0.0940	0.1035
		(0.0759)	(0.0763)
L2			-0.0509
			(0.0703)
Bin 90-120	0.1203**	0.0760	0.0917*
	(0.0595)	(0.0513)	(0.0520)
L1		0.0769	0.0873
		(0.0638)	(0.0676)
L2			-0.0824
			(0.0532)
Bin 120-150	0.0384	0.0083	0.0184
	(0.0557)	(0.0498)	(0.0495)
L1		0.0075	0.0154
		(0.0416)	(0.0433)
L2			-0.0436
			(0.0349)
Bin 0-30 * Urban (2000)	-0.0118	-0.0242	-0.0173
	(0.0850)	(0.0886)	(0.0870)
L1		0.0179	0.0210
		(0.0911)	(0.0905)
L2			0.0121
			(0.0893)
Bin 30-60 * Urban (2000)	0.0794	0.0693	0.0784
	(0.0881)	(0.0873)	(0.0867)
L1		0.0529	0.0701
		(0.0573)	(0.0572)
L2			-0.1007
			(0.1019)
Bin 60-90 * Urban (2000)	-0.0325	-0.0456	-0.0372
	(0.0652)	(0.0645)	(0.0653)
L1		0.0171	0.0336
		(0.0726)	(0.0771)
L2			0.0071
			(0.0825)
Bin 90-120 * Urban (2000)	0.0301	0.0221	0.0399
	(0.1017)	(0.1070)	(0.1102)
L1		-0.0295	-0.0126
		(0.0839)	(0.0922)
L2			-0.0971
			(0.0905)
Bin 120-150 * Urban (2000)	-0.0094	-0.0370	-0.0320
	(0.0528)	(0.0621)	(0.0651)
L1		-0.0085	0.0096
		(0.0561)	(0.0611)
L2			-0.0995
			(0.0726)

(Table continues on next page)

(Table 2.5 continued)

Bin 0-30 * Urban (5000)	0.0207	0.0163	0.0321
	(0.1714)	(0.1524)	(0.1543)
L1		0.0823	0.1532
		(0.1757)	(0.1901)
L2			-0.2189*
			(0.1312)
Bin 30-60 * Urban (5000)	-0.0822	-0.0733	-0.0497
	(0.0960)	(0.1008)	(0.1069)
L1		-0.1002	-0.0589
		(0.1032)	(0.1084)
L2			-0.0975
			(0.1304)
Bin 60-90 * Urban (5000)	-0.1254*	-0.0850	-0.0803
	(0.0721)	(0.0670)	(0.0683)
L1		-0.1031	-0.0932
		(0.1000)	(0.1028)
L2			-0.0193
			(0.0945)
Bin 90-120 * Urban (5000)	0.1413	0.1810	0.1589
	(0.1370)	(0.1566)	(0.1449)
L1		-0.2286	-0.2137
		(0.1672)	(0.1785)
L2			0.0819
			(0.2585)
Bin 120-150 * Urban (5000)	0.4634	0.5002*	0.4946*
	(0.2851)	(0.2944)	(0.2837)
L1		0.1042	0.0974
		(0.2026)	(0.1910)
L2			-0.0623
			(0.1653)
Bin 0-30 * Urban (10000)	-0.0719	-0.0414	-0.0299
	(0.0854)	(0.0779)	(0.0770)
L1		-0.0936	-0.0447
		(0.0898)	(0.1098)
L2			-0.1389*
			(0.0829)
Bin 30-60 * Urban (10000)	-0.0423	-0.0248	-0.0059
	(0.0460)	(0.0398)	(0.0397)
L1		-0.0161	0.0305
		(0.0433)	(0.0565)
L2			-0.1033
			(0.0905)
Bin 60-90 * Urban (10000)	-0.0624	-0.0441	-0.0193
	(0.0472)	(0.0447)	(0.0466)
L1		-0.0478	-0.0094
		(0.0468)	(0.0498)
L2			-0.0858
			(0.0711)
Bin 90-120 * Urban (10000)	-0.1285**	-0.1064**	-0.0858
	(0.0491)	(0.0494)	(0.0536)
L1		-0.0590	-0.0286
		(0.0505)	(0.0602)
L2			-0.1170
			(0.0708)
Bin 120-150 * Urban (10000)	-0.0347	-0.0200	-0.0174
	(0.0757)	(0.0714)	(0.0667)
L1		-0.0375	0.0001
		(0.0418)	(0.0419)
L2			-0.0425
			(0.0532)
Observations	19,460	19,460	19,460
Number of groups	506	506	506
Town FE	Y	Y	Y
Year FE	Y	Y	Y
D-C	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

However, the average effects estimated above potentially hide occasions where war leads to much larger mortality spikes either during particular years or in particular places. First, we turn to the significance of the war-mortality relation in explaining the main aggregate mortality years in the Low Countries as identified in Figure 2.1. Table 2.6 reports the results of a simple OLS-regression of increase over trend mortality on the respective distance bins—the contemporaneous effect and first two lags—for the peak years. What is clear from Table 2.6 is that while large increases in mortality in 1625 were connected to same-year war events, and that large increases in mortality in 1676 were connected to previous-year war events, the other mortality peaks in the seventeenth-century Low Countries had little to do with proximity to war events. The plague of 1636, for example, brought widespread mortality increases in all the different distance bins, but without a clear association with war events.[29] The most significant average mortality increase was for those localities within 30 kilometers of a war event in 1625, which was nearly a sevenfold increase, and thus representing roughly a 20 to 30 percent death rate. But even when significantly related to war events, most mortality increases tended to be around a doubling or tripling of mortality over trend, thus exhibiting overall death rates of 10 to 15 percent of the population.

Second, the data contains a number of localities in certain years with exceptional increases in mortality, as seen from Table 2.7. For instance, the twenty-fold increases over trend mortality in the two Campine villages of Gierle and Minderhout correspond to death rates of 60 to 80 percent—if the 'normal' death rates can be considered 3 to 5 percent, and without accounting for possible outward migration at that time. These are exceptional demographic losses that are comparable to figures cited for the Black Death of 1347-52 (Alfani and Murphy 2017). Table 2.7 does point to a connection between exceptional local mortality spikes and proximity of war events, as 7 out of the 10 highest local mortality spikes were near a war event in that same year or the previous year—and one more case, Gierle (1636) that falls just outside this mark. Nonetheless, there are several exceptional local mortality spikes that seem to be unconnected to warfare.[30]

Overall, the results presented here point towards a clear spatial relation between warfare and raised mortality. That relation, however, is relatively limited in terms of space—affecting localities mostly within 60 kilometers—and in size when placed in a comparative perspective within early modern Europe. Alfani (2013a; 2013b),

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[29] See for the plague of 1625 in the Low Countries (Rommes 2015) and for dysentery in 1676 Bruneel (1977). See Curtis (2016b) for an overview of epidemics in the seventeenth-century Low Countries.

[30] The massive mortality spike in Termunten in 1686 is connected to to the St. Martins Flood in November of that year.

Table 2.6: War-mortality relationship in peak mortality years

Increase over trend mortality	(1) 1625	(2) 1636	(3) 1646	(4) 1669	(5) 1676	(6) 1694
Bin 0-30	5.7190***	2.3511	-0.0418	.	0.6864	-0.9043
	(1.4623)	(4.3797)	(1.2774)	.	(0.5532)	(1.4551)
L1	-0.7215	-1.0541	1.8669*	.	0.8789	-0.2106
	(1.8183)	(3.4922)	(0.9859)	.	(0.5451)	(0.9894)
L2	(n/t)	-0.2259	1.0411	0.2346	0.5651	1.8488
		(1.5061)	(0.9354)	(0.1922)	(0.7651)	(1.4429)
Bin 30-60	2.5892*	1.0485	-0.5684	.	-0.1308	-1.6332
	(1.4720)	(4.0302)	(1.2365)	.	(0.5088)	(1.4432)
L1	-0.3330	-1.1009	0.5991	.	2.0071***	-0.4729
	(1.8332)	(3.5521)	(0.9766)	.	(0.4423)	(0.9620)
L2	-0.3497	2.1415	0.6480	0.1000	0.4879	2.3000
	(2.3612)	(1.3421)	(0.8597)	(0.2063)	(0.7416)	(1.4315)
Bin 60-90	0.9702	1.0278	-0.7359	.	0.6895	-1.8445
	(1.2026)	(4.1543)	(1.1476)	.	(0.5095)	(1.2976)
L1	0.0173	-1.9101	0.4905	.	1.7684***	-0.2115
	(1.7430)	(3.5984)	(0.9579)	.	(0.3661)	(0.8466)
L2	-3.5552***	1.8272	0.2838	0.3520	0.5589	2.3909*
	(1.3099)	(1.3176)	(0.8649)	(0.2651)	(0.7209)	(1.3078)
Bin 90-120	0.2112	0.4016	-0.3448	.	-0.0692	-1.6055
	(1.0858)	(4.1288)	(1.1083)	.	(0.6712)	(1.0471)
L1	-0.2581	-2.0848	0.0508	.	1.5581***	-0.1211
	(1.5262)	(3.6587)	(0.9087)	.	(0.4152)	(0.7132)
L2	-1.6348*	-0.7539	-0.3135	0.4578*	0.2593	1.8428*
	(0.9218)	(1.5653)	(0.8372)	(0.2581)	(0.7056)	(1.0358)
Bin 120-150	-0.1900	-0.3170	-0.2818	.	-0.2568	-0.9550
	(0.9700)	(3.9477)	(0.9949)	.	(0.5582)	(0.7957)
L1	-0.0057	(n/t)	-0.1289	.	0.6697*	-0.0723
	(1.7753)		(1.0191)	.	(0.4035)	(0.4017)
L2	-2.6690***	0.0876	-0.1964	0.6558***	0.2246	0.9090
	(0.7534)	(1.4308)	(0.7661)	(0.2491)	(1.0475)	(0.7747)
Observations	124	155	201	275	304	346
R-squared	0.3105	0.1143	0.2809	0.0329	0.2449	0.0968

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

Table 2.7: Largest local mortality increases and distance to war

Town (year)	(1) Mortality increase	(2) Nearest war event	(3) Nearest war event in previous year
		Largest local mortality increases	
Termunten (1686)	31.86	.	.
Sint-Truiden, Sint Nicolaas (1665)	28.00	145.66	295.13
Gijverinkhove (1647)	27.03	118.55	10.93
Sint-Truiden, Sint Nicolaas (1631)	26.00	123.62	120,92
Fenain (1676)	24.00	9.30	111.44
Walem (1636)	21.33	124.62	27.19
Gierle (1636)	20.72	88.23	31.64
Minderhout (1625)	19.64	16.80	18.56
Schriek (1647)	17.50	121.88	29.87
Gentbrugge (1646)	17.38	4.85	4.85
	For localities within 30 kilometers of war event in same or previous year		
Gijverinkhove (1647)	27.03	118.55	10.93
Fenain (1676)	24.00	9.30	111.44
Walem (1636)	21.33	124.62	27.19
Minderhout (1625)	19.64	16.80	18.56
Schriek (1647)	17.50	121.88	29.87
Gentbrugge (1646)	17.38	4.85	4.85
Flostoy (1636)	15.73	101.40	8.66
Maastricht, Sint Nicolai (1633)	15.47	28.28	0.21
Rijsbergen (1625)	14.50	8.45	9.57
Kropswolde (1666)	13.77	111.90	20.81

Note: Localities occurring in both panels are italicized. Kropswolde (1666) in the lower panel is the seventeenth highest local mortality rate of all localities.
Sources: See Appendix B for a description of data and sources.

for instance, highlighted the exceptional severity and wide territorial spread of raised mortality associated with the plague of 1629-30 in Northern Italy, which was likely linked to war events (Hanlon 2014). Almost all of the mortality increases during this plague were more than 300 percent, with some more than 900 percent, and overall he cited a death rate of 30 to 35 percent during this epidemic (Alfani 2013a; 2013b). These figures for Northern Italy generally surpass death rates established elsewhere in Western Europe. Mortality rates of 25 to 30 percent are listed during epidemics in Southern Germany and some Swiss territories being the worst afflicted places during the Thirty Years' War (Eckert 1996). Mortality rates of around 20 percent are found for interior Spain and London during plague epidemics (Pérez Moreda 1980; Cummins, Kelly and Ó Gráda 2016), and around 10 percent for other parts of early modern England (Slack 1985). In the seventeenth-century Low Countries, substantial mortality spikes did exist but they were few and usually much more localized. And although there is a clear relation between warfare and mortality increases, the substantial mortality spikes did not always coincide with war events.

2.5   Conclusion

In this paper, we have used a large amount of detailed, new data to empirically study the connection between warfare and civilian mortality in the seventeenth-century Low Countries. There are three main findings. First, the mortality effect of war is substantial, suggesting on average a 30 to 43 percent increase in mortality for localities within a 30 kilometers of a war event. The mortality effect is seen further away too, as localities as far as 90 to 120 kilometers from a war event still experienced an increase over trend mortality of about 10 percent. Lagged mortality effects are large and strong, but only for localities within 60 kilometers of a war event in the previous year. The magnitude of that effect is as sizable as the contemporaneous effect within 30 kilometers, and even surpasses the contemporaneous effect for the 30 to 60 kilometers distance. Accordingly, war events seem to have generated a 'rippling' mortality effect, and although indirect evidence, this is equivalent to what one would expect from the spread of epidemic-driven mortality. Second, the mortality effects of warfare were felt in both city and countryside, in both contemporaneous and one-year lagged mortality effects, with no clear discernible urban-rural differences. Third, most of the individual war episodes led to raised mortality, though the size, strength and urban-rural differences of the effect varied across wars.

The first broader implication of these findings for the relation between warfare, demography and economic development relates to urban-rural differences in mortality

outcomes. Warfare has been associated with mortality in particular for the cities mostly driven by epidemic disease (Voigtländer and Voth 2013). We find that war-related mortality effects—both contemporaneous and lagged effects—are at least as high in rural localities. That findings is perhaps not surprising because rural localities were particularly vulnerable to soldiers due to a lack of defenses, which is the main argument for war-induced rural-to-urban flight (Rosenthal and Wong 2011; Dincecco and Onorato 2016; 2017). Furthermore, recent work has shown that diseases such as plague were not an exclusively urban phenomenon in Northwest Europe (Curtis 2016b). Note, however, that both diseases and rural-to-urban migration should result in a higher urban impact of warfare in our measure—recall that our calculations of deviation from mortality implicitly absorb immigration. Thus, our results imply that as wars pushed up rural mortality up at least as much as urban mortality, wars may have hampered the rural surplus needed to replenish cities—much in the same way as described for Northern Italy in 1629-30 (Alfani 2013a). Accordingly, our results do not support a war-induced escape from Malthusian pressures along two lines—war-induced mortality was not higher in the cities while the substantial rural effect may have hindered rural-to-urban migration. That is not to say that (a) urban mortality was not high in 'normal' times, or to (b) dismiss a more general migration-mortality link in the seventeenth-century Low Countries—we simply argue that this had little to do with wars per se.

A second broader economic implication is that in some places such as the Brabant villages of Hoge Mierde, Lage Mierde and Hulsel, mentioned in the introduction, high mortality went hand-in-hand with direct military activity, and thus destruction of capital and infrastructure and disruption of trade, besides killing people. However, other places around the same time as this war event, or in the aftermath of this war event, also experienced raised mortality while being some distance from the destructive effects on capital goods or infrastructure. Accordingly, wars in the seventeenth-century Low Countries could create economic redistribution among people through demographic channels, but this economic redistribution was itself spatially complex. Labor-capital or people-resources realignments did not occur equally over regions, and this may have moved regional economies in different directions. This is in contrast to some of the other channels through which early modern warfare is linked to economic development. War-induced state capacity, for instance, could be expected to operate at an aggregate level—although the effects of historical changes in state capacity on economic development could be manifold too (O'Brien 2018). The direct consequences of early modern warfare were not simply a matter of destroying rural capital and infrastructure and killing urban citizens.

A third and final point to make is that although we show a strong connection

between warfare and mortality in the seventeenth-century Low Countries, with epidemic disease as a plausible causal mechanism, the scale and impact of war-related civilian mortality was, on average, relatively minor with the exception of some particular places and years. Alfani has shown exceptional mortality increases afflicting a large amount of localities over a wide area all at the same time in Northern Italy during the plague of 1629-30 (2013a) that led to an aggregate demographic contraction with lasting economic consequences. In contrast, exceptionally high death rates in the Low Countries (a) occurred at different localities in various different years—Table 2.7 has entries from eight different years—and (b) despite the wide mortality spread, many local mortality increases were relatively weak. Hence, with such a wide diversity and variability in mortality outcomes, even from the same individual war event, it is difficult to argue for one universally applicable framework for understanding economic change or development in the aftermath of wars, based on the channel of civilian mortality.

Appendices

A   Additional results and robustness checks
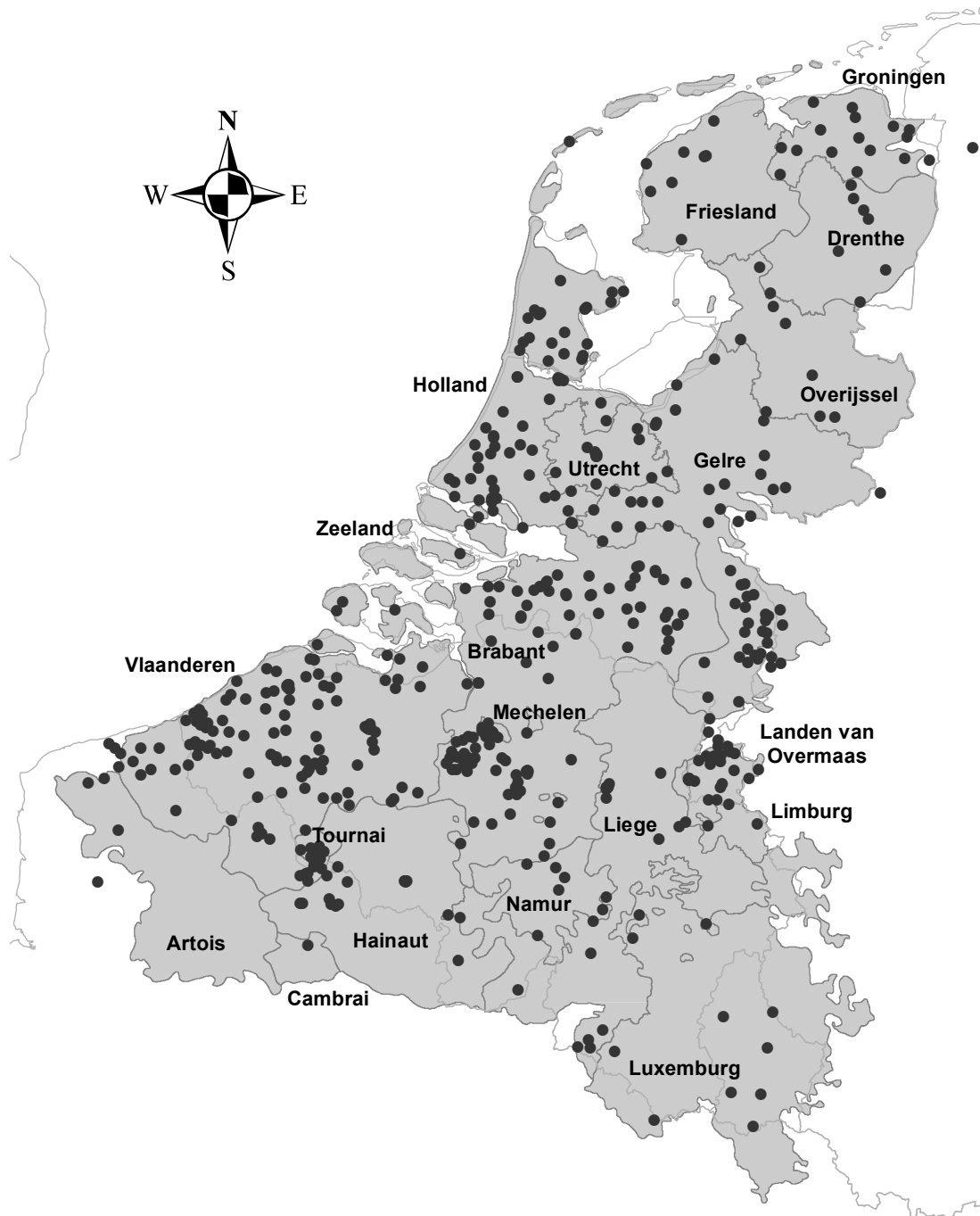
Figure 2A.1: Spatial distribution of the burial records—full size map

Sources: The shapefiles underlying this map were provided by Iason Jongepier, GIStorical Antwerp (UAntwerpen/Hercules Foundation). The locations are based on the burial records. See Appendix B for

a description of data and sources.
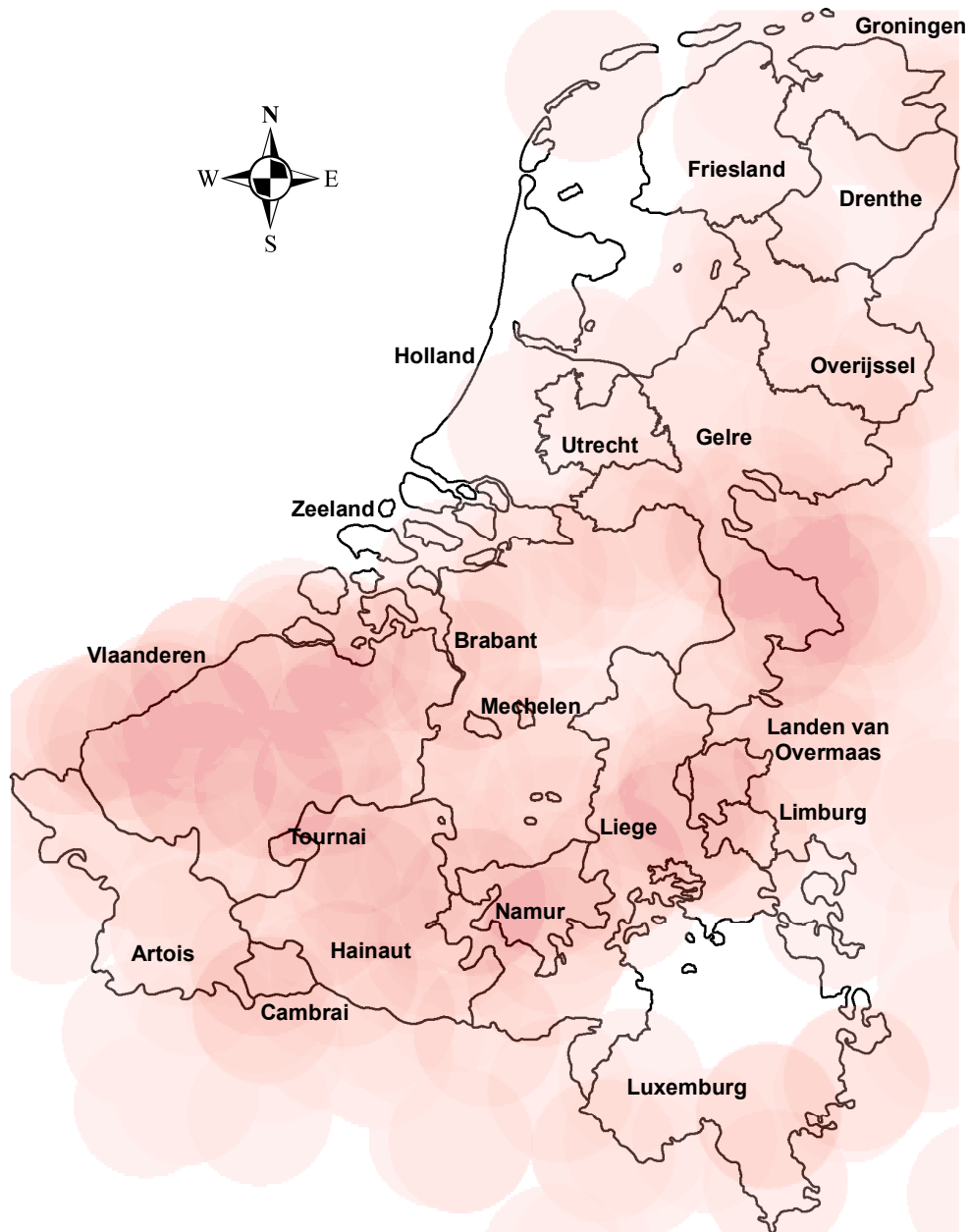
Figure 2A.2: Spatial distribution of the burial records—full size map

Sources: The shapefiles underlying this map were provided by Iason Jongepier, GIStorical Antwerp (UAntwerpen/Hercules Foundation). The locations are based on the war events. See Appendix B for a description of data and sources.

Figure 2A.3: Years of coverage per locality, urban and rural

Note: Small, Medium and Large refer to localities with 2,000 to 4,999; 5,000 to 9,999; and 10,000 and more inhabitants.

Sources: See Appendix B for a description of data and sources.

Coverage per region
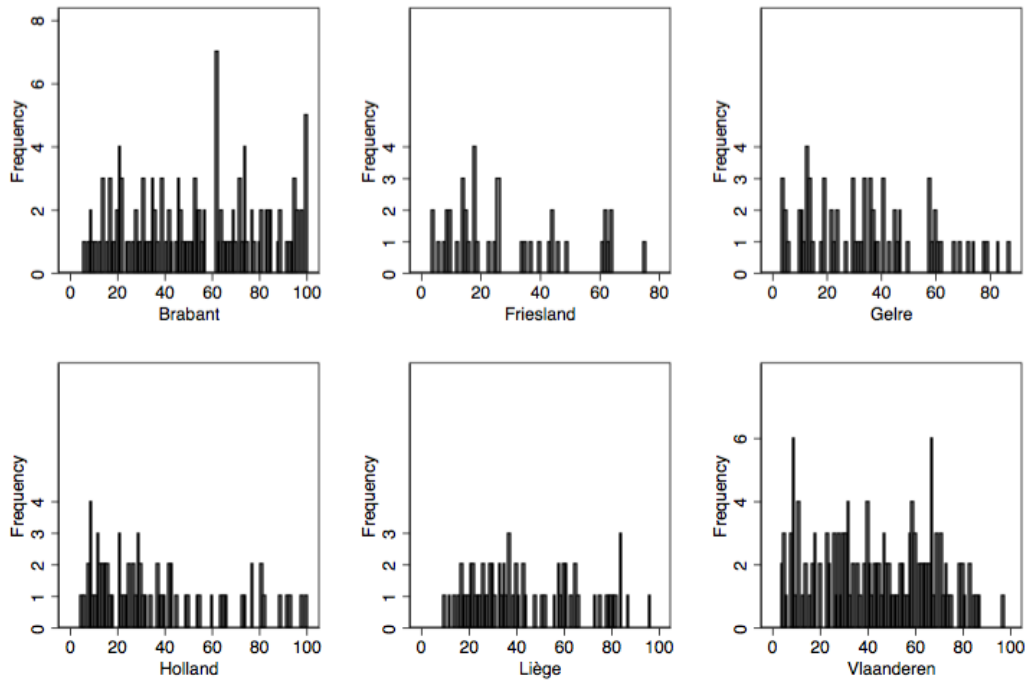
Figure 2A.4: Years of coverage per locality per region

Note: Small, Medium and Large refer to localities with 2,000 to 4,999; 5,000 to 9,999; and 10,000 and more inhabitants.
Sources: See Appendix B for a description of data and sources.
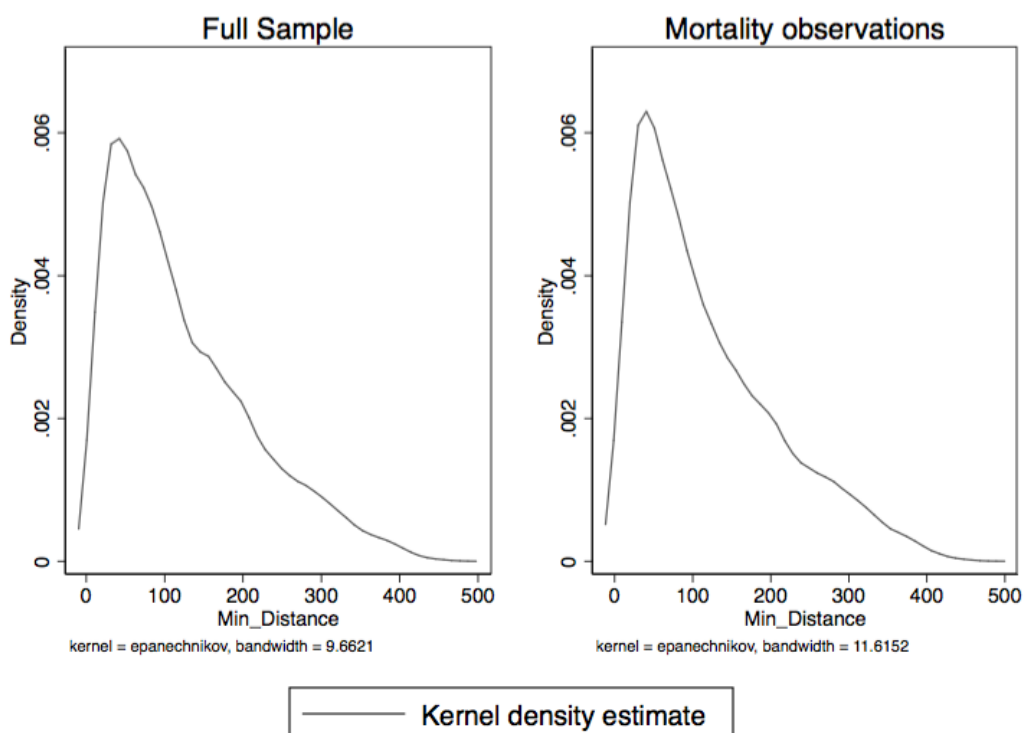
Figure 2A.5: Density of distance from burials locations to war locations

Note: The left panel shows the density of distance between all locality-year pairs and its distance to the nearest war-event. The right panel shows this only for those localities for which we have mortality data for that same year. *Sources*: authors' own calculation. See Appendix B for a description of data and sources.

Table 2A.1: Aggregate mortality effect per war—including first 15 years

Increase over, trend mortality	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
War	0.1039***	0.1077***						
	(0.0347)	(0.0401)						
* Urban (>5000)		-0.0142						
		(0.0348)						
War (alt)			0.1066*	0.0952				
			(0.0608)	(0.0674)				
* Urban (>5000)				0.0419				
				(0.0350)				
War (60)					0.2006***	0.2152***		
					(0.0495)	(0.0579)		
* Urban (>5000)						-0.0584		
						(0.0436)		
War (120)							0.1571***	0.1775***
							(0.0471)	(0.0539)
* Urban (>5000)								-0.0764*
								(0.0402)
Observations	20,143	20,143	20,143	20,143	20,143	20,143	20,143	20,143
Number of groups	511	511	511	511	511	511	511	511
Town FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	N	N	N	N	N	N	N	N
D-C	Y	Y	Y	Y	Y	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

Table 2A.2: Aggregate mortality effect per war (60)—including first 15 years

Increase over trend mortality	(1)	(2)	(3)	(4)
Revolt (60)	0.0198	-0.0086	0.0224	-0.0055
	(0.0602)	(0.0939)	(0.0602)	(0.0937)
Thirty Years-Ext (60)	0.2172***	0.2268**		
	(0.0796)	(0.0944)		
Thirty Years-Short (60)			0.3362**	0.3498**
			(0.1353)	(0.1728)
Thirty Years-French (60)			0.1563*	0.1624*
			(0.0836)	(0.0972)
Anglo, Munster, Devolution (60)	0.2650***	0.2489***	0.2626***	0.2464***
	(0.0413)	(0.0492)	(0.0405)	(0.0481)
Franco-Dutch (60)	0.2591**	0.3028*	0.2577**	0.3011*
	(0.1289)	(0.1554)	(0.1286)	(0.1550)
Reunions (60)	0.1318**	0.1077*	0.1283*	0.1038*
	(0.0661)	(0.0569)	(0.0662)	(0.0570)
Nine Years (60)	0.1529	0.1627	0.1506	0.1600
	(0.0995)	(0.1060)	(0.0996)	(0.1060)
Revolt * Urban (>5000)		0.0561		0.0551
		(0.1135)		(0.1131)
Thirty Years-Ext * Urban (>5000)		-0.0394		
		(0.0713)		
Thirty Years-Short * Urban (>5000)				-0.0589
				(0.1703)
Thirty Years-French * Urban (>5000)				-0.0245
				(0.0633)
Anglo, Munster, Devolution * Urban (>5000)		0.0707		0.0715
		(0.0477)		(0.0466)
Franco-Dutch * Urban (>5000)		-0.1755		-0.1742
		(0.1160)		(0.1158)
Reunions * Urban (>5000)		0.1302*		0.1317**
		(0.0660)		(0.0657)
Nine Years * Urban (>5000)		-0.0379		-0.0362
		(0.0546)		(0.0545)
Observations	20,143	20,143	20,143	20,143
Number of groups	511	511	511	511
Town FE	Y	Y	Y	Y
Year FE	N	N	N	N
D-C	Y	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

Table 2A.3: Aggregate mortality effect per war (120)—including first 15 years

Increase over trend mortality	(1)	(2)	(3)	(4)
Revolt (120)	0.1485***	0.2031**	0.1515***	0.2062**
	(0.0478)	(0.0854)	(0.0481)	(0.0857)
Thirty Years-Ext (120)	0.1618**	0.1764**		
	(0.0659)	(0.0734)		
Thirty Years-Short (120)			0.2132**	0.2175*
			(0.0938)	(0.1178)
Thirty Years-French (120)			0.1385*	0.1584*
			(0.0787)	(0.0885)
Anglo, Munster, Devolution (120)	0.1775***	0.2029***	0.1769***	0.2025***
	(0.0541)	(0.0584)	(0.0537)	(0.0582)
Franco-Dutch (120)	0.2190*	0.2632*	0.2182*	0.2625*
	(0.1256)	(0.1507)	(0.1255)	(0.1507)
Reunions (120)	0.0860	0.0848	0.0842	0.0834
	(0.0605)	(0.0575)	(0.0606)	(0.0576)
Nine Years (120)	0.1118	0.1262	0.1106	0.1252
	(0.0859)	(0.0883)	(0.0858)	(0.0882)
Revolt * Urban (>5000)		-0.1331		-0.1334
		(0.1176)		(0.1176)
Thirty Years-Ext * Urban (>5000)		-0.0537		
		(0.0428)		
Thirty Years-Short * Urban (>5000)				-0.0191
				(0.1001)
Thirty Years-French * Urban (>5000)				-0.0725
				(0.0455)
Anglo, Munster, Devolution * Urban (>5000)		-0.0927**		-0.0933**
		(0.0379)		(0.0378)
Franco-Dutch * Urban (>5000)		-0.1850		-0.1856
		(0.1190)		(0.1193)
Reunions * Urban (>5000)		0.0139		0.0131
		(0.0342)		(0.0342)
Nine Years * Urban (>5000)		-0.0566		-0.0569
		(0.0420)		(0.0419)
Observations	20,143	20,143	20,143	20,143
Number of groups	511	511	511	511
Town FE	Y	Y	Y	Y
Year FE	N	N	N	N
D-C	Y	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

Table 2A.4: Aggregate mortality effect per war (50)—excluding first 15 years

Increase over trend mortality	(1)	(2)	(3)	(4)
Thirty Years-Ext (50)	0.2955***	0.3035***		
	(0.0866)	(0.1026)		
Thirty Years-Short (50)			0.4206***	0.4379**
			(0.1284)	(0.1715)
Thirty Years-French (50)			0.2320**	0.2343**
			(0.0969)	(0.1111)
Anglo, Munster, Devolution (50)	0.2705***	0.2468***	0.2684***	0.2444***
	(0.0344)	(0.0455)	(0.0338)	(0.0446)
Franco-Dutch (50)	0.3062**	0.3668**	0.3051**	0.3654**
	(0.1363)	(0.1623)	(0.1361)	(0.1620)
Reunions (50)	0.1351*	0.1244*	0.1319*	0.1207*
	(0.0746)	(0.0670)	(0.0748)	(0.0672)
Nine Years (50)	0.1743	0.1880	0.1724	0.1857
	(0.1060)	(0.1136)	(0.1061)	(0.1136)
Thirty Years-Ext * Urban (>5000)		-0.0340		
		(0.0909)		
Thirty Years-Short * Urban (>5000)				-0.0736
				(0.2172)
Thirty Years-French * Urban (>5000)				-0.0108
				(0.0765)
Anglo, Munster, Devolution * Urban (>5000)		0.0977		0.0985
		(0.0636)		(0.0626)
Franco-Dutch * Urban (>5000)		-0.2444**		-0.2432**
		(0.1133)		(0.1131)
Reunions * Urban (>5000)		0.0643		0.0664
		(0.0587)		(0.0583)
Nine Years * Urban (>5000)		-0.0530		-0.0513
		(0.0614)		(0.0614)
Observations	19,460	19,460	19,460	19,460
Number of groups	506	506	506	506
Town FE	Y	Y	Y	Y
Year FE	N	N	N	N
D-C	Y	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

Table 2A.5: Aggregate mortality effect per war (100)—excluding first 15 years

Increase over trend mortality	(1)	(2)	(3)	(4)
Thirty Years-Ext (100)	0.1368**	0.1588**		
	(0.0592)	(0.0692)		
Thirty Years-Short (100)			0.2157*	0.2213
			(0.1110)	(0.1358)
Thirty Years-French (100)			0.0994*	0.1299*
			(0.0585)	(0.0746)
Anglo, Munster, Devolution (100)	0.1611**	0.1743***	0.1597**	0.1732***
	(0.0642)	(0.0624)	(0.0635)	(0.0617)
Franco-Dutch (100)	0.2103	0.2546	0.2092	0.2536
	(0.1295)	(0.1553)	(0.1293)	(0.1552)
Reunions (100)	0.0764	0.0789	0.0738	0.0766
	(0.0647)	(0.0605)	(0.0646)	(0.0604)
Nine Years (100)	0.1052	0.1211	0.1034	0.1196
	(0.0918)	(0.0940)	(0.0917)	(0.0939)
Thirty Years-Ext * Urban (>5000)		-0.0819		
		(0.0496)		
Thirty Years-Short * Urban (>5000)				-0.0249
				(0.0981)
Thirty Years-French * Urban (>5000)				-0.1132*
				(0.0672)
Anglo, Munster, Devolution * Urban (>5000)		-0.0501		-0.0515
		(0.0314)		(0.0315)
Franco-Dutch * Urban (>5000)		-0.1863		-0.1865
		(0.1229)		(0.1234)
Reunions * Urban (>5000)		-0.0053		-0.0063
		(0.0414)		(0.0418)
Nine Years * Urban (>5000)		-0.0651		-0.0654
		(0.0433)		(0.0435)
Observations	19,460	19,460	19,460	19,460
Number of groups	506	506	506	506
Town FE	Y	Y	Y	Y
Year FE	N	N	N	N
D-C	Y	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

Table 2A.6: Spatial and temporal variation in local mortality—full sample

Increase over trend mortality	(1)	(2)	(3)
Bin 0-30	0.4233***	0.3133***	0.3284***
	(0.0928)	(0.0778)	(0.0806)
L1		0.3157***	0.3374***
		(0.0881)	(0.0860)
L2			-0.1057*
			(0.0632)
Bin 30-60	0.1531**	0.0763	0.0891*
	(0.0624)	(0.0519)	(0.0535)
L1		0.1720***	0.1843***
		(0.0642)	(0.0658)
L2			-0.0353
			(0.0612)
Bin 60-90	0.1047*	0.0482	0.0632
	(0.0580)	(0.0473)	(0.0511)
L1		0.0968	0.1063
		(0.0742)	(0.0742)
L2			-0.0544
			(0.0688)
Bin 90-120	0.1242**	0.0810	0.0971*
	(0.0588)	(0.0508)	(0.0513)
L1		0.0713	0.0815
		(0.0625)	(0.0657)
L2			-0.0846
			(0.0516)
Bin 120-150	0.0345	0.0052	0.0158
	(0.0547)	(0.0489)	(0.0484)
L1		0.0101	0.0174
		(0.0406)	(0.0421)
L2			-0.0444
			(0.0338)
Bin 0-30 * Urban (2000)	-0.0190	-0.0335	-0.0261
	(0.0830)	(0.0871)	(0.0847)
L1		0.0207	0.0235
		(0.0891)	(0.0891)
L2			0.0144
			(0.0878)
Bin 30-60 * Urban (2000)	0.0739	0.0607	0.0706
	(0.0862)	(0.0857)	(0.0853)
L1		0.0500	0.0675
		(0.0561)	(0.0571)
L2			-0.1023
			(0.0974)
Bin 60-90 * Urban (2000)	-0.0315	-0.0460	-0.0366
	(0.0612)	(0.0620)	(0.0631)
L1		0.0350	0.0512
		(0.0685)	(0.0724)
L2			-0.0035
			(0.0764)
Bin 90-120 * Urban (2000)	0.0051	-0.0091	0.0075
	(0.0988)	(0.1038)	(0.1056)
L1		-0.0205	-0.0044
		(0.0797)	(0.0879)
L2			-0.0892
			(0.0869)
Bin 120-150 * Urban (2000)	-0.0016	-0.0305	-0.0257
	(0.0535)	(0.0626)	(0.0647)
L1		0.0121	0.0303
		(0.0576)	(0.0626)
L2			-0.0836
			(0.0722)

(Table continues on next page)

Bin 0-30 * Urban (5000)	0.0085	0.0002	0.0184
	(0.1720)	(0.1513)	(0.1537)
L1		0.1093	0.1828
		(0.1727)	(0.1859)
L2			-0.2348*
			(0.1293)
Bin 30-60 * Urban (5000)	-0.0766	-0.0656	-0.0439
	(0.0926)	(0.0992)	(0.1054)
L1		-0.1056	-0.0637
		(0.0990)	(0.1058)
L2			-0.0996
			(0.1270)
Bin 60-90 * Urban (5000)	-0.1000	-0.0595	-0.0606
	(0.0708)	(0.0653)	(0.0663)
L1		-0.1253	-0.1152
		(0.0970)	(0.0997)
L2			0.0075
			(0.0914)
Bin 90-120 * Urban (5000)	0.1445	0.1860	0.1612
	(0.1353)	(0.1541)	(0.1425)
L1		-0.2264	-0.2127
		(0.1595)	(0.1712)
L2			0.0844
			(0.2571)
Bin 120-150 * Urban (5000)	0.4688*	0.5074*	0.4996*
	(0.2768)	(0.2862)	(0.2740)
L1		0.1030	0.0972
		(0.1952)	(0.1832)
L2			-0.0636
			(0.1569)
Bin 0-30 * Urban (10000)	-0.0735	-0.0433	-0.0322
	(0.0838)	(0.0765)	(0.0754)
L1		-0.0885	-0.0411
		(0.0883)	(0.1080)
L2			-0.1348*
			(0.0803)
Bin 30-60 * Urban (10000)	-0.0376	-0.0205	-0.0030
	(0.0447)	(0.0386)	(0.0385)
L1		-0.0245	0.0227
		(0.0424)	(0.0558)
L2			-0.1059
			(0.0867)
Bin 60-90 * Urban (10000)	-0.0564	-0.0377	-0.0147
	(0.0456)	(0.0433)	(0.0453)
L1		-0.0441	-0.0064
		(0.0452)	(0.0488)
L2			-0.0752
			(0.0680)
Bin 90-120 * Urban (10000)	-0.1269**	-0.1072**	-0.0907*
	(0.0485)	(0.0482)	(0.0513)
L1		-0.0551	-0.0254
		(0.0480)	(0.0574)
L2			-0.1171*
			(0.0671)
Bin 120-150 * Urban (10000)	-0.0277	-0.0136	-0.0113
	(0.0733)	(0.0691)	(0.0648)
L1		-0.0384	-0.0017
		(0.0401)	(0.0405)
L2			-0.0339
			(0.0517)
Observations	20,143	20,143	20,143
Number of groups	511	511	511
Town FE	Y	Y	Y
Year FE	Y	Y	Y
D-C	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

Table 2A.7: Spatial and temporal variation in local mortality—excluding localities without population estimates and first 15 years

Increase over trend mortality	(1)	(2)	(3)
Bin 0-30	0.4947***	0.3797***	0.3942***
	(0.1280)	(0.1166)	(0.1216)
L1		0.3592***	0.3804***
		(0.0949)	(0.0997)
L2			-0.1299*
			(0.0753)
Bin 30-60	0.1245	0.0394	0.0523
	(0.0786)	(0.0719)	(0.0749)
L1		0.1722**	0.1809**
		(0.0668)	(0.0696)
L2			-0.0260
			(0.0790)
Bin 60-90	0.0661	0.0028	0.0211
	(0.0681)	(0.0584)	(0.0632)
L1		0.0957	0.1048
		(0.0856)	(0.0850)
L2			-0.0627
			(0.0839)
Bin 90-120	0.0818	0.0330	0.0515
	(0.0712)	(0.0679)	(0.0696)
L1		0.0939	0.1041
		(0.0817)	(0.0833)
L2			-0.1009
			(0.0748)
Bin 120-150	0.0502	0.0220	0.0355
	(0.0727)	(0.0687)	(0.0660)
L1		-0.0539	-0.0489
		(0.0501)	(0.0500)
L2			-0.0644
			(0.0467)
Bin 0-30 * Urban (2000)	-0.0883	-0.0931	-0.0846
	(0.1047)	(0.1080)	(0.1069)
L1		-0.0338	-0.0355
		(0.0959)	(0.1030)
L2			0.0461
			(0.1022)
Bin 30-60 * Urban (2000)	0.1120	0.1113	0.1217
	(0.1022)	(0.1019)	(0.1002)
L1		0.0382	0.0559
		(0.0577)	(0.0616)
L2			-0.1122
			(0.1080)
Bin 60-90 * Urban (2000)	0.0006	-0.0051	0.0007
	(0.0683)	(0.0686)	(0.0673)
L1		0.0036	0.0186
		(0.0703)	(0.0771)
L2			0.0195
			(0.0861)
Bin 90-120 * Urban (2000)	0.0595	0.0601	0.0767
	(0.0939)	(0.0981)	(0.1040)
L1		-0.0627	-0.0490
		(0.0820)	(0.0859)
L2			-0.0826
			(0.0959)
Bin 120-150 * Urban (2000)	-0.0271	-0.0542	-0.0498
	(0.0690)	(0.0751)	(0.0790)
L1		0.0352	0.0544
		(0.0538)	(0.0577)
L2			-0.0856
			(0.0672)

(Table continues on next page)

Bin 0-30 * Urban (5000)	-0.0558	-0.0509	-0.0345
	(0.1846)	(0.1719)	(0.1718)
L1		0.0276	0.0920
		(0.1775)	(0.1937)
L2			-0.1816
			(0.1321)
Bin 30-60 * Urban (5000)	-0.0608	-0.0410	-0.0181
	(0.0949)	(0.1035)	(0.1077)
L1		-0.1171	-0.0774
		(0.1023)	(0.1078)
L2			-0.1041
			(0.1309)
Bin 60-90 * Urban (5000)	-0.0998	-0.0491	-0.0468
	(0.0742)	(0.0698)	(0.0730)
L1		-0.1276	-0.1204
		(0.0954)	(0.0946)
L2			-0.0004
			(0.0905)
Bin 90-120 * Urban (5000)	0.1698	0.2159	0.1942
	(0.1438)	(0.1616)	(0.1531)
L1		-0.2472	-0.2359
		(0.1557)	(0.1613)
L2			0.0859
			(0.2470)
Bin 120-150 * Urban (5000)	0.4355	0.4688	0.4633
	(0.2944)	(0.3028)	(0.2936)
L1		0.1613	0.1575
		(0.2074)	(0.1964)
L2			-0.0439
			(0.1646)
Bin 0-30 * Urban (10000)	-0.1470	-0.1065	-0.0945
	(0.1156)	(0.1015)	(0.1021)
L1		-0.1545	-0.1131
		(0.1114)	(0.1416)
L2			-0.1053
			(0.1037)
Bin 30-60 * Urban (10000)	-0.0135	0.0137	0.0319
	(0.0590)	(0.0536)	(0.0514)
L1		-0.0266	0.0166
		(0.0475)	(0.0678)
L2			-0.1035
			(0.1184)
Bin 60-90 * Urban (10000)	-0.0389	-0.0139	0.0072
	(0.0554)	(0.0523)	(0.0548)
L1		-0.0592	-0.0248
		(0.0544)	(0.0601)
L2			-0.0688
			(0.0890)
Bin 90-120 * Urban (10000)	-0.1094	-0.0806	-0.0633
	(0.0755)	(0.0760)	(0.0804)
L1		-0.0766	-0.0494
		(0.0619)	(0.0668)
L2			-0.0936
			(0.0937)
Bin 120-150 * Urban (10000)	-0.0470	-0.0334	-0.0334
	(0.0975)	(0.0938)	(0.0881)
L1		0.0175	0.0541
		(0.0460)	(0.0449)
L2			-0.0209
			(0.0616)
Observations	13,664	13,664	13,664
Number of groups	338	338	338
Town FE	Y	Y	Y
Year FE	Y	Y	Y
D-C	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

Table 2A.8: Spatial and temporal variation in local mortality—time-region FE, excluding first 15 years

Increase over trend mortality	(1)	(2)	(3)
Bin 0-30	0.4202***	0.3293***	0.3420***
	(0.1124)	(0.1055)	(0.1107)
L1		0.3237***	0.3343***
		(0.0911)	(0.0938)
L2			-0.0747
			(0.0753)
Bin 30-60	0.1729**	0.1064	0.1189*
	(0.0681)	(0.0651)	(0.0695)
L1		0.1925***	0.1989***
		(0.0722)	(0.0735)
L2			-0.0427
			(0.0688)
Bin 60-90	0.1055*	0.0609	0.0714
	(0.0600)	(0.0588)	(0.0597)
L1		0.1123	0.1152
		(0.0762)	(0.0759)
L2			-0.0321
			(0.0580)
Bin 90-120	0.1126**	0.0794*	0.0912**
	(0.0430)	(0.0445)	(0.0431)
L1		0.1147	0.1174
		(0.0764)	(0.0769)
L2			-0.0741
			(0.0559)
Bin 120-150	0.0091	-0.0133	-0.0084
	(0.0330)	(0.0344)	(0.0335)
L1		0.0395	0.0412
		(0.0482)	(0.0472)
L2			-0.0231
			(0.0351)
Bin 0-30 * Urban (2000)	-0.0489	-0.0567	-0.0506
	(0.0929)	(0.0932)	(0.0907)
L1		0.0045	0.0031
		(0.0932)	(0.0917)
L2			0.0410
			(0.0820)
Bin 30-60 * Urban (2000)	0.0288	0.0182	0.0306
	(0.0725)	(0.0742)	(0.0755)
L1		0.0161	0.0319
		(0.0441)	(0.0453)
L2			-0.0934
			(0.0753)
Bin 60-90 * Urban (2000)	0.0196	0.0097	0.0209
	(0.0794)	(0.0779)	(0.0772)
L1		0.0476	0.0651
		(0.0592)	(0.0578)
L2			-0.0338
			(0.0615)
Bin 90-120 * Urban (2000)	0.0146	0.0115	0.0264
	(0.0885)	(0.0924)	(0.0956)
L1		0.0021	0.0150
		(0.0727)	(0.0780)
L2			-0.0561
			(0.0694)
Bin 120-150 * Urban (2000)	-0.0207	-0.0344	-0.0243
	(0.0487)	(0.0552)	(0.0572)
L1		0.0080	0.0202
		(0.0556)	(0.0584)
L2			-0.1095
			(0.0818)

(Table continues on next page)

Bin 0-30 * Urban (5000)	-0.0587	-0.0561	-0.0504
	(0.2046)	(0.1698)	(0.1673)
L1		0.0807	0.1342
		(0.1491)	(0.1578)
L2			-0.1780
			(0.1184)
Bin 30-60 * Urban (5000)	-0.0508	-0.0355	-0.0125
	(0.1096)	(0.1039)	(0.1076)
L1		-0.0977	-0.0611
		(0.0878)	(0.0924)
L2			-0.0620
			(0.1218)
Bin 60-90 * Urban (5000)	-0.0781	-0.0469	-0.0388
	(0.0853)	(0.0713)	(0.0727)
L1		-0.0989	-0.0758
		(0.1174)	(0.1119)
L2			-0.1036
			(0.0977)
Bin 90-120 * Urban (5000)	0.0764	0.1224	0.1166
	(0.1069)	(0.1240)	(0.1209)
L1		-0.2414	-0.2092
		(0.1525)	(0.1541)
L2			0.0348
			(0.2179)
Bin 120-150 * Urban (5000)	0.4278	0.4639	0.4629
	(0.2768)	(0.2851)	(0.2808)
L1		0.0927	0.0952
		(0.1937)	(0.1886)
L2			-0.1331
			(0.1831)
Bin 0-30 * Urban (10000)	-0.0656	-0.0265	-0.0121
	(0.0920)	(0.0838)	(0.0818)
L1		-0.1113	-0.0721
		(0.0990)	(0.1189)
L2			-0.1314
			(0.0807)
Bin 30-60 * Urban (10000)	-0.0712	-0.0467	-0.0273
	(0.0562)	(0.0479)	(0.0446)
L1		-0.0415	-0.0078
		(0.0460)	(0.0573)
L2			-0.0868
			(0.0892)
Bin 60-90 * Urban (10000)	-0.0452	-0.0237	-0.0075
	(0.0464)	(0.0423)	(0.0490)
L1		-0.0514	-0.0275
		(0.0474)	(0.0480)
L2			-0.0400
			(0.0738)
Bin 90-120 * Urban (10000)	-0.1260**	-0.0992*	-0.0890*
	(0.0544)	(0.0519)	(0.0488)
L1		-0.0802	-0.0623
		(0.0608)	(0.0728)
L2			-0.0741
			(0.0712)
Bin 120-150 * Urban (10000)	-0.0167	0.0083	0.0057
	(0.0803)	(0.0740)	(0.0709)
L1		-0.0456	-0.0213
		(0.0449)	(0.0449)
L2			-0.0356
			(0.0750)
Observations	19,460	19,460	19,460
Number of groups	506	506	506
Town FE	Y	Y	Y
Region*Year FE	Y	Y	Y
D-C	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

Table 2A.9: Spatial and temporal variation in local mortality—excluding Holland and first 15 years

Increase over trend mortality	(1)	(2)	(3)
Bin 0-30	0.4405***	0.3245***	0.3387***
	(0.0937)	(0.0757)	(0.0794)
L1		0.3348***	0.3513***
		(0.0915)	(0.0893)
L2			-0.0881
			(0.0704)
Bin 30-60	0.1723***	0.0901*	0.1019*
	(0.0638)	(0.0513)	(0.0535)
L1		0.1893***	0.1980***
		(0.0689)	(0.0701)
L2			-0.0271
			(0.0679)
Bin 60-90	0.1265**	0.0657	0.0773
	(0.0593)	(0.0476)	(0.0510)
L1		0.1099	0.1162
		(0.0771)	(0.0773)
L2			-0.0338
			(0.0737)
Bin 90-120	0.1361**	0.0911	0.1038*
	(0.0671)	(0.0590)	(0.0594)
L1		0.0892	0.0967
		(0.0654)	(0.0685)
L2			-0.0607
			(0.0526)
Bin 120-150	0.0614	0.0270	0.0350
	(0.0686)	(0.0605)	(0.0595)
L1		-0.0007	0.0072
		(0.0430)	(0.0451)
L2			-0.0416
			(0.0375)
Bin 0-30 * Urban (2000)	-0.0096	-0.0167	-0.0179
	(0.0873)	(0.0920)	(0.0917)
L1		0.0047	0.0017
		(0.0947)	(0.0925)
L2			0.0403
			(0.0868)
Bin 30-60 * Urban (2000)	0.0778	0.0753	0.0764
	(0.0882)	(0.0894)	(0.0884)
L1		0.0391	0.0487
		(0.0585)	(0.0559)
L2			-0.0682
			(0.1015)
Bin 60-90 * Urban (2000)	-0.0184	-0.0270	-0.0279
	(0.0671)	(0.0674)	(0.0668)
L1		0.0211	0.0303
		(0.0786)	(0.0838)
L2			0.0477
			(0.0857)
Bin 90-120 * Urban (2000)	0.0568	0.0584	0.0620
	(0.1046)	(0.1090)	(0.1137)
L1		-0.0871	-0.0830
		(0.0876)	(0.0957)
L2			-0.0309
			(0.0924)
Bin 120-150 * Urban (2000)	-0.0022	-0.0203	-0.0227
	(0.0568)	(0.0653)	(0.0692)
L1		0.0217	0.0316
		(0.0570)	(0.0593)
L2			-0.0985
			(0.0653)

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Bin 0-30 * Urban (5000)	-0.0217	-0.0119	0.0014
	(0.1840)	(0.1621)	(0.1658)
L1		0.0693	0.1186
		(0.1783)	(0.1966)
L2			-0.1778
			(0.1530)
Bin 30-60 * Urban (5000)	-0.0912	-0.0757	-0.0529
	(0.0876)	(0.1036)	(0.1109)
L1		-0.1256	-0.0946
		(0.1173)	(0.1218)
L2			-0.0574
			(0.1528)
Bin 60-90 * Urban (5000)	-0.1862**	-0.1436*	-0.1433*
	(0.0882)	(0.0849)	(0.0839)
L1		-0.1116	-0.1127
		(0.1206)	(0.1269)
L2			-0.0241
			(0.1196)
Bin 90-120 * Urban (5000)	0.1537	0.1946	0.1493
	(0.1708)	(0.1970)	(0.1799)
L1		-0.2287	-0.2288
		(0.2023)	(0.2192)
L2			0.1696
			(0.3143)
Bin 120-150 * Urban (5000)	0.5907	0.6214*	0.6007*
	(0.3655)	(0.3714)	(0.3601)
L1		0.1671	0.1540
		(0.2551)	(0.2425)
L2			-0.0296
			(0.2082)
Bin 0-30 * Urban (10000)	-0.0720	-0.0454	-0.0344
	(0.0926)	(0.0852)	(0.0850)
L1		-0.0791	-0.0400
		(0.0867)	(0.1054)
L2			-0.1205
			(0.0890)
Bin 30-60 * Urban (10000)	-0.0633	-0.0530	-0.0352
	(0.0515)	(0.0427)	(0.0439)
L1		-0.0088	0.0308
		(0.0446)	(0.0523)
L2			-0.0977
			(0.0841)
Bin 60-90 * Urban (10000)	-0.0769	-0.0641	-0.0453
	(0.0504)	(0.0452)	(0.0523)
L1		-0.0152	0.0126
		(0.0463)	(0.0487)
L2			-0.0425
			(0.0774)
Bin 90-120 * Urban (10000)	-0.1426**	-0.1330**	-0.1166*
	(0.0618)	(0.0642)	(0.0612)
L1		-0.0351	-0.0125
		(0.0616)	(0.0753)
L2			-0.1391*
			(0.0794)
Bin 120-150 * Urban (10000)	-0.0104	-0.0037	-0.0116
	(0.1060)	(0.1042)	(0.0996)
L1,		-0.0333	0.0042
		(0.0634)	(0.0618)
L2			-0.0209
			(0.0938)
Observations	17,138	17,138	17,138
Number of groups	437	437	437
Town FE	Y	Y	Y
Year FE	Y	Y	Y
D-C	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

Table 2A.10: Spatial and temporal variation in local mortality—trend including center point and at least 4 observations, excluding first 15 years

Increase over trend mortality	(1)	(2)	(3)
Bin 0-30	0.3361***	0.2507***	0.2642***
	(0.0729)	(0.0596)	(0.0620)
L1		0.2363***	0.2527***
		(0.0746)	(0.0715)
L2			-0.0762
			(0.0466)
Bin 30-60	0.1254***	0.0651*	0.0756**
	(0.0459)	(0.0357)	(0.0366)
L1		0.1317**	0.1417***
		(0.0508)	(0.0506)
L2			-0.0411
			(0.0431)
Bin 60-90	0.1104**	0.0654*	0.0747*
	(0.0467)	(0.0363)	(0.0376)
L1		0.0741	0.0805
		(0.0626)	(0.0632)
L2			-0.0180
			(0.0606)
Bin 90-120	0.0968**	0.0628	0.0731*
	(0.0463)	(0.0382)	(0.0386)
L1		0.0419	0.0498
		(0.0486)	(0.0510)
L2			-0.0585
			(0.0434)
Bin 120-150	0.0567	0.0332	0.0403
	(0.0472)	(0.0408)	(0.0389)
L1		0.0099	0.0172
		(0.0318)	(0.0328)
L2			-0.0371
			(0.0276)
Bin 0-30 * Urban (2000)	-0.0428	-0.0471	-0.0438
	(0.0649)	(0.0652)	(0.0641)
L1		0.0029	0.0035
		(0.0885)	(0.0843)
L2			0.0106
			(0.0668)
Bin 30-60 * Urban (2000)	0.0396	0.0338	0.0442
	(0.0598)	(0.0619)	(0.0635)
L1		0.0356	0.0475
		(0.0406)	(0.0407)
L2			-0.0676
			(0.0708)
Bin 60-90 * Urban (2000)	-0.0855	-0.0938*	-0.0833
	(0.0539)	(0.0531)	(0.0530)
L1		0.0178	0.0325
		(0.0550)	(0.0568)
L2			-0.0321
			(0.0510)
Bin 90-120 * Urban (2000)	-0.0034	-0.0104	0.0107
	(0.0741)	(0.0764)	(0.0785)
L1		-0.0065	0.0098
		(0.0617)	(0.0678)
L2			-0.1114*
			(0.0669)
Bin 120-150 * Urban (2000)	-0.0533	-0.0722	-0.0623
	(0.0456)	(0.0496)	(0.0537)
L1		0.0077	0.0207
		(0.0469)	(0.0493)
L2			-0.0695
			(0.0487)

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Bin 0-30 * Urban (5000)	-0.0503	-0.0616	-0.0658
	(0.1129)	(0.1015)	(0.1017)
L1		0.0524	0.0964
		(0.1007)	(0.1079)
L2			-0.1258
			(0.0781)
Bin 30-60 * Urban (5000)	-0.1307*	-0.1306**	-0.1303*
	(0.0672)	(0.0637)	(0.0665)
L1		-0.0449	-0.0345
		(0.0536)	(0.0650)
L2			-0.0037
			(0.0712)
Bin 60-90 * Urban (5000)	-0.1837***	-0.1549***	-0.1595***
	(0.0632)	(0.0527)	(0.0499)
L1		-0.0558	-0.0605
		(0.0542)	(0.0603)
L2			0.0751
			(0.0940)
Bin 90-120 * Urban (5000)	-0.0310	-0.0020	-0.0198
	(0.0683)	(0.0751)	(0.0760)
L!		-0.1204	-0.1252
		(0.0822)	(0.0894)
L2			0.0151
			(0.0856)
Bin 120-150 * Urban (5000)	0.0113	0.0404	0.0387
	(0.0848)	(0.0853)	(0.0791)
L1		-0.0138	-0.0189
		(0.0705)	(0.0704)
L2			0.0038
			(0.1050)
Bin 0-30 * Urban (10000)	-0.0013	0.0177	0.0231
	(0.0725)	(0.0710)	(0.0714)
L1		-0.0559	-0.0196
		(0.0835)	(0.0916)
L2			-0.0957
			(0.0607)
Bin 30-60 * Urban (10000)	-0.0274	-0.0170	-0.0056
	(0.0302)	(0.0258)	(0.0259)
L1		-0.0149	0.0221
		(0.0373)	(0.0430)
L2			-0.0777
			(0.0680)
Bin 60-90 * Urban (10000)	-0.0720*	-0.0609*	-0.0423
	(0.0376)	(0.0330)	(0.0325)
L1		-0.0215	0.0099
		(0.0426)	(0.0458)
L2			-0.0964*
			(0.0578)
Bin 90-120 * Urban (10000)	-0.1038**	-0.0905**	-0.0730*
	(0.0421)	(0.0392)	(0.0418)
L1		-0.0224	0.0025
		(0.0331)	(0.0387)
L2			-0.0863
			(0.0567)
Bin 120-150 * Urban (10000)	-0.0690	-0.0604	-0.0559
	(0.0672)	(0.0632)	(0.0603)
L1		-0.0284	-0.0010
		(0.0368)	(0.0337)
L2			-0.0439
			(0.0430)
Observations	20,993	20,993	20,993
Number of groups	515	515	515
Town FE	Y	Y	Y
Year FE	Y	Y	Y
D-C	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

Table 2A.11: Spatial and temporal variation in local mortality—trend including center point and at least 5 observations, excluding first 15 years

Increase over trend mortality	(1)	(2)	(3)
Bin 0-30	0.3055***	0.2258***	0.2403***
	(0.0717)	(0.0592)	(0.0616)
L1		0.2279***	0.2474***
		(0.0666)	(0.0658)
L2			-0.0889*
			(0.0461)
Bin 30-60	0.1054**	0.0500	0.0620
	(0.0461)	(0.0374)	(0.0386)
L1		0.1238**	0.1362***
		(0.0492)	(0.0512)
L2			-0.0411
			(0.0433)
Bin 60-90	0.0815*	0.0401	0.0522
	(0.0481)	(0.0389)	(0.0423)
L1		0.0743	0.0833
		(0.0635)	(0.0642)
L2			-0.0370
			(0.0535)
Bin 90-120	0.0869*	0.0558	0.0681*
	(0.0443)	(0.0384)	(0.0389)
L1		0.0432	0.0529
		(0.0493)	(0.0521)
L2			-0.0660
			(0.0406)
Bin 120-150	0.0321	0.0111	0.0186
	(0.0405)	(0.0362)	(0.0355)
L1		0.0092	0.0167
		(0.0319)	(0.0333)
L2			-0.0336
			(0.0262)
Bin 0-30 * Urban (2000)	-0.0041	-0.0173	-0.0126
	(0.0675)	(0.0679)	(0.0663)
L1		0.0274	0.0298
		(0.0746)	(0.0725)
L2			0.0117
			(0.0677)
Bin 30-60 * Urban (2000)	0.0703	0.0610	0.0685
	(0.0677)	(0.0671)	(0.0668)
L1		0.0444	0.0590
		(0.0453)	(0.0467)
L2			-0.0769
			(0.0761)
Bin 60-90 * Urban (2000)	-0.0371	-0.0467	-0.0380
	(0.0477)	(0.0470)	(0.0482)
L1		-0.0022	0.0133
		(0.0514)	(0.0542)
L2			-0.0125
			(0.0700)
Bin 90-120 * Urban (2000)	0.0095	0.0036	0.0212
	(0.0737)	(0.0771)	(0.0797)
L1		-0.0197	-0.0037
		(0.0619)	(0.0700)
L2			-0.0911
			(0.0714)
Bin 120-150 * Urban (2000)	-0.0139	-0.0352	-0.0285
	(0.0417)	(0.0493)	(0.0517)
L1		-0.0119	0.0040
		(0.0441)	(0.0483)
L2			-0.0921*
			(0.0527)

(Table continues on next page)

Bin 0-30 * Urban (5000)	-0.0295	-0.0534	-0.0569
	(0.1159)	(0.1001)	(0.1015)
L1		0.0928	0.1319
		(0.1127)	(0.1227)
L2			-0.1204
			(0.0797)
Bin 30-60 * Urban (5000)	-0.0936	-0.0966	-0.0987
	(0.0662)	(0.0653)	(0.0702)
L1		-0.0432	-0.0345
		(0.0567)	(0.0700)
L2			-0.0027
			(0.0736)
Bin 60-90 * Urban (5000)	-0.1391**	-0.1202***	-0.1264***
	(0.0531)	(0.0449)	(0.0446)
L1		-0.0572	-0.0619
		(0.0549)	(0.0627)
L2			0.0858
			(0.1003)
Bin 90-120 * Urban (5000)	0.0119	0.0325	0.0178
	(0.0781)	(0.0837)	(0.0832)
L1		-0.0937	-0.0975
		(0.0771)	(0.0849)
L2			0.0114
			(0.0917)
Bin 120-150 * Urban (5000)	0.0566	0.0802	0.0782
	(0.0849)	(0.0856)	(0.0800)
L1		-0.0013	-0.0051
		(0.0754)	(0.0754)
L2			-0.0139
			(0.1109)
Bin 0-30 * Urban (10000)	0.0190	0.0359	0.0447
	(0.0787)	(0.0737)	(0.0746)
L1		-0.0494	-0.0131
		(0.0736)	(0.0885)
L2			-0.0935
			(0.0630)
Bin 30-60 * Urban (10000)	-0.0221	-0.0127	0.0012
	(0.0300)	(0.0274)	(0.0274)
L1		-0.0151	0.0214
		(0.0335)	(0.0440)
L2			-0.0855
			(0.0736)
Bin 60-90 * Urban (10000)	-0.0446	-0.0349	-0.0168
	(0.0363)	(0.0342)	(0.0360)
L1		-0.0225	0.0071
		(0.0467)	(0.0482)
L2			-0.0694
			(0.0538)
Bin 90-120 * Urban (10000)	-0.0873**	-0.0757*	-0.0587
	(0.0380)	(0.0381)	(0.0410)
L1		-0.0307	-0.0069
		(0.0371)	(0.0436)
L2			-0.0858
			(0.0542)
Bin 120-150 * Urban (10000)	-0.0421	-0.0345	-0.0310
	(0.0584)	(0.0565)	(0.0538)
L1		-0.0268	0.0021
		(0.0347)	(0.0339)
L2			-0.0425
			(0.0393)
Observations	19,465	19,465	19,465
Number of groups	506	506	506
Town FE	Y	Y	Y
Year FE	Y	Y	Y
D-C	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

Table 2A.12: Spatial and temporal variation in local mortality—using 20-km bins, excluding first 15 years

Increase over trend mortality	(1)	(2)	(3)
Bin 0-20	0.5283***	0.4209***	0.4312***
	(0.1264)	(0.1143)	(0.1167)
L1		0.3333***	0.3602***
		(0.1052)	(0.1051)
L2			-0.1359*
			(0.0724)
Bin 20-40	0.2351***	0.1476**	0.1559**
	(0.0736)	(0.0668)	(0.0703)
L1		0.2690***	0.2862***
		(0.0735)	(0.0697)
L2			-0.0411
			(0.0726)
Bin 40-60	0.0996*	0.0359	0.0418
	(0.0596)	(0.0557)	(0.0603)
L1		0.1353**	0.1430***
		(0.0543)	(0.0530)
L2			0.0167
			(0.0617)
Bin 60-80	0.0899	0.0398	0.0469
	(0.0712)	(0.0670)	(0.0709)
L1		0.0865	0.0935*
		(0.0545)	(0.0521)
L2			-0.0044
			(0.0728)
Bin 80-100	0.0840**	0.0448	0.0591
	(0.0383)	(0.0357)	(0.0387)
L1		0.0818	0.0846
		(0.0810)	(0.0799)
L2			-0.1034*
			(0.0554)
Bin 100-120	0.1000*	0.0680	0.0716
	(0.0550)	(0.0488)	(0.0488)
L1		0.0907	0.0971
		(0.0590)	(0.0610)
L2			-0.0054
			(0.0375)
Bin 0-20 * Urban (2000)	-0.1195	-0.1251	-0.1213
	(0.1053)	(0.1111)	(0.1155)
L1		0.0303	0.0250
		(0.0927)	(0.0963)
L2			-0.0313
			(0.0929)
Bin 20-40 * Urban (2000)	0.1482*	0.1348	0.1345
	(0.0884)	(0.0871)	(0.0857)
L1		-0.0094	-0.0117
		(0.0880)	(0.0780)
L2			0.0738
			(0.0892)

(Table continues on next page)

Bin 40-60 * Urban (2000)	0.0312	0.0355	0.0249
	(0.0885)	(0.0889)	(0.0898)
L1		0.0685	0.0625
		(0.0594)	(0.0587)
L2			-0.0863
			(0.0817)
Bin 60-80 * Urban (2000)	-0.0497	-0.0562	-0.0501
	(0.0749)	(0.0706)	(0.0693)
L1		0.0771	0.0787
		(0.0959)	(0.0961)
L2			-0.0399
			(0.0724)
Bin 80-100 * Urban (2000)	0.0226	0.0158	0.0105
	(0.1465)	(0.1495)	(0.1558)
L1		-0.0625	-0.0547
		(0.0803)	(0.0771)
L2			0.1000
			(0.1414)
Bin 100-120 * Urban (2000)	0.0352	0.0331	0.0329
	(0.0540)	(0.0604)	(0.0640)
L1		-0.0451	-0.0340
		(0.0782)	(0.0844)
L2			-0.1367
			(0.0964)
Bin 0-20 * Urban (5000)	-0.0866	-0.0967	-0.1138
	(0.3466)	(0.3242)	(0.3078)
L1		0.1245	0.1326
		(0.2736)	(0.2750)
L2			-0.2018
			(0.1440)
Bin 20-40 * Urban (5000)	-0.1550	-0.1478	-0.1365
	(0.1220)	(0.1209)	(0.1301)
L1		-0.1275	-0.1118
		(0.1062)	(0.1234)
L2			0.0472
			(0.1348)
Bin 40-60 * Urban (5000)	-0.1800	-0.1547	-0.1151
	(0.1385)	(0.1269)	(0.1232)
L1		-0.0690	-0.0652
		(0.0955)	(0.1230)
L2			-0.1829*
			(0.1024)
Bin 60-80 * Urban (5000)	-0.2652***	-0.2012**	-0.1954**
	(0.0947)	(0.0855)	(0.0882)
L1		-0.1268	-0.1620
		(0.1083)	(0.1342)
L2			-0.0518
			(0.1173)

(Table 2A.12 continued)

Bin 80-100 * Urban (5000)	-0.0182	-0.0099	-0.0435
	(0.1753)	(0.1905)	(0.1916)
L1		-0.2131	-0.1902
		(0.1452)	(0.1545)
L2			0.2983
			(0.3773)
Bin 100-120 * Urban (5000)	0.0274	0.0738	0.0585
	(0.1342)	(0.1183)	(0.1219)
L1		-0.1536	-0.1722
		(0.1410)	(0.1669)
L2			0.0440
			(0.2075)
Bin 0-20 * Urban (10000)	-0.1427	-0.1497	-0.1406
	(0.1176)	(0.1235)	(0.1117)
L1		0.0090	0.0621
		(0.1326)	(0.1508)
L2			-0.1623
			(0.1059)
Bin 20-40 * Urban (10000)	0.0209	0.0490	0.0655
	(0.1065)	(0.1036)	(0.1061)
L1		-0.1623**	-0.1337
		(0.0814)	(0.0878)
L2			-0.0278
			(0.0800)
Bin 40-60 * Urban (10000)	-0.0511	-0.0329	-0.0021
	(0.0417)	(0.0398)	(0.0446)
L1		0.0503	0.0796
		(0.0454)	(0.0579)
L2			-0.1648
			(0.1030)
Bin 60-80 * Urban (10000)	-0.0407	-0.0144	0.0028
	(0.0668)	(0.0687)	(0.0658)
L1		-0.0106	0.0103
		(0.0505)	(0.0593)
L2			-0.0778
			(0.0769)
Bin 80-100 * Urban (10000)	-0.1427**	-0.1312**	-0.1082*
	(0.0549)	(0.0541)	(0.0627)
L1		-0.0764	-0.0634
		(0.0683)	(0.0669)
L2			-0.0534
			(0.0853)
Bin 100-120 * Urban (10000)	-0.0756	-0.0572	-0.0332
	(0.0625)	(0.0714)	(0.0673)
L1		-0.0854	-0.0555
		(0.0606)	(0.0660)
L2			-0.1379***
			(0.0495)
Observations	19,460	19,460	19,460
Number of groups	506	506	506
Town FE	Y	Y	Y
Year FE	Y	Y	Y
D-C	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

Table 2A.13: Spatial and temporal variation in local mortality—40-km bins, excluding first 15 years

Increase over trend mortality	(1)	(2)	(3)
Bin 0-40	0.3553***	0.2544***	0.2687***
	(0.0856)	(0.0692)	(0.0724)
L1		0.2796***	0.2983***
		(0.0777)	(0.0772)
L2			-0.0895
			(0.0632)
Bin 40-80	0.1129*	0.0455	0.0576
	(0.0663)	(0.0575)	(0.0606)
L1		0.0929	0.1023*
		(0.0581)	(0.0587)
L2			-0.0135
			(0.0580)
Bin 80-120	0.1077*	0.0620	0.0792
	(0.0564)	(0.0489)	(0.0494)
L1		0.0684	0.0783
		(0.0773)	(0.0792)
L2			-0.0854
			(0.0535)
Bin 120-160	0.0156	-0.0126	-0.0014
	(0.0528)	(0.0461)	(0.0462)
L1		-0.0203	-0.0107
		(0.0365)	(0.0379)
L2			-0.0526
			(0.0317)
Bin 0-40 * Urban (2000)	0.0608	0.0528	0.0593
	(0.0649)	(0.0697)	(0.0715)
L1		-0.0024	0.0073
		(0.0658)	(0.0656)
L2			-0.0073
			(0.0705)
Bin 40-80 * Urban (2000)	0.0059	0.0056	0.0139
	(0.0710)	(0.0710)	(0.0726)
L1		0.0696	0.0886
		(0.0691)	(0.0708)
L2			-0.0915
			(0.0786)
Bin 80-120 * Urban (2000)	0.0363	0.0353	0.0453
	(0.0950)	(0.1011)	(0.1098)
L1		-0.0585	-0.0408
		(0.0683)	(0.0750)
L2			-0.0315
			(0.0998)
Bin 120-160 * Urban (2000)	0.0447	0.0287	0.0355
	(0.0524)	(0.0584)	(0.0611)
L1		-0.0338	-0.0225
		(0.0415)	(0.0401)
L2			-0.0613
			(0.0585)

(Table continues on next page)

Bin 0-40 * Urban (5000)	-0.0347	-0.0206	-0.0093
	(0.1147)	(0.1156)	(0.1157)
L1		-0.0617	-0.0524
		(0.1446)	(0.1425)
L2			-0.0207
			(0.1224)
Bin 40-80 * Urban (5000)	-0.1199	-0.0685	-0.0493
	(0.0828)	(0.0866)	(0.0860)
L1		-0.1397	-0.1422
		(0.1073)	(0.1166)
L2			-0.1608*
			(0.0966)
Bin 80-120 * Urban (5000)	0.1240	0.1668	0.1521
	(0.1109)	(0.1301)	(0.1137)
L1		-0.1891	-0.1749
		(0.1514)	(0.1593)
L2			0.1224
			(0.2262)
Bin 120-160 * Urban (5000)	0.4054*	0.4210*	0.4178*
	(0.2169)	(0.2263)	(0.2162)
L1		0.0029	0.0107
		(0.1527)	(0.1487)
L2			-0.0729
			(0.1307)
Bin 0-40 * Urban (10000)	-0.0438	-0.0124	-0.0032
	(0.0827)	(0.0736)	(0.0740)
L1		-0.1138	-0.0797
		(0.0782)	(0.0926)
L2			-0.0569
			(0.0715)
Bin 40-80 * Urban (10000)	-0.0507	-0.0214	-0.0069
	(0.0439)	(0.0439)	(0.0433)
L1		0.0125	0.0468
		(0.0363)	(0.0514)
L2			-0.1088
			(0.0818)
Bin 80-120 * Urban (10000)	-0.1160***	-0.0907**	-0.0786*
	(0.0437)	(0.0411)	(0.0468)
L1		-0.0780	-0.0549
		(0.0594)	(0.0602)
L2			-0.0787
			(0.0704)
Bin 120-160 * Urban (10000)	-0.0227	-0.0014	-0.0098
	(0.0665)	(0.0634)	(0.0626)
L1		-0.0357	-0.0134
		(0.0428)	(0.0440)
L2			0.0631
			(0.0640)
Observations	19,460	19,460	19,460
Number of groups	506	506	506
Town FE	Y	Y	Y
Year FE	Y	Y	Y
D-C	Y	Y	Y

Note: Driscoll-Kraay corrected standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sources: See Appendix B for a description of data and sources.

B   Explanation of data and sources

The mortality data

The mortality data used in the paper originate from counting the number of burials in a locality per year across the seventeenth century. These burials are mainly recorded by parish churches, though other institutions were also capable of recording burials such as beguinages, hospitals, and pest houses. Burials were recorded in both the seventeenth-century cities and towns, and in villages and hamlets of the countryside, although not all localities supplied data for the whole seventeenth century. Some localities provided burial series with occasional gaps (individual years, for example), and some localities only began their burial series at some point into the seventeenth century (therefore not covering the whole 100 years). Generally speaking, cities could provide longer runs of burial data going back earlier in time than rural localities, but this was not an absolute rule—there are also long runs of rural data. Some localities even offer burials information from before the seventeenth century, though not often going back beyond 1550. There are some exceptions such as Mechelen and Alkmaar going back into the early sixteenth century, and the earliest serial burials information found for the Low Countries so far has been from the Sint Bavo church at Haarlem where annual deaths could be found from 1411. Overall in the Low Countries, both the baptismal and marriage registers tend to begin earlier than the burial records—although this is not a universal finding.

Some localities provided more than one series of burials from different 'burial institutions', and their chronologies could overlap. For cities this was more frequently the case, since each neighborhood or parish church of the city could have a burial register, alongside beguinage, hospital and pest house deaths, which were more frequently found in the cities. Large places such as Amsterdam could provide as many as 15 different burial institutions in the seventeenth century. In the case of the Low Countries, however, rural localities could also provide more than one series of burials—particularly in those cases, in the context of the Dutch Revolt and Thirty Years' War, where two religious denominations could be found. This was often Catholic and Dutch Reformed (Calvinist), but also could include Mennonites, for example. Whether or not these series overlapped, they were treated separately in our analysis. This is done because it is not always clear that non-overlapping series were compiled using the same approach or accounting for the exact same representation of population. In the case were series did overlap, treating them separately simply yields more precision in the analysis.

Most frequently, burials were compiled in a document that was kept separate to

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the baptisms and marriages, though not always. Especially in the Southern Netherlands, dominated by the Catholic Church, registers did often combine all indicators together in one document. Even when burials were found on one document, there were still some variations in recording practice. Some documents simply referred to date of burial, some documents referred to date of death and date of burial, and some burial documents provided even broader distinctions—for example, whether the deceased received a full burial or just the tolling of the church bells, whether a new grave was opened up for them or not, whether they received a kind of physical monument (*legherstede*), what kind of bells were tolled (if there was a choice), and whether the deceased received any additional 'extras' such as particular cloth, candles, or wine. This kind of more precise information, often with the different costs of all components involved, was much rarer, and generally when burials were part of broader church accounts (*kerkrekeningen* or *diaconierekeningen*). Churches tended to have standardized prices for adult and child burials, and the tolling of the bells (which could vary in status), but payment for testaments could often bring about different amounts. Accordingly, although some scholars have suggested that the burial records did not fully record the poorer sections of society – especially given that during mortality crises many families did not have the funds to pay for numerous expected funerals – it is our contention that a number of the poor still appeared due to simply paying for the church bells, which cost less. Moreover, although burials were costly, the *'arme'* and 'paupers' still appeared in great numbers as explicitly recorded in the registers (often during periods of social distress), and their burials were often facilitated by being paid for out of the poor table.

The burials for a locality were frequently registered by a local priest, who acted as the scribe. The deaths of a priest were also recorded in the burial registers themselves (sometimes with a short hiatus), by the deceased priest's successor. Priests also were mobile and sometimes moved away to a new place: this is rarely mentioned in the burial records, but of course we can discern change from the different style and type of handwriting presented. The longevity of priests in their task as registering burials differed markedly—some stayed in that role all their lives and can be found giving 40 years' worth of information, while others had much shorter tenures. Priests often went to the houses of the sick to read them their last sacraments, or act as a witness for a last testament. On the one hand, this meant that the priest could often discern firsthand what the person was dying from (a disease, for example) based on their own knowledge of the signs and symptoms, and on the other hand, this meant that in times of epidemic diseases, priests could be highly susceptible to contagion. The fact that in the Low Countries rural localities often provided more diverse cause of death information for the deceased (diseases, or symptoms of diseases) may be attributable to the fact that

village priests had fewer households to personally visit, while priests of urban parishes often had too many names to register.

In the paper, the burials information is aggregated annually, though in the burial records themselves, this temporal recording is often more precise—on many occasions the deceased are marked with the exact date of burial. This is not found everywhere—sometimes no such precise information is provided, and sometimes this information only appears a bit later in a locality's burial register series: overall, this precise temporal information has been found in 78 per cent of the localities included in the database. In quite a large proportion of these localities, both the dates of death and dates of burial are included, and this is useful information, because it eases our fears that the dates of burial are delayed significantly after the actual deaths. In fact, the gap between death and burial was not usually particularly long – often some days or at most a week or so. During epidemics such as plague, urban authorities often made it explicit as part of their announced 'plague ordinances' that those dying of plague needed to be buried with great speed—often citing 24 or 48 hours. Very rarely do we see on account of the scale and magnitude of death in a short period, the mechanisms for burial completely breaking down—with unburied bodies, for example, caused by lack of coffins—but we do, from time to time, see that church burial sites or cemeteries become full or suffer from lack of space. Urban governments tended to respond to this problem quite quickly, however, and new burial sites were often constructed outside the city walls.

The burial registers could record victims of all ages—that is to say plenty of localities recorded adults and children, and more to the point, distinguished between them. On occasion, this was through an exact age provided at death, although this was not frequent, and mainly found for the Catholic institutions in the Southern Netherlands. More often, the distinct was made on the basis of simply referring to the deceased as a child via terms such as *'proles'*, *'kind'*, *'enfant'*, *'puer'*, *'puella'*, and so on, although of course one problem then is that we do not know what 'childhood' status meant to the compilers of the burial records. It must be said, however, that when a specific age is simultaneously mentioned with a marker for 'child status' (such as *'kind van'*), this age tends to be below 15 years. A trickier category was those that were marked as 'son of' or 'daughter of', since on occasion this could refer to offspring who were actually adults (young single adults living in their parents' home), although mostly when we see the terms 'son of/daughter of' and a specific age, it tends to be below 15 years. The fact that infants were sometimes explicitly recorded in the burial registers is important, since infant mortality comprised a large proportion of the total mortality – especially in the cities. Generally child mortality (those below 15 years) was equivalent to adult mortality, and in the largest cities such as Leiden, for example, the adult-child mortality

ratio for the whole seventeenth-century taken together was 0.74. In the most complete cases of burial records, we even learn when a child died as a stillborn—interesting because it shows that a child did not need to first be baptized to be registered as dead.

Nevertheless, there were occasions when either (a) infants and adults may have been recorded, but there is no way of distinguishing between them based on the provided information, or (b) adults are explicitly recorded for a locality, but children are not—or at best, some children are recorded but clearly not systematically. For the purposes of the paper, this is unlikely to be too problematic, as long as the more limited form of burials information remains consistent through an individual locality's series. However, it must be conceded at least that some epidemic diseases may have created sharper mortality spikes for children rather than adults, or vice-versa: smallpox was a major child killer, for example. Our collective knowledge of the age-selective effects of epidemic diseases leading to mortality spikes in the pre-industrial period is still decidedly low.

Unsurprisingly, the burial records were much more consistent and systematic in their recording of actual proportions of male and female death. For adults, this is obviously discernible from first names, and when first names are not provided (often for women), then other markers are used—for example, 'mother of', 'widow of', 'wife of', 'sister of', and so on. Cities tended to recorded more female deaths than male, while in the countryside this was the opposite way round: in a sub-sample of specifically sex-disaggregated adult deaths from our database of 245 rural localities and 55 urban localities, the male-female sex ratio in mortality was narrowly above 1 in the countryside, and below 1 in the cities, when consolidating all the data together for all years of the seventeenth century. This is not evidence of systematic under-registration in the burial records of a certain sex, but reflective of the actual spatial distribution: quite simply by the seventeenth century in the Low Countries, as in other parts of Western Europe, more women lived in cities. From that sub-sample of 300 specifically sex-disaggregated adult deaths from our database, the overall sex ratio in mortality for adults was around 1. There was also a small proportion of adult burials in the database that provided no sex indication—and this was more frequently the case during mortality spikes caused by epidemic diseases. In these cases, the dead would sometimes be grouped together anonymously ('4 *doden*', for example), or described as an *'arme dood'* (poor death), or an anonymous death attributable to a pest house or hospital – an issue more found in cities than rural areas. In any case, in this paper we remove all burial series taken from institutions that were not ordinary churches (pest houses, hospitals, and so on), and thus do not have to deal with these kinds of issues. This is further important given that these kinds of institutions often had deliberate social

selection—the Catharinagasthuis in Leiden had in 1614 five women's rooms and only two men's rooms, while the Elisabethgasthuis accepted only women (Ladan 2012, 102-3). Of course, as mentioned above, it is not always possible to confidently distinguish between adult men and women, and those who were in fact boys and girls. On occasion, we do have, as already mentioned, the terms *'puer'* and *'puella'*, or 'son/daughter of', but for children the systematic distinction on the grounds of sex is generally much harder for most localities simply because children are often not referred to by their first names but as simply 'child of' along with their parent's name. In more confusing cases, it is even not entirely clear whether the first name given is that of the child or that of the parent. Nevertheless, any methodological issues regarding the identification of the sex of the deceased in the burial records have little impact on our paper's empirical strategy, and we can instead at least be confident of the fact that both sexes were fully recorded in the source, and did not suffer from typical problems such as 'missing women'.

One issue of significance when using the burial records to reconstruct mortality trends in individual localities over time is how the registration process is affected by the dynamics of migration. At the very least, there is something to be said for the notion that there may have been a certain proportion of people in cities that were less likely to be captured in burial records, as migrants had fewer family members or social networks living close by. However, many of these poor migrants did find their way into the burial records—they were simply the anonymous deaths (sometimes clustered together in a group), which also tended to increase in number during epidemics. Furthermore, there is direct evidence that recent migrants were still often recorded in the burial records of some cities: in Louvain, for example, many of the seventeenth-century deaths in a large number of the different churches are marked with indicators of where they had recently come from—often the surrounding countryside. This was also seen in the countryside too: for example, migrants to different localities in the eastern parts of Groningen to work on dike building and maintenance projects were explicitly recorded as coming from areas of Munster and especially Westphalia. And moreover, there is very little evidence at all of a trend towards migrants being buried instead in their localities of origin—some isolated cases can be traced of this, but not at all quantitatively significant.

Given the focus of the paper on the mortality impact of warfare for civilians, we must also pay attention to the issue that soldiers may appear in our burial registers. It must be noted, first of all, that soldiers do appear with greater frequency during episodes of known mortality crises caused by epidemic diseases (acting as basic supporting evidence for our overall contention in the paper that warfare was linked to raised mortality in some way). However, their greatest number tend to appear in institutions

such as plague houses and hospitals, which makes sense if they were being treated there for injuries or sickness. This is seen from the burial records of the Onze-Lieve-Vrouw gasthuis in Mechelen, which listed 358 soldiers out of 853 total burials for the institution in the years 1692-7 (42 per cent), at the height of the Nine Years' War. This is another justification for not including burial institutions such as plague houses and hospitals in the overall dataset, since the large number of soldiers may skew an accurate picture we need to reconstruct for civilian mortality trends. Elsewhere, soldiers were sometimes buried by ordinary local church institutions, although notwithstanding a handful of odd localities, the numbers of soldiers was decidedly small. Most frequently with a burial series for a locality, individual soldiers would appear in just a few years and not in great numbers. This is hardly surprising given that local communities experiencing the direct hardships as a result of military occupations were not particularly disposed to financing expensive burials for random outsider soldiers. More to the point, some of these soldiers recorded were not 'outsiders' but simply living in the locality in question. Accordingly, we do not remove soldiers from the ordinary church burials database simply because their quantitative number is not high enough to qualitatively affect in a particular direction the civilian mortality trends we are trying to calculate.

Finally, the Low Countries burial records have been largely disregarded as a source of information for saying anything about causes of death – beyond sporadic references to violence and drowning (Devos and Janssens 2017, 7)—and this hinders in some ways our attempts to establish the precise mechanisms linking warfare and mortality increases. It is, however, not true that there is nothing to be gleaned from the burial records with regard to cause of death. Overall, out of 553 series of information from burial institutions from 435 different localities, 244 of these series had at least one reference to a cause of death in at least one year of the seventeenth century (41 per cent), which was substantially more than the 1 per cent of 600 parish registers with explicit cause of death consulted by the seminal works of Mary Dobson for southeast England in the early modern period. This included all references to a disease or sickness of some description, death via childbirth, and death by being killed or injured.[31] The issue is that this is not systematic information, but for many localities, just the odd sporadic reference to a cause of death in a particular year, perhaps for a particular person. Indeed, overall, out of 1,532,010 individual burials in the entire seventeenth-century database, only 29,947 of these were actually given a cause of death (2 per cent). Furthermore, the lack of systematic recording did not only vary between localities, but within the same locality across time. For example, in Leiden in 1604, there were 1,717 burials

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[31] However, this does not include *'subita morte'* (sudden death), which is fairly common in the burial records, but difficult to interpret. It also does not include those that drowned.

out of a total 3,311 (52 per cent) explicitly attributed to plague (marked with a p behind their names), yet in bigger epidemics such as in 1624 (7,709 burials) and 1635 (16,336 burials), there was not one solitary 'p', 'pest' or reference to any other cause of death—and in 1635 this is remarkable given the total death rate may have been as much as 30-40 per cent of the population.

The largest proportion of terms used to describe cause of death by disease were those that tend to be associated with plague—*pest*, *peste*, *pestilentia*, and the like, although it is rather an assumption that this kind of terminology in the seventeenth century was definitively applicable for the disease we know to be caused by *Yersinia pestis* rather than a catch-all term for diseases which may have displayed similar symptoms to plague. References to other kinds of afflictions did appear, however—they just did not feature as much in the burial records. The easiest to recognize diseases were those such as dysentery (*rode loop*, *rode melisoen*, *dissenteria*, *bloedgang*, and so on), likely on account of its distinctive symptoms of frequent defecation and blood in the stool; or smallpox, on account of the distinctive marks to the skin visible to all; or dropsy, on account of the clear swelling of the ankles. Other afflictions were much more problematic. Diseases such as typhus were rife in the seventeenth century, and yet were never formally identified as such. Probably many simply fell under the broad terms used for 'fevers'; the same went for malaria too. Occasionally, especially in the Southern Netherlands, the term *'phthysis'* was used—apparently relating to the disease known as tuberculosis—and yet it is unclear on what symptoms this was being distinguished from other diseases with feverous characteristics. Only on very special occasions were there more specific description of symptoms in the burial records: for example, in the northern coastal village of Weiwerd (Groningen) in 1680, one girl died of a *'jammerlijke plaege, hebbende het vijer in het mond'*—a terrible affliction bringing a burning sensation to the mouth. This was during a time said to be of heightened malaria outbreaks all across the North Sea area from 1679-81. In the Campine village of Poppel (Brabant) in 1626, one person was said to have died suddenly in a state of *'nigronisa'*—a black color which may have been reference to buboes during a plague outbreak. Generally it was the burial records from the Catholic church that brought the most colorful descriptions beyond the standardized formulaic information: hot fevers in 1694 in the village of Lottum in Limburg led one sufferer to 'become a spectacle of the whole community', while in nearby Sittard in 1656, one man suffering from discharge from his stomach after sickness simply ran into a pool of water and drowned.

A lack of clarity also surrounds those deaths being attributed to a 'contagion' of some kind. In some cases, vague terms for a contagious sickness appear in years that have a lot of localities recording plague deaths. The notion that these terms were

simply replacements for *'peste'*, however, is further complicated by those localities where we have in the very same year, by the very same priest, both the terms 'pest' and *'contagieuse sieckte'*. If they were always one and the same thing, then why are different terms being used in a very short time period? Partially, the reason may simply lay in the fact that it was difficult to come to a diagnosis based on symptoms that were often very similar—see the number of afflictions with 'unexplainable death'—and disagreements likely occurred. Indeed, in the Limburg village of Beek in 1652, it was noted that there was a suspicion that a Jan Craechs was infected with plague, but others disagreed and said no. Accordingly, much of this information on cause of death from the burial records can be instructive, and help fill in gaps in our knowledge, but (a) can rarely be employed in any systematic quantitative sense, and (b) even with the information is provided, we have to recognize that these are subjective assessments based on the interpretation of signs and symptoms often by people without any specialist knowledge.

Population estimates

A note on estimates of population size per locality The burials locations are supplemented with an estimate of their population size. As alluded to before, accurate population figures for all these localities are not available and certainly not at a high temporal resolution. Except for a few cities, information on population per locality tends to derive from various kinds of fiscal source that are irregularly spaced over time and rarely consistent in their definition of population. To distinguish small villages from cities, and to discriminate between smaller and larger cities, population estimates were constructed for as many localities as possible. The reconstruction intends to provide a population estimate for a date as close as possible to 1600 and 1700 based on a variety of sources, although sometimes we only have information on population for one of those time points. In accordance with existing literature on urbanization in early modern Northwest Europe (for example, Clark 2002), we define villages as localities that never have more than 2,000 inhabitants just before, during or directly after the seventeenth century. Small cities have at least 2,000 but less than 5,000 inhabitants; middle-sized cities have between 5,000 and 10,000 inhabitants; and large cities have more than 10,000 inhabitants at one point just before, during or directly after the seventeenth century. In some cases, the size of a locality changes across the seventeenth century—for example, in 1600 having 8,000 inhabitants, and then in 1700 having 11,000 inhabitants. For the purposes of this paper, we attribute the largest settlement status to the locality—thus in this cited hypothetical example, it would be a +10,000 urban settlement classification. It must be noted that while populations of localities did change across the seventeenth century, not many of them changed in such

a way as to move entirely into a new category.

As a starting point, population estimates from the often-used dataset by Buringh were used to identify cities.[32] The data identify cities with a minimal population of 5,000 inhabitants at some point between the year 800 and 1800. We expanded the data with population information from the many detailed historical studies on particular regions in the Low Countries, such as Bieleman (1982), Brusse (1999), Van Cruyningen (2000), Guignet (1997), Gutmann (1980), Hoppenbrouwers (1992), Klep (1981), Morsa (1987), Roessingh (1964), Van Schaïk (1987), Slicher van Bath (1957), Vandewalle (1992), Vermoesen (2011), Van der Woude (1962), Van Zanden (1994), and more. Most of these studies exploit fiscal sources that document the number of households or hearths in the localities of particular regions, and accordingly this requires us to sometimes use a multiplier in order to estimate overall populations. For this we often take the multiplier cited in the particular study (since different regions had very different household sizes: Van der Woude 1972), and if no specific multiplier is suggested, we use an appropriate one for the type of source from which the hearth/household counts have been taken. For example, for those sources that only record populations of ages 16 or over, we know from historical demography studies that usually the population of an early modern locality under 16 years of age was often equivalent to that over 16 – and thus the overall population figure is double. In doing so, we consistently use the same multiplier for similar sources concerning different places.[33] For localities without population estimates based on such literature, we continued to search for even more localized literature or by consulting published fiscal sources directly—such as the hearth counts listed in Arnould (1956) and Cuvelier (1912).

This search yielded information for the large majority of the 442 localities comprising the burials data, but 169 localities remain without accurate population figures. Nonetheless, these 169 localities invariably are small hamlets even today, and thus were unlikely to have been cities in the seventeenth century. The 273 localities with population estimates do not always yield estimates very close to 1600 or 1700—or even between those dates. In several cases, the population estimates are for later or earlier dates but these are still used when they are illustrative for the population category of the locality—for instance, it is unlikely that the hamlet of Glimes with 55 inhabitants in 1496 had more than 2,000 inhabitants during the seventeenth century.

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[32] This dataset, used in Bosker and Buringh (2017) and Bosker, Buringh and Van Zanden (2013), is an expanded version of the original data provided by Bairoch, Batou and Chevre (1988).

[33] Besides the number of adults, several sources are based on the number of households, hearths, or the number of 'heads of households' per locality are multiplied by five to obtain total population—a multiplier of 6 is used when only taxable households are listed. Counts of the 'number of able-bodied men' are multiplied by four, counts of the number of 'communicants' are multiplied by 1.5.

A potential alternative for population size to discriminate villages from larger towns is to use information on city rights. Accordingly, we collected information about city rights and the dates at which these were granted – usually at some point in the Middle Ages. However, even a brief glance at the data reveals that the use of city rights as a marker for city-size is problematic. Although virtually all large cities had city rights, there are plenty of places with city rights that remained very small during the early modern period—for instance, the city of Sint Anna ter Muiden had only 200 inhabitants in 1698 and the city of Coevorden had approximately 1,000 inhabitants throughout the seventeenth century. Accordingly, the paper does not exploit variation in city rights across towns as a marker of city size.

Construction of the war events

The list of war events in the seventeenth-century Low Countries and its direct surroundings derives from a detailed survey of existing literature in military and political history. A war event is an occurrence of hostile military activity at a certain place and time, and includes battles, skirmishes, sieges and hostile occupations of towns. The reconstruction of the war events builds on the contention that military activity in early modern Europe was concentrated in annual campaigns. During winter, armies were put to rest for lack of provisions to collect on the march, and to take shelter against the wet and cold. Particularly in the northern parts of Europe, including the Low Countries, winters significantly reduced the possibilities of travel by roads and rivers. Armies traveled with ample baggage, provisions and cannons, all of which were difficult to transport. In the Low Countries, the season of military campaigns ran from late April to late September with only minor movement of troops outside that periods, and hardly any movement between December and early March (Parker 1972; Van Nimwegen 2010). The few daring—or foolish—commanders that pursued sieges throughout winters produced textbook examples of the problems of military activity during winters.[34]

Winters were used to design the next military campaign, to arrange budgets and decide on strategic targets. The political deliberations surrounding such planning and, even more, the attempts to anticipate the enemy's next military campaigns have yielded a rich source of documentary evidence that is often used by political and military historians—with the additional benefit that the seventeenth-century Low Countries are

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[34] The Siege of Breda (1624-25) and the Siege of Ostend (1601-04), the two major examples of eventually successful sieges running through the winter in the seventeenth century Low Countries are renowned for the massive loss of life among besieging armies—as well as the besieged. The short winter siege of Jülich (1621-22) forms the exception to this rule.

particularly well-studied. Accordingly, for each year it is quite clear which generals were brought into the field, with what armies and goals. By way of example, Jonathan Israel writes the following based on the correspondence between Francisco de Moncada, Marquis of Aytona, the chief commander of the Spanish forces—and eventually interim-governor—in the Low Countries in the early 1630s and King Philip IV of Spain:

> "The Dutch resumed their offensive, having restored their superiority in forces in the spring of 1633. With Count Willem posing a threat to Flanders from Zeeland with a force of 5,000 men, Frederik Hendrik massed the bulk of his field force at Schenkenschans. On 11 May he appeared before Rheinberg, now the foremost Spanish base in the east, and besieged the town [...] Meanwhile, Aytona, with an army of 14,000, advanced eastwards to the Maas which was only lightly covered by a Dutch cavalry force under Stakenbroeck. Aytona occupied Maaseik, Montfoort, Weert, and the highly strategic Stevensweert [...]" Israel (1982: 189).

Exploiting this type of information, we obtain for each year information indicating whether there was any military activity, which generals took the field, where they did so, and with what aim. Using these annual profiles as starting point, we reconstruct for each general and army as much of their military activity as possible throughout each year. This is done through an extensive survey of the literature focusing on these generals, war periods, or geo-political struggles related to military activity in the seventeenth-century Low Countries.[35] Notably, this reconstruction includes military activity taking place in close proximity to the Low Countries.

Hence, and except for a few years wherein no war events occurred—which can be verified through the same literature—the data often consists of several detailed sequences of military events per year. The reconstruction of these individual sequences allows for cross-referencing the different sequences per year. This can also be observed from the example given above, which highlights that armies acted in clear recognition of the behavior of other armies—both allied and enemy forces. Accordingly, coverage of all relevant sequences of military events is likely to be quite complete. The same does not necessarily hold true for events within each sequence. Although our reconstruction

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[35] The majority of the war events follow from several main references: Childs (1991), Duerloo (2012), Israel (1982; 1997), Lynn (1997; 1999), Van Nimwegen (2010) and Parker (1972; 1984). Following their depictions of military activity for particular episodes of the seventeenth century, we expanded the search to specific years, areas or armies where the information in the main sources was slim. Hence, for specific parts of the seventeenth century, we extensively used Anderson (1999), De Cauwer (2007), Duffy (1979), Fruin (1972), De Groot (2017), Gutmann (1980), 't Hart (2014), Maland (1980), Parrott (2001), Wilson (2009) and Young (2004). Several minor gaps were filled using specific references or sources followed up through this literature—which can be found in the data-Appendix.

is focused on covering as many events per sequence, there are several reasons why each skirmish or occupied hamlet, farm, or sometimes even each town, will not necessarily be covered per sequence. To a large extent, this is simply due to source availability. Generals did not in all cases report all the villages they attacked during their campaigns. Even though the information from their reports can be augmented with, for instance, local fiscal sources or reports, such sources may also miss out on particular events. Therefore, we make no claim that our reconstruction of war events is exhaustive. Rather, we assert that it does provide a consistent sketch of the annual spatial spread of hostile military activity in the Low Countries. In the main text we discuss that by drawing concentric circles around each war event and coding all localities within the circle as similarly war-affected, we obtain unbiased approximations of military activity. This is the case even if, over the trajectory travelled by an army, several military events but not all are missed in the data.

At the same time, this approach entails that little reliable information can be obtained from the 'density' of war events, apart from the information whether or not there was any war event nearby. For instance, it is not necessarily the case that a locality with three military events within a given distance—say, thirty kilometers distance—experiences more warfare than a locality with just one event. However, it is the case that one or three military events within thirty kilometers is different than experiencing no war event within that distance.

In addition, the source material provides relatively limited information about war events. That is, in some of the better covered cases—some famous battles and major sieges—there is ample information on the duration of the event and the number of soldiers involved. For most events, however, there is hardly any information regarding the nature of the event besides the mention itself—the sack, storming, siege, or occupation of a town, soldiers of different armies coming to blows at a certain place. Even where information on the nature of the event is available, it remains difficult to determine the intensity of a war event—how much more intense is a siege of two weeks than a one-week siege, and how much larger does a besieging army need to be to make up for a one-day shorter siege? Because of these two reasons and given that the actual intensity of war events might be correlated with the probability that the war event is covered in the data, we abstain from constructing intensity measures of individual war events.

An important consideration is whether the war events covered in the data—battles, skirmishes, sieges and hostile occupations of towns—comprise a broad enough approximation of the pressure of warfare. To be sure, warfare produced other burdens on civilian life. However, if the question is whether warfare directly affects civilian

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mortality, as it is in this paper, then there should be a clear relation between military activity and civilian mortality. This is true for mortality through violence, but also for mortality through disease and dearth—particularly in the Low Countries where trade networks were dense enough to ensure that blockades in one place would not produce dearth in places far removed from military activity. Still, military activity also took shape through lodging and billeting of soldiers, military acquisitions and the levying of contributions—these factors are extensively discussed by Gutmann (1980) in a remarkable study of the impact of warfare on civilian life. These elements do not necessarily correspond fully to the war events in the data.

Lodging and billeting often took place in friendly or neutral territory, although usually in relative proximity to the frontline. The levying of contributions did center on hostile territory but was not necessarily associated with war events as in our definition. Nonetheless, contributions and acquisition took place in the proximity of hostile military activity, whereas billeting and lodging are not violent military activity per se. In addition, lodging, billeting, military acquisitions, and contributions are not clearly covered in the literature nor are they consistently traceable in the sources. And even where contributions are traceable, they often lack a clear spatial pattern because contributions were often imposed on regions rather than on specific towns (Gutmann 1980). Accordingly, including mentions of contributions as war events would entail several important problems in terms of consistency. Instead, leaving contributions out altogether entails the implicit assumption that the spatial pattern of warfare, as exemplified by the war events, correlates closely enough with spread of contributions.

Another potential caveat is the possibility that data are not missing at random and that the pattern of nonrandom data limitations correlates with other variables that explain the variable of interest—deviations from trend mortality within localities. This would be the case, for instance, if there are some regions that are less covered in the literature and, as a result, in our data and that these same regions are the more likely to experience high (or low) mortality effects—or, alternatively, that regions with a specific settlement pattern (many cities; many small villages) are more or less likely to be covered in the data. Obviously, whether data are missing at random or not cannot be tested directly given that it is unknown which data is missing. Instead, what can be shown is that the main results established in the paper do not depend on particular periods or regions—such as highly urban, highly rural, early or late periods, or periods of spectacular military activity. Appendix A provides such robustness checks, which consistently deliver the same results as the main results established in the paper.

Ultimately, the war events data in this paper comprise 755 war events in and around the Low Countries between 1600 and 1699. This number could be contrasted

with existing literature, using major battles and sieges from encyclopedic sources such as Bradbury (2004), Clodfelter (2002) and Jacques (2007). For instance, Dincecco and Onorato (2016) combine data from Bradbury and Clodfelter to obtain 800 war events for the whole of Europe between 800 and 1799. However, this comparison is somewhat off. The 800 war events of Dincecco and Onorato (2016) are used to gauge the locations of major conflicts at a macro scale. Therefore, their terminology of war events as 'conflicts' is more apt for the focus of their study. The 755 war events in the seventeenth-century Low Countries are not locations of major conflicts but points on a map that jointly describe the spatial pattern of warfare per annum. Importantly, both approaches allow for hardly any additional information about the distribution of the intensity of warfare within these spatial patterns. That is, not all places near a war event, nor even places located at the same distance of a specific war event, may experience the same 'war intensity'. Nonetheless, this paper contends that places located near a war event do experience warfare in such a sense that it can be compared to the same locality when it experiences 'peace'—no war event nearby—which is the aim of this study.

Chapter 3

Sharecropping as a Kick-Start Contract: Coping with Warfare in the Central Low Countries, ca. 1500-1550

[1] Bram van Besouw

Abstract. Because share contracts entail the sharing of ex ante unknown final output and non-labor inputs at a pre-determined rate, they provide weak productive incentives and ample scope for deceit. Nonetheless, share contracts outperform fixed-rent contracts in particular contracting situations; to combine productive incentives with limited risks to tenants, combining multiple conflicting inputs, or when tenants are credit constrained. Accordingly, sharecropping is theoretically and historically associated with poor tenants, risky agricultural production, and underdeveloped economies. Instead, in this paper I use the rich archives of Mariënweerd Abbey, a large landowner in an economically advanced, commercial livestock region of the Northern Low Countries, to show that share contracts were successfully used to facilitate investments in livestock and generate liquidity in the wake of a period of intense warfare. Importantly, the Abbey used these share contracts exclusively for its largest tenants, whereas it solved the standard contracting problems with its normal tenants through fixed-rent contracts. Accordingly, this use of share contracts entail a strikingly different association with economic development.

3.1 Introduction

Agricultural contracts provide rich information about economic and social relations in the agricultural sector of economies. Prices and payments have been used to infer factor productivity and rewards, while contract duration and stipulations about investments shed light on the trade-off between exploitation and investments or, put differently, between short term profits and long term productivity of the farm. Unsurprisingly, agricultural contracts have been widely studied by economists and historians.[2] Particularly well-studied are the implications of sharecropping, a type of lease contract wherein the tenant and landlord share non-labor inputs and final output of the farm according to a predetermined rate—often at a rate of fifty per cent. Share contracts are one of two archetypal contracts for leasing land, the other being fixed rent. Share contracts are in wide use in many parts of the developing world and, throughout history, were used for instance in several parts of the Mediterranean and in the US South (Otsuka, Chuma and Hayami 1992; Federico 2006).

Because share contracts do not grant tenants the entire marginal product of their labor, they provide weaker productive incentives than fixed-rent contracts. In addition, tenants and landlord need to spend considerable effort to observe the correct sharing of final output and non-labor inputs. For these reasons, share contracts are considered sub-optimal to fixed rents in most contracting situations except for some specific ones— arising when contracts and information are imperfectly observed or enforceable. In particular, share contracts are often useful to balance incentives and risk to tenants, they provide better incentives when several inputs in the production process need to be combined, and they reduce problems deriving from limited liability of tenants (Ackerberg and Botticini 2000; Bandiera 2007). These theoretical observations consistently predict that sharecropping is more likely among poorer tenants, in the production of crops that are vulnerable to over-exploitation or that have volatile yields, and in regions where few alternative institutions for risk-spreading and capital constraints exist (Hoffman 1996). Accordingly, share contracts are associated with less developed economies rife with market imperfections (Stiglitz 1974; Deiniger, Jin and Yadav 2013).

The historical literature is particularly rich in empirical studies that seek to explain why share contracts were used in particular settings. It has long emphasized that sharecropping existed only in specific conditions and in a limited number of cases (Carmona and Simpson 2012). In the Mediterranean, share contracts in some cases provided a solution to capital constraints of tenants and the risks associated with

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[2] See for instance, and among many others, the seminal contributions by Allen (1992) and Hoffman (1996).

viticulture. There, share contracts extended viticulture to more marginal areas that would not have been cultivated otherwise, and to tenants that would normally not have been able to engage in commercial farming (Hoffman 1982 1984; Cohen and Galassi 1990; Carmona and Simpson 1999; Garrido 2017). Others have argued that the use of share contracts should be understood in the context of dysfunctional rural markets and economic oppression of tenants (Epstein 1991, 1994).[3] The historical literature puts more emphasis on the specific contract details and on social and economic conditions to explain the existence and effects of sharecropping in particular cases. However, the historical literature similarly studies sharecropping as cross-sectional variation between types of crops, categories of tenants and different regions. And, again, it is related to small-scale farming and studied with particular focus on underdevelopment (Emigh 2009).

In this article, I draw attention to the use of share contracts in a economic context often considered incompatible with sharecropping: that of the highly developed rural economies of the northern Low Countries. I firstly show that share contracts were quite common in several parts of the early modern Low Countries. The instances where share contracts were used resonate the attention of historical literature to the specifics of the share contracts as well as local economic and social relations. Furthermore, many of the instances confirm the standard association of sharecropping with poor tenants, specific crops and, often, relatively underdeveloped markets—or tenants lacking access to markets. However, sharecropping was also used for relatively short periods of time in the wake of crises. A prodigious example of the latter is the use of share contracts by Mariënweerd Abbey in the 1530s following two decades of warfare in the area. The Abbey was located in the central river area where the early disappearance of feudal impositions in combination with clearly defined and enforced property rights had resulted in flourishing lease markets and a highly productive rural economy based on commercial livestock farming (Van Bavel 2009; 2010). In addition, this area was well-connected to the burgeoning capital and commodity markets of neighboring Holland. Importantly, share contracts were exclusively used for large farms, with up to 90 hectares of farmland, thus with wealthy tenants. All of these elements are at odds with standard predictions on sharecropping.

Based on a closer examination of the Mariënweerd lease contracts, I show that the

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[3] Another widely studied historical case of sharecropping is the Post-Bellum US South. There sharecropping is often interpreted as an intermediate rung on the 'tenancy ladder' for poor farmers, allowing them to save money and engage in fixed rent or become outright owner of land in a later stage of their life (Reid 1973; Alston and Kauffman 1997; Alston and Ferrie 2005). A contrasting view is that share contracts mainly served the purposes of white landlords in restricting geographic and economic mobility of credit-constrained, politically oppressed, black tenants (Fishback 1989; Ransom and Sutch 2001).

standard contracting problems—risk, combining multiple inputs, and limited liability—commonly associated with sharecropping are relevant in this case, but that they were normally solved through additional stipulations in fixed-rent contracts by the Abbey. In addition, the Abbey's detailed knowledge of the value of different plots and, by implication, of tenant's relative productivity, limited problems of moral hazard. To solve limited liability problems and to reduce tenant risk, the Abbey allowed tenants to accumulate debts during adverse periods. In some cases, it even acquitted debt during crises. Furthermore, the Abbey financed improvements by tenants or directly engaged in investments. Given that share contracts increased tenants' scope for deceit and limited their productive incentives, the natural question is why Mariënweerd Abbey would adopt share contracts, and if so, why it did so only for its wealthiest tenants. In this paper, I show that share contracts were particularly useful to facilitate recovery following periods of intense warfare that left both Abbey and tenants in crisis. As share contracts implied joint investments of non-labor inputs, they facilitated recovery of livestock-oriented farms were heavy capital investments were required. Furthermore, they automatically reduced the burden of rent in the first years of the contract while farm production recovered. By using share contracts for a limited number of tenants on particularly large farms, the Abbey managed to kick-start recovery on a substantial portion of its landed estate.

The argument developed here is that not only are share contracts useful in specific economic environments given particular crops and market conditions (Carmona 2006; Carmona and Simpson 2012) but also to temporarily facilitate investment and liquidity through the sharing of non-labor inputs. The specific conditions found in this temporary use of share contracts do not point in the same direction as existing predictions regarding the use and efficiency of sharecropping, let alone regarding the implications about the general economic affluence of a region based on the use of share contracts. The important advantage of share contracts in the case of Mariënweerd Abbey is that it facilitated extensive joint investment in livestock—that is, in capital. Existing historical literature on sharecropping emphasizes the difficulties to render share contracts compatible with production-enhancing investments, except in some particular cases related to viticulture (Garrido and Calatayud 2011; Garrido 2017). In this paper, I emphasize that theoretical predictions about share contracts and investments do not necessarily warrant that conclusion but, instead, provide several contrasting predictions regarding sharecropping and investment. More than adding yet another rationale for sharecropping, I show that the temporal usage of sharecropping require a clearly different interpretation of its usefulness and its implications for economic development.

The next Section provides a focused overview of the theoretical literature and

empirical work in economic history explaining when and why share contracts are used. In Section 3.3, the understudied role of sharecropping in the early modern Low Countries is connected to this wider literature. Importantly, share contracts should, theoretically, be rare in this economically developed part of Europe. Nonetheless, Section 3.3 illustrates the wide spectrum of instances of sharecropping in the early modern period. Some of these cases align rather well with theoretical predictions about sharecropping and the general historical literature, but several cases point to the use of share contracts as response to crises. The case of sharecropping in Mariënweerd, as a prodigious and exceptionally source-rich example of sharecropping as response to crises, is described in Section 3.4, whereas this particular use of share contracts is contrasted to the standard explanations of sharecropping in Section 3.5. Section 3.6 concludes.

3.2   Theories of sharecropping

Models of agricultural contract choice are generally set up as principal-agent models wherein both tenant and landlord freely optimize income given several contract margins and given their preferences and existing production technologies. Doing so, any concluded contract is by construction at least as attractive to both parties as their alternative income options. The margins of a lease contract are, at least, stipulations regarding remuneration—which can be fixed payments and transfers proportional to production in both directions—the amount of land in the contract, the duration of the contract, and stipulations regarding effort and investments made by both sides in the bargain. Accordingly, agricultural contracts are often interpreted as connecting several markets such as those for land, labor and insurance (Hallagan 1978; Braverman and Stiglitz 1982; 1986; Mitra 1983).[4]

    All these contract margins have individual and, often, combined effects on risk-sharing and incentives that influence the multiple aspects of agricultural production—which often depends on several inputs and yields several outputs, as well as balancing short term against long term objectives. The most straightforward ones are that, given uncertainties in output, remuneration determines the distribution of risk between the parties while proportional transfers distort productive incentives. The party receiving a fixed rent or a fixed wage is insured against fluctuations in output, whereas proportional

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[4] The literature in sharecropping is very large and varied. Consequently, a survey of the entire literature is beyond the scope of this paper. More extensive surveys can be found in Singh (1989) and Otsuka et al. (1992). A slightly more focused but clear review is provided by Federico (2006). Note, furthermore, that this Section as well as most of the literature compares fixed rent and share contracts, and is not about wage labor.

transfers imply that both parties are affected by fluctuations. Furthermore, proportional transfers distort the marginal return on each factor input in the production process. Under sharecropping, each additional unit of labor provided by the tenant increases production, but this increase in production has to be shared with the landlord. The same is true for the inputs provided by the landlord. Fixed rent, on the other hand, leaves the entire marginal product with the tenant whereas investments made by the landlord will be reflected in the price of the rent (Cheung 1968; Otsuka et al. 1992).

Importantly, as long as contracts are perfectly enforceable without costs there are no differences in outcomes between contracts. Cheung (1968; 1969) demonstrated this theoretically by showing that the disincentive implied by share contracts can be compensated through other margins in the contract, such as the size and number of plots per tenant or through inputs supplied by the landlord, to yield exactly the same outcomes as a fixed rent contract in terms of inputs, output, aggregate utility and distribution of utility. Even if there are some costs to observing and enforcing contracts, Stiglitz (1974) and Newbery and Stiglitz (1979) showed that although output risk is differentially distributed in different agricultural contracts, optimal contracts can be derived by mixing contract types as well as through using appropriate shares in share contracts. Unsurprisingly, all subsequent models of sharecropping start with some plausible deviation from the ideal world of perfect information and perfectly enforceable contracts.

Contracting problems arise because not all contract margins are observable with the result that the exact amount of inputs provided by each agent involved in the contract is private information to the provider.[5] For example, suppose a landlord cannot observe labor input provided by the tenant directly but only through an observable final outcome, which depends on chance as well as factor inputs. Knowing that the outcome depends to some extent on labor input, the landlord wants to compensate the tenant based on the observed final production. However, this leads to a chance element in the tenant's final income, for which he, as a risk-averse agent, wants to be compensated. Share contracts, although suboptimal in terms of their implied incentives to production, might be preferred over fixed rent in such situations (Stiglitz 1974; Allen 1984; Otsuka et al. 1992). In such a setting, the contract choice becomes a balancing act between providing incentives and distributing output risk (Grossman and Hart 1983; Holmström and Milgrom 1987).

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[5] In a setting where production uncertainty and asymmetric information exist and where agents are risk averse, 'optimal contracts' do not exist and contract choice is highly relevant—and, importantly, the 'second best' contract might be quite different from the optimal, perfectly enforceable contract (Lipsey and Lancaster 1956). For instance, mixing contracts or observing the correct sharing of output is costly in such a situation and, therefore, inefficient (Bardhan and Srinivasan 1971).

Much of the theoretical literature on sharecropping revolves around moral hazard and monitoring costs in contracts wherein output requires the combination of several, potentially conflicting inputs (Allen and Lueck 1995; Jacoby and Mansuri 2009). Of course, agricultural production involves more than just the labor input of the tenant. It involves decisions regarding non-labor factor inputs, regarding farming intensity, and regarding investments in farming capital. Put differently, it entails several trade-offs between direct profit and long term productivity of the farm, and involves various investment decisions and potential restrictions on output. Given imperfect monitoring and enforcement, many of these decisions are ultimately made by the tenant but affect the landlords (future) income. The landlord cannot observe these decisions directly and needs to incur costs if he wants to monitor the tenants choices. Once the landlord monitors its tenants to ensure that they cannot hide part of their produce, he simultaneously monitor the tenant's effort on the farm, thus reducing the main theoretical disadvantage of share contracts—i.e. that share contracts distort incentives—(Lucas 1979). Hence, share contracts are less problematic in types of agricultural where monitoring intensity is high for additional reasons (Hoffman 1984).

Eswaran and Kotwal (1985) model share contracts as a partnership-like contract when risk-neutral landlords and tenants provide complementary inputs that are difficult to contract otherwise—see also Reid (1976). Similarly, share contracts might be preferred when one party faces moral hazard along conflicting inputs at the hands of the other agent. For instance, when a landlord cannot perfectly observe tenant effort and the exploitation techniques used by the tenant, a fixed rent contract would induce a tenant to supply effort optimally but also to use exploitation techniques that could be suboptimal for the landlord (Ghatak and Pandey 2000). The risk of over-exploitation becomes problematic especially when tenants are credit constrained and cannot pay their fixed rent when output is below average. In that case, tenants might engage in excessive risk-taking since they profit from high output but cannot pay when output is low; a case where share contracts are optimal compared to fixed rent (Basu 1992; Sengupta 1997). Others have shown that *ex ante* limited liability—the inability to pay for production enhancing inputs or to pay an upfront rent to the landlord—in combination with moral hazard in tenant effort can also explain sharecropping (Shetty 1988; Laffont and Matoussi 1995; Ray and Singh 2001). Jacoby and Mansuri (2008) associate sub-optimal investment on tenanted land with moral hazard on the part of landlords—*ex post* unwillingness to honor contracted investment in the farm—for which share contracts are a better remedy than fixed rent (Eswaran and Kotwal 1985; Ray 2005). Dubois (2002) shows conditions wherein the reduced productive incentives in share contracts can be used to maintain the fertility of land, a prediction in line with

previously discussed models of sharecropping and vulnerable crops—although in this case, the soils are vulnerable, not the crops. Importantly, all these theories predict that landlords use fixed-rent contracts for their richer tenants and share contracts for the poorer.

All these models require several simultaneously operating market imperfections for share contracts to be optimal in some range of the imperfections (Huffman and Just 2004). The combination of risk-aversion and moral hazard is one such combination of imperfections (Stiglitz 1974), another is due to tenants having to perform several tasks with contradicting effects on landlord incomes (Ray 2005; Dam and Perez 2012), or when moral hazard is combined with limited liability (Laffont and Matoussi 1995; Ghatak and Pandey 2000).[6] Note that problems related to limited liability and risk aversion suggest that share contracts are more likely for poor tenants, for risky or vulnerable crops, and in regions were there are no capital or insurance markets (Hoffman 1996).

Importantly, even in cases where the conditions exist that make sharecropping a plausible alternative to fixed rent, the choice for share contracts is not obvious. First of all, landlords and tenants do not end up in a contract by chance. Instead, landlords select tenants based on their expectations of the behavior of that particular tenant (Ackerberg and Botticini 2002). Accordingly, if landlords and tenants select into a contract, then share contracts might be less often necessary to solve any of the above mentioned problems (Prendergast 2002; Ghatak and Karaivanov 2014). Second, because agricultural contracts entail several margins through which the incentives of tenants are affected, several of the contracting problems can be solved by specific clauses in the contract type that at first seems sub optimal (Bandiera 2007). For instance, Arimoto (2005) discusses a type of fixed rent contract used in early 20th-century Japan that strongly reduces limited liability and risk aversion problems due to an explicit rent-reduction for bad harvests, when tenant rent-payment is changed into a share payment.[7] Third, and in addition to formal contracts and direct enforcement, contracting problems could be solved through informal arrangements such as informal sharing of knowledge about tenant behavior and productivity (Laffont and Matoussi 1995) or through informal

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[6] The large number of potential combinations of contracting problems further increases when including dynamic considerations into the analysis. For instance, tenure security stimulates tenants to improve soil quality and to better maintain long-lasting farm capital because they will be able to reap the benefits of the improvements, regardless of the exact type of agricultural contract (Banerjee, Gertler and Ghatak 2002; Jacoby and Mansuri 2008; Abdulai, Owusu and Goetz 2011; Deiniger et al. 2013).

[7] As will become evident in Section 3.3, a similar type of share contract existed in the Low Countries (Slicher van Bath 1957, 668-70; Roessingh 1968/69). Of course, such a contractual arrangement is difficult to enforce effectively (Mansuri 2007). However, as emphasized before, share contracts are more likely in situations where landlords monitor their tenants closely.

risk-sharing and credit arrangements (Townsend 1994; Udry 1994; Dubois, Jullien and Magnac 2008). Given so many contracting margins as well as contracting problems, it is perhaps unsurprising that empirical support for the theoretical predictions on the choice of agricultural contracts remains weak (Ackerberg and Botticini 2002; Prendergast 2002; Chiappori and Salanie 2003; Ghatak and Karaivanov 2014).[8]

There are several theoretically plausible setting wherein share contracts are more efficient than fixed rent. The empirical literature on sharecropping in modern-day countries is extensive although mostly focused on assessing the relative inefficiencies of the contract compared to fixed rent or peasant ownership.[9] The historical literature, in contrast, is particularly rich in addressing the questions raised in this paper: investigating under what conditions share contracts are used and what problems they solve. Studies on the historical use of sharecropping derive mainly from two cases: the post-bellum US South and the Mediterranean where share contracts were used from the medieval period until modern times. Focusing in the European case, Hoffman (1984, 1996) has argued that the expansion of sharecropping in early modern France can be explained by the difficulties that landlords experienced in attracting reliable tenants to their more remote properties. According to this argument, tenants were often poor, risk averse and unreliable, making share contracts preferable over fixed rent—in line with the idea that share contracts balance risk and incentives (Stiglitz 1974), and augmented with the claim that landlords need to monitor tenant behavior regardless of contract choice (Hoffman 1984). Similar arguments have been proposed to explain the proliferation of share contracts in central Italy (Cohen and Galassi 1990; Emigh 2009) and Catalonia (Carmona and Simpson 1999). Ackerberg and Botticini (2000; 2002) provide support for all the three main arguments in favor of sharecropping—i.e. balancing risk and incentives, solving moral hazard regarding multiple inputs, and solving limited liability problems combined with moral hazard.

Based on a more precise reading of the historical sources, Carmona and Simpson (2012) have argued that previous studies overestimate the widespread use of share contracts in Mediterranean wine-producing regions. According to them sharecropping is preferable to fixed rent only when—in addition to tenants being poor, risk-averse and prone to moral hazard—landlords need to attract tenants with production-specific skills. To do so, landlords need to provide family-sized and varied farms, and downstream-

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[8] The lack of direct empirical support is true for agency theory more generally and has resulted in a new wave of highly abstract contract theory—e.g. Biais, Mariotti, Rochet and Villeneuve (2010); Fisher (2013); Opp and Zhu (2015); Demarzo and Sannikov (2016).

[9] See, among many others, Abdulai et al. (2011) or Deiniger et al. (2013) and the work cited therein.

production facilities to attract and maintain such tenants. Even when these conditions are met, they argue that share contracts can only be preferable to fixed rent when harvest sharing is not too costly to monitor (Carmona and Simpson 2012)—see Carmona (2006) for an extension of these factors to explain sharecropping in mixed husbandry regions in central and western France (Carmona 2006). Garrido (2017) argues that the efficient use of sharecropping depends crucially on the ability of landlords to compensate tenants for improvements on the farm which, according to him is hardly possible outside viticulture. According to these studies, the range of contracting problems wherein share contracts are preferable to fixed rent contracts is even more restricted than theoretical predictions suggest.

An important objection to recent literature on sharecropping in Mediterranean viticulture (Carmona and Simpson 2012; Garrido 2017) is that this claim of limited use of the contract is built on top of a previous claim that sharecropping was not used in pre-industrial Europe outside Mediterranean viticulture at all (Hoffman 1984). However, a quick overview of more specialized literature on agricultural history shows that share contracts were not uncommon in the Low Countries (Bieleman 2010: 95, 109–115) and Germany (e.g. Kopsidis, Pfister, Scholten and Bracht 2017). Recent research has even revealed the use of sharecropping in England from medieval up to modern times (Griffiths and Overton 2009). Accordingly, the existing literature posits an empirical conundrum. Whereas, share contracts are theoretically useful in many settings, share contracts are not necessarily abundant even where many or all elements of such settings are found—that is, in underdeveloped regions with risky or vulnerable crops, and poor tenants. By examining the literature on the early modern Low Countries, the next Section underlines this sharecropping-conundrum. At the same time, it suggests that current empirical and historical literature on sharecropping may have too narrowly followed the emphasis of the theoretical literature on poor tenants, particular crops and particular regions.

3.3 Sharecropping in the pre-industrial Northern Low Countries

The rural economy of the early modern Low Countries is particularly well-studied, and is commonly recognized as a main driver of its flourishing economy (De Vries 1974; Van Bavel 2010). The role of lease markets has received particular attention in this process. Van Bavel (2001; 2009) argued that the uniquely transparent—in terms of the allocation of property rights—and competitive lease markets that existed in much of the Low Countries stimulated specialization and commercialization of agriculture

and, thereby, economic growth. Despite the demonstrated importance of lease markets, the richness of the existing literature and source material, and the remarkable regional variation within the Low Countries, sharecropping has received little systematic study by economic historians.[10] Even within agricultural history, sharecropping has received little systematic attention. Nonetheless, an overview of the literature reveals that it was in quite common use throughout the Northern Low Countries.[11]

A recent synthesis of Dutch agriculture since 1500 by Bieleman (2010) notes the common use of share contracts in the southeastern province of Limburg throughout the early modern period (115–16) and the occasional use on the poorer sandy soils of some of the inland provinces during the sixteenth century (133–34). In addition, sharecropping occurred with the introduction of tobacco and potato production, mostly in the central and inland province of Gelre (109–112), and it was used to contract poor families to the *droogmakerijen*—recent polders—in eighteenth century Holland (95). Finally, sharecropping was also occasionally used in livestock farming in Brabant and Gelre from the 15th up to the 18th century (Jansen 1955; Brusse 1999; Van Bavel 1999). Sharecropping was thus mostly restricted to the inland regions of the northern Low Countries. The one occasion where it did spread to Holland occurred after the economic heyday of the Dutch Republic. However, it is hardly the case that all inland provinces where poor or economically underdeveloped. As will become evident below, it is also not the case that sharecropping related to particular crops or to particularly poor tenants in all these cases.

In some cases of sharecropping in the Low Countries the theoretically predicted link with poor tenants and with volatile or vulnerable crops is clear—see Section 3.2—as, for instance, with potatoes. Potato cultivation spread relatively slowly throughout the Low Countries starting in the last decade of the 17th century. It remained confined to several less developed regions, such as Limburg, and poorer parts of relatively wealthy regions—specific parts of the river area in Gelre, which was relatively much poorer in the 18th than in the 16th century discussed in Section 3.4 (Brusse 1999); and

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[10] It has been studied by a long tradition of comparative economic history, that contrasts economic development in the Low Countries with that in other European regions, often favoring the well-defined property rights enjoyed by peasants in the coastal provinces as a main source of the flourishing of their rural economies (e.g. De Vries 1974; De Vries and Van der Woude 1997). Sharecropping is often used as the virtual opposite of the Dutch system: a backward system wherein poor tenants lack any productive incentives or incentives to improve their plots. More importantly, in this view, sharecropping hardly existed in the main provinces of the Dutch Republic (De Vries 1974: 15–16, 196–97).

[11] The Northern Low Countries coincide with the later Dutch Republic and roughly with the present-day Netherlands. Although most of this Section refers to this Northern part, some parallels with the rest of the Low Countries—roughly equal to present-day Belgium and Luxembourg—will be drawn. Regional references use early modern principalities, thus Gelre instead of the present-day province of Gelderland. Accordingly, Holland refers to the early modern principality—the County of Holland.

the new *droogmakerijen* in Holland (Bieleman 2010: 93–96). It took much longer before potato became a widely accepted staple food in the Low Countries, with the result that commercial profits from potatoes were uncertain (Vandenbroeke 1971). Unsurprisingly, potatoes were mostly produced by poor tenants using heavily labor intensive techniques on small plots of sharecropped land exclusively used for potato production (Vandenbroeke 1971; Bieleman 2010: 109). Share contracts provided an optimal way of balancing incentives for labor with the high risk involved in the mono production of a price sensitive crop (Stiglitz 1974)—see Section 3.2. A similar, although less widespread, case is found in small-scale labor intensive flax and onion production Zeeland-Flanders and the new *droogmakerijen* in Holland during the last decades of the 18th century (Bieleman 2010: 95).

The association of sharecropping with tobacco production is even stronger. Tobacco cultivation spread rapidly in several parts of Gelre from the 17th century onward (Roessingh 1976; Brusse 1999). Initially confined to the sandy Veluwe area—on the border between Gelre and Utrecht—it soon spread to the rest of the river area in Gelre too (Bieleman 2010: 111). Tobacco was a more specialized crop that, although labor intensive, required input of physical capital in the downstream production process—tobacco production required several labor intensive measures to adapt the crop to local climate (Roessingh 1976; Bieleman 2010: 73-74). Here, again, share contracts provided a balance between incentives and risk—as again, tobacco production on a small plot of land absorbed most of the labor capacity of a peasant household. In addition, the full production process of tobacco requires a range of factor inputs, where share contracts allowed a fluid integration of the different inputs—as in Eswaran and Kotwal (1985). Especially, this form of sharecropping aligns well with the conditions emphasized by Carmona (2006) and Carmona and Simpson (2012)—see Section 3.2. It should be noted that the share contracts used in tobacco production often entailed a fixed ceiling. That is, share tenants had the option to pay a fixed rent instead of their share payment, which allowed them to profit in good years while insuring them against downside risk (Roessingh 1968/69; 1976; cf. Arimoto 2005; Section 3.2).

Although the use of share contracts in the cultivation of particular crops, such as potato and tobacco, was not entirely restricted to underdeveloped regions, it was confined to poor tenants with little access to markets and living in the more marginal parts of these regions.[12] Sharecropping was more widespread for long periods of time does in Salland—the western part of Overijssel—and Limburg, two of the economically

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[12] Besides the regions mentioned in the text, sharecropping of potatoes existed in the recent, but relatively infertile polders of Holland and Zeeland. In addition, sharecropped tobacco existed in the river area of Gelre already in the mid-17th century while the region was still doing relatively well (Brusse 1999).

least developed regions of the Low Countries during the early modern period (De Vries and Van der Woude 1997). The social and economic fabric of this region contrasts starkly with that of the western coastal provinces. Noble and ecclesiastical landownership was very high throughout the early modern period (Van Bavel 2010: 243). Much of the rural economy was based on large-scale livestock farming but some parts of Salland were dominated by small-scale arable farming based on a range of crops and small numbers of livestock per household. In addition, the arable parts were relatively cash poor (Slicher van Bath 1957: 613–25). Sharecropping was clearly associated with arable parts of Salland and, according to Slicher van Bath, imposed a relatively high lease burden on tenants. However, he provides tentative evidence that here too share contracts included an often implicit fixed ceiling (1957: 555, 661–80).

J. Jansen (1968) suggests that sharecropping came to replace fixed rent contracts during the 17th century in southern Limburg, another economically underdeveloped region dominated by small-scale arable farming—even though the Limburg soils are quite fertile. This region often served as battleground for the numerous wars fought during the 17th century (Gutmann 1980). According to J. Jansen (1968), sharecropping was a suitable response to the continuous threat of warfare because it spread risk between tenant and landlord and it obliged the landlord to share the burden of new investments in production. The first share contracts mentioned by J. Jansen (1968), however, are registered in 1658. That is, ten years after the Treaty of Westphalia that ended the Eighty Years' War, and at roughly the same time that the French and Spanish crowns finally decided to end their continued fighting—with the Treaty of the Pyrenees in 1659.[13] In comparison to the use of share contracts along with the production of potato or tobacco—and with the historical literature discussed in Section 3.2—J. Jansen (1968) puts more emphasis on the investment-side of the contract. In particular, he emphasizes the importance of the landlord's investment in seeds and farm capital, which is a standard part of a share contract—see also Bieleman (2010: 115–16).

More implicit forms of sharecropping seem to have been in long use in Limburg. Sharecropping in Limburg, especially the forms that existed before the 17th century, took quite a peculiar form, because it often concerned only part of the crops and livestock on the farm and, arguably, even more related to investments. One form, called *hoevenarij*, implied that certain crops were produced in a sharecropping arrangement while fixed rents were paid on other crops and livestock (H. Jansen 1955; J. Jansen 1968; Ubachs 2000: 113). *Stalrecht* is a share contract wherein livestock is leased

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[13] The latest sign of hostile military activity in the region is the siege of the Venlo in 1646, however, while most of the subsequent military activity took place in Flanders and northern France (Van Besouw and Curtis 2018, see Chapter 1).

separately under some share arrangement. This type of contract came in several forms ranging from one wherein the landlord stocked part of his livestock on the farm against a proportional remuneration of the tenant, to forms wherein the tenant effectively bought himself into the landlord's livestock regarded as 'part' of the rented farm (Lindemans 1952: 232-33; H. Jansen 1955: 45–48). Unfortunately, little systematic research exists on the use and spread of these livestock-sharing contracts in Brabant and surrounding regions.[14]

Based on this overview of sharecropping in the Low Countries, I want to emphasize four conclusions. First, many of the instances of sharecropping in the early modern Low Countries included clauses that effectively blurred the distinction between fixed rent and share rent. This is clear from the use of fixed ceilings in share contracts which allow the landlord to insure tenants against risk, while providing strong productive incentives. It is even clearer from the mixed share-fixed contracts under *hoevenarij* and *stalrecht*. Second, and related, share contracts in the northern Low Countries seem to have been used to facilitate landlord investments in the farm, even though landlord investments could be arranged in fixed rent contracts as well. However, landlord investments under fixed-rent lease might be less convenient in some cases, for instance because the lease price of the improved farm becomes too high or because these investments are too sensitive to overexploitation by the tenant. Furthermore, landlords have stronger incentives to actually execute contracted investments on the farm under share contracts. In particular, if the landlord does not fully honor his contractual obligation to invest repeatedly or when renewed investments in capital are needed but not provided by the landlord, then a sharecropper can adjust his input accordingly—to the detriment of the landlord—whereas a tenant under fixed rent would still have to pay the fixed rent.

Third, sharecropping is often explicitly related to warfare in the literature. J. Jansen (1968) relates the transition to complete share contracts in Limburg to warfare in the 17th century, while Bieleman (1987: 179) makes a case for a temporary switch to sharecropping in response to warfare for late 16th and early 17th century Drenthe, another relatively underdeveloped economic region in the northeast of the Low Countries where sharecropping was rare. And even where the literature does not refer to war, as in the case of Salland during the 16th and 17th century, the expansion of sharecropping

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[14] Besides several isolated mentions (Heerman 2006; Van Onacker 2016: 177–78), the main source of evidence is H. Jansen (1955), which provides a wide-ranging overview of leasehold in Brabant. It is focused on the 14th and 15th centuries and not specifically devoted to sharecropping. It is clear from the sources mentioned in the text, however, that *Stalrecht* was geographically quite widespread. This in contrast to the name at which it is often cited in the literature: *Kempisch stalrecht*, in reference to its origin in the Campine region in Duchy of Brabant.

coincides clearly with periods of warfare in the area.[15] Fourth, rather than fully aligning with theoretical predictions on the use of sharecropping, the pattern of sharecropping in the Northern Low Countries often includes poor tenants but sometimes also livestock farmers. Furthermore, the contract was used for a wide range of crops and, again, with livestock. And importantly, several of the regions associated with sharecropping were among the economically more developed ones—while the early modern Northern Low Countries as such were at the forefront of economic development in that period (De Vries and Van der Woude 1997).

A conspicuous example of the use of share contracts in relation to warfare is the use of share contracts among tenants in the River Area of Gelre during the 15th, 16th and 17th centuries (Van Bavel 1993; Brusse 1999), a fertile part of the Duchy of Guelders along the main rivers in the center of the Northern Low Countries (Van Bavel 2010; Curtis 2014). Large landownership dominated property relations in this area throughout the later Middle Ages and the early modern period.[16] Feudal impositions, however, disappeared early in this area, from roughly the 12th century onward (Van Bavel 2001; 2010). Stimulated by a legal framework that limited tenant claims to leased land and that fostered transparent claims on property, a highly competitive market for short-term leased land emerged during the 14th century in this area (Van Bavel 2001; 2008).

The western part of the region, including the swampy area bordering Holland has been studied by Van Bavel (1999; 2001), while Brusse (1999) studied the eastern part of the area. Both authors stress the importance of a competitive lease markets dominated by fixed rent contracts of around 10 years in duration in this region—although this changed somewhat with the introduction of tobacco cultivation in the 17th century in particular villages in the area (Roessingh 1968/69; Brusse 1999: 137). In addition, and importantly, both authors mention the occasional use of sharecropping by large livestock tenants and link this to periods of crisis. As part of a short discussion of sharecropping, Brusse (1999: 137) provides two examples of large tenants switching

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[15] In particular, the information used by Slicher van Bath (1957) derives mostly from tax registers dated in specific years. The use of sharecropping expands markedly between 1583, during the chaotic first phase of the Dutch Revolt, and 1601 when the Eighty Years' War was well underway but had moved away from Salland for quite a few years (Slicher van Bath 1975: 555). Similarly, the relative number of sharecroppers around 1520 was particularly large. The broad outlines of military activity mentioned here can be found in Struick (1960) and Tracy (1990)—early 16th century—Van Nimwegen (2010), and Van Besouw and Curtis (2018) or Chapter 1—late 16th and 17th centuries.

[16] Most of this area was occupied during the early Middle Ages and cultivated along manorial lines with the result that ecclesiastical institutions and the nobility owned roughly three quarters of cultivable land, peasants roughly fifteen percent, while the remainder was owned by burghers from the many small towns in the area (Van Bavel 2001).

from fixed rent to share contracts during periods of warfare.[17] Similarly, based on the extensive lease accounts of Mariënweerd Abbey, Van Bavel (1993: 388–89) provides a short overview of the use of share contracts to show that such contracts were used sparsely during the 15th and 16th centuries but that their use seems linked to periods of crises and that sharecroppers are usually large tenants.[18] The long-term analysis performed by Van Bavel provides only tentative evidence for the use of share contracts by Mariënweerd Abbey. Although the lease documents of the Abbey span several centuries, the information is concentrated in particular periods within those centuries— ca. 1430, ca. 1450–60 and ca. 1490–1500. Several tenants leasing plots on share contracts can be found around 1450 and one more in 1490. However, all the periods covered in the ledgers can be associated with crises.[19] Based on a close investigation of the lease practices of Mariënweerd Abbey during the 16th century, however, Section 3.4 provides further detail and evidence on the general observations on sharecropping in the early modern Northern Low Countries.

3.4 Sharecropping at Mariënweerd Abbey

This Section sets out to detail three things. First, it further investigates the link between warfare and the adoption of share contracts by Mariënweerd Abbey. Second, it shows that share contracts were used exclusively for large tenants with farms of 45 to 90 hectares, but that these tenants worked side-by-side with similarly large farmers, in the same economy and with the same mix of crops and livestock. Even tenants with much smaller plots—for whom the Abbey always used fixed-rent contracts—seem to have used the same mix of crops and animals. Put differently, the sharecroppers of Mariënweerd Abbey cannot be set apart from other tenants of the Abbey along the lines of standard theoretical predictions. Third, it highlights the prominence of several forms of investments in the lease dealings of the Abbey and, in particular, the stipulations

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[17] The first is from the 1670s with explicit reference to the ravages of the Franco-Dutch war (1672–78). The second occurs during the late 1690s without explicit reference to a crisis. However, the 1690s is another period of known crises due to the Nine Years' War (1688–97) and major harvest failures throughout northwestern Europe (Curtis and Dijkman 2017; Alfani and Ó Gráda 2017).

[18] The lease accounts of other large landowners in the region point in the same direction as those of Mariënweerd Abbey (Van Bavel 1999: 555–56).

[19] During the 1430s, the Abbey and its tenants had to cope with repeat flooding as well as with the political and military turmoil of succession wars in the County of Holland that repeatedly crossed over to neighboring principalities, such as Utrecht and Gelre. However, the political turmoil had not disappeared in the second period, now with Utrecht as the center of action and also affecting other parts of the Low Countries (Ter Braake 2009). The end of the Burgundian Succession Wars and the start of the Guelders Wars affected the region directly during the final decade of the 15th century and the early decades of the 16th (Haemers 2009).

regarding extensive investments in livestock in the actual share contracts. In order to do so, this Section starts with a brief discussion of the sources left by the Abbey, and the economic and temporal context, because periods of warfare affected the availability of source material as well as the lease practices. This is followed by a closer examination of the rich lease accounts.

The extensive archives of Mariënweerd Abbey are dominated by accounts concerning the Abbey's lease practices which, taken together, span several centuries, although the detail on the execution of leases varies over time (Van Bavel 1993). The 15th century accounts are ledgers that provide a detailed, plot by plot accounting of the tenants of the Abbey, but limited detail about the actual execution of the lease and limited possibilities to trace continuity of tenants. The source material of Mariënweerd Abbey for the 16th century is much richer.[20] There is, however, a clear gap between the ledgers from the 15th century—which end during the 1490s—and the lease accounts starting from ca. 1530. There is no documentation to suggest that internal reasons could be the cause of this. Surely, the Abbey experienced all kinds of minor changes in its internal administration often associated with transitions between abbots. The first lease books of the 16th century should be dated to 1531 and are written by the new abbot Jan Sterck van den Berghe (abbot from 1530 to 1539). However, most of the leases in these accounts are signed by a previous abbot, Herman van Rossem (abbot from 1513 to 1529).[21] Another lease document from this Herman van Rossum with registered leases from 1517 onward survived (AM 76). This small document with notes is far from a complete ledger as used during the 15th century and seems to have functioned as an auxiliary document. It has the same structure as the later lease accounts by Jan Sterck—except that it misses general lease stipulations—as it describes the plot, the tenant and often the previous tenants, and lease conditions followed by payments. Several entries from this document are copied word for word in the lease accounts of Jan Sterck.

It seems reasonable to take Herman van Rossum's lease policy as the foundation for the lease accounts starting around 1530, instead of attributing the break in lease accounting to the transition of abbots. However, that still leaves the gap in lease

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[20] The ledgers with lease information can be found in *Gelders Archief, Arnhem; Abdij Mariënweerd te Beesd 2* (AM) 67, 68, 69 and 116, respectively. For the lease accounts of the 16th century, see AM 70-75. Whereas the ledgers have a clear chronological ordering, the lease accounts in AM 70-75 strongly overlap in terms of temporal coverage and general content. The documents of the Abbey can be accessed online. Therefore, to facilitate reproduction of the results, the page references to the documents refer to the scan numbers of the online documents.

[21] There was one abbot in between: Hendrik van der Voort, 1529-30. This abbot is forced out under pressure from Charles of Gelre, who supported the appointment of Jan Sterck van den Bergh (Van Bavel 1993: 360).

accounting to be explained. After all, Mariënweerd Abbey has left a centuries-long trace of its leases (Van Bavel 1993). The small lease document of Herman van Rossum (AM 76), fortunately, provides a temporal pattern that suggests another clear cause for the dearth of lease documentation during the first two decades of the 16th century. The document registers 44 new leases—and several entries with lease extensions of those 44—of which 23 start in the years from 1517 to 1520. A further 7 new contracts start between 1521 and 1525; the final 14 and numerous contract extensions start from 1528 onward, while there are no new contracts and only two contract extension in the years 1526 and 1527. In addition, payments are registered exclusively for the years 1520–25 and then from 1528 onward. The implied temporal pattern is that of a gradual initiation of lease accounting from 1517 onward, interrupted in the mid 1520s and followed by an explosion of lease accounting in the late 1520s that culminated in the new lease books of the early 1530s—which register over a hundred leases for just the years 1528-1532 (AM 70; 71). This temporal pattern closely resembles the developments of military conflict in the river area of Gelre.

The Burgundian Wars of Succession, ignited by the death of Charles the Bold of Burgundy in 1477, resulted in a series of revolts throughout the Low Countries that kept Maria of Burgundy and Maximilian of Austria—the heirs of Charles the Bold—occupied for several decades, and that brought them into an almost continuous war with France (Haemers 2009). Gelre was one of several principalities that revolted against the Habsburg succession of the Burgundian Low Countries through Maximilian of Austria. But in contrast to the rest of the Low Countries where the Habsburg affirmed their control during the 1490s, Gelre successfully retained its independence from 1477 up to 1543, despite almost continuous open conflict with the Habsburg rulers of the Low Countries (Struick 1960; Tracy 1990).

The first three decades of the 16th century, saw almost continuous warfare between Charles, duke of Gelre, and surrounding Habsburg principalities (Tracy 1990). Charles of Gelre adopted an aggressive policy, intervening in the northern principalities Friesland, Groningen, Drenthe and Overijssel over which he gained control during much of the 1510s and 20s. In addition, his armies engaged in raiding campaigns throughout the Low Countries. At the same time, Gelre was subjected to numerous invasions and raiding campaigns between 1502 and 1518, and in 1527-28 (Struick 1960). The river area itself was particularly hard hit for two reasons. First, the region was of strategic importance because it bordered Brabant and Holland, two of the most important and economically most developed principalities in the Habsburg Low Countries, and because of the economic importance of the rivers crossing the region. Second, the semi-independent lordship of Buren was located in the middle of the river area.

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This small but strong county remained loyal to the Habsburgs and functioned as one of their local strongholds. Mariënweerd Abbey was located within a few kilometers distance to Buren and maintained strong ties to the Counts of Buren (Van Bavel 1993, 173-76).

It is perhaps unsurprising that the lease accounts of Mariënweerd Abbey are completely silent with regards to the years between 1502 and 1517. Although it seems impossible to completely reconstruct all military activity of this war-period, it is clear that the fighting between the Duke of Gelre and the Habsburgs heavily affected the river area. Over the years, several nearby towns were attacked and the river area was continuously raided.[22] The most evident occasion is the flight of the canons of the Abbey and many of their tenants into the walled city of Culemborg in 1516 (Van Bavel 1993, 390). The subsequent years proved more peaceful. Peace negotiations between Gelre and Habsburg were initiated in late 1517 while the focus of military activity moved towards the northern principalities (Struick 1960, 256-80). Interestingly, the Abbey started recording new leases precisely during this period from the end of 1517 onward (AM 76).

Diplomatic relations between the Duke of Gelre and the Habsburgs turned explosive again in 1525-26 as peace negotiations stalled and the Duke's covered operations in neighboring principalities escalated (Struick 1960, 291-98). Finally, outright warfare returned to the river area following the Duke's intervention in a political crisis in Utrecht in 1527, now with clear evidence of the heavy damages inflicted on the Abbey, its canons and its tenants. In a note that should be dated at ca. 1531, abbot Jan Sterck van den Berghe described the looting of the Abbey by a Habsburg-Utrecht army in 1527 (AM 71, 5). Several buildings of the Abbey were torched and the canons were forced to pay three large contributions totaling 2,800 guilders and approximately 600 guilders worth of malder and costly textiles—see also Van Bavel (1993, 390). Military activity remained concentrated on the river area throughout 1528, for instance with the siege of the nearby town of Tiel, forcing the canons again to flee to Culemborg. Ultimately, the Duke of Gelre was forced to settle for peace in 1528 (Struick 1960, 331-50).

Although the peace of 1528 did not end the Guelders Wars completely, there was no more military activity in the river area until the final, short-lived phase of the

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[22] A detailed history of the wars between Charles of Gelre and the Habsburgs can be found in Stuick (1960). Among many other incidences of conflict, he mentions several conflicts near Mariënweerd. 1505: Capture of Arnhem and several towns in the Betuwe, (Struick 1960, 64). 1506: Raiding in Land van Heusden (107). 1508: Poederoyen, raiding in Veluwe (124). 1509: Sack of Cuijk; raiding in the Betuwe (151). 1511: Capture of Zaltbommel (198). 1512: Raiding campaign in Bommelerwaard (209). 1513: Capture of Tiel (215). 1514: Capture of Arnhem (225). 1517: Raiding campaign of Frisian mercenaries through Holland, culminating in the destruction of Asperen (255).

Guelders War in 1542-43, when the new Duke of Gelre lost the duchy to the Habsburgs. The years from 1528 up to 1566 were, therefore, years of relative quiet with the occasional disturbance of violence—as in 1542-43—or flooding. The Iconoclastic Fury (1566) and the subsequent Dutch Revolt, however, marked the beginning of the end of the catholic Mariënweerd Abbey—(cf. Van Bavel 1993, 329-33 for an impression of the damages inflicted by Protestant rebels on the Abbey in the year 1567). The reconstruction of the lease activities between 1520 and 1550 studied here, should thus be seen in light of these periods of crisis.

3.4.1   Reconstructing the lease accounts in the wake of the Guelders Wars

From 1528 onward, following the devastating military events of the previous year, abbot Herman van Rossum and his successors managed to lease out most of the Abbey's land within a few years. The landed estate of the Abbey comprised of 3,600 *morgen*—a *morgen* is 0,86 hectare, hence roughly 3,100 hectare. Of these lands, about 1,100 *morgen* were located at relatively large distance from the Abbey in regions with different judicial systems. Another 400 *morgen* of the lands in close proximity were directly farmed by the Abbey (Van Bavel 1993, 288). The remaining 2,100 *morgen* near the Abbey were leased out in plots of various sizes that, in the 16th century, were regularly combined or split into new plots. Detailed lease accounts from the Abbey remain that describe the leases contracted in the years from 1528 to 1532, at which point most of the Abbeys landed estate had been leased out again. During the mid 1530s, some small new leases and many contract extensions are noted down in the accounts. This is followed by a period, from roughly 1540 up to 1545, wherein many new leases and renewals are written down, but in a lease book that covers only part of the landed property of the Abbey that it leases out. These years coincide with another period of political turmoil around the Abbey: the final episode of the Guelders Wars in 1542-43.[23] The lease accounts continue from the late 1540s up to the end of the 1560s when the onset of the Dutch Revolt devastated the area.

In contrast to the detailed lease accounts, the archives of Mariënweerd Abbey

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[23] The 1542-43 war between Gelre and the Habsburgs was a sharp but short-lived military conflict and was followed by the incorporation of Gelre into the Habsburg Low Countries. Damages to Mariënweerd were relatively limited. This can be inferred from a correspondence between abbots of Mariënweerd and Berne—a sister Abbey located at the opposite sites in the conflict. The abbots account for the losses incurred during the conflict, and the share of those losses incurred on behalf of one another—the Abbey's used each others contacts to buy protection from both sides of the conflict. The abbot of Berne lists total losses due to the conflict at 1037.9 guilder, 625.75 of which were incurred on behalf of Mariënweerd. However, Mariënweerd had incurred a similar amount and, consequently, these debts were canceled (Hoevenaars 1887).

contain only a limited amount of information concerning the general financial situation of the Abbey during the 16th century. Nonetheless, several documents survive that underline the troubled financial position of the Abbey following the events in 1527. The Abbey was forced to contract an unusual number of large debts during this period. It contracted 10 annuities with a total worth of 1,400 guilders at 6.25 per cent interest, directly following the flight of the canons to Culemborg in 1527. Seven more annuities were sold at 5 per cent interest in the following years with a total worth of 3,360 guilders (Van Bavel 1993, 527-33). Considering that the average rental price for one *morgen* of land around 1530 was slightly more than 2 guilders—as will become clear below—the total worth of the rents sold, 4,760 guilders, was a huge sum—more than the total annual rent of the 2,100 *morgen* of landed property that the Abbey leased out. Accordingly, these loans can only be understood in the context of the heavy 'contributions' imposed on the Abbey in 1527, and subsequent costs associated with reconstructions, recovery of farming, and several extraordinary taxes levied during the years 1530 to 1533 (Van Bavel 1993, 380-84).

Information on leases is scattered over several different books that never cover the entire landed property of the Abbey and that cannot always be chronologically and spatially synchronized. Tenants are occasionally lost for several years, while plots were regularly bundled or separated. Nonetheless, a sample of 79 plots which can be traced over time—between 1519 and 1560—with constant size—on average 6.5 *morgen*, or 513.5 *morgen* in total—and consistent information on lease prices per *morgen* can be drawn from the lease accounts. The average number of observations for these plots is 3.55—hence 264 observations in total. The left panel of Figure 3.1 presents general trends in lease prices in this sample, based on a random-effects panel regression estimating the price for each year, and controlling for the size of each plot and contract duration—both are statistically insignificant, see Appendix A. Of course, the number of observations in this sample is limited, and not constant over time as shown in the right panel of Figure 3.1. The price trend and fluctuations should, therefore, be interpreted with caution. Nonetheless, it is clear that the average lease price increased from roughly 2 guilders per *morgen* around 1530 to 4 guilders in the 1550s, in a period when inflation was minimal—see Figure Appendix A. Furthermore, a clear and relatively precisely estimated drop in lease prices is visible following the war periods of 1527-28 and 1542-43.[24]

The information in the accounts concerning lease stipulations, actual practices

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[24] Although none of the drops in the price trend are statistically significant, and almost all yearly estimates depend on very few observations, the drops following the war periods are relatively precisely estimated. The other large drops in 1534 and in 1558 are much less precise and dependent on just one observation.

Figure 3.1: Development of lease prices based on sample of plots
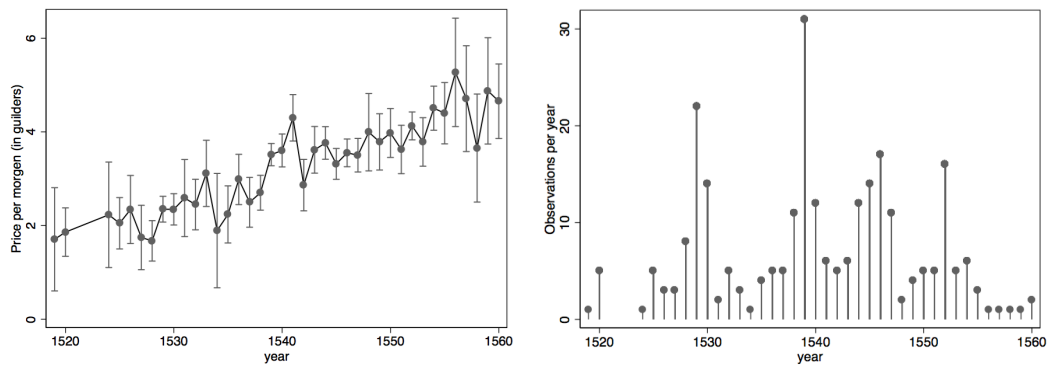
Note: The left panel of the Figure presents a graphical representation of a random effects regression of the lease price of newly contracted plots, per *morgen*. The right panel indicates the number of new contracts per year as an additional expression of the precision of the estimates in the left panel. In particular, it shows that the first half of the 1520s and the mid 1530s are estimated quite imprecisely. *Sources*: The results are based on the same of plots of constant size that can be consistently trace through the lease books. See the main text for an explanation.

and payments is surprisingly rich. These entries generally start out with a description of the land leased to a particular tenant—often comprising several plots—the rent to be paid, the starting year and date and the duration of the rent. In addition, the entries often mention the previous tenants of the plots, and additional payments such as a tax-payment on the use of the Abbey's mills. The majority of the leases in the accounts are fixed rent contracts for plots of between 2.5 and 6 *morgen*, although tenants often combine plots. Most tenants farm roughly 4 to 8 *morgen*, but several tenants farm up to 20 *morgen*—in line with the sample of plots described above and depicted in Figure 3.1. Given that the annual rents are specified and usually registered in cash, it is not directly evident from the accounts what tenants produced. The actual payments provide more evidence, however, because payments were regularly made in kind and subsequently translated in a cash amount. These in-kind payments comprise of grains, butter, an occasional piece of livestock and this combination is surprisingly similar for tenants with small or large total of plots.

Usually, the entries for individual tenants are relatively short and refer to the general lease stipulations found in several account books—a full transcription of several of these general stipulations can be found in Appendix B.[25] These general stipulations further mention the dates of payments for maintenance of dikes adjacent to the plots, willows, ditches and other local infrastructure. Furthermore, the stipulations contain bans on subleasing and on breaking crop-rotation, some additional payments—a capon

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[25] These general stipulations can be found in several of the lease accounts (AM 70, 1; 71, 2; 74, 99; 77, 2).

per *morgen* and ten pounds of butter, half a pound of pepper and half a pound of ginger for tenants with more than ten *morgen*. And some more general statements, such as the requirement that tenants were to 'farm properly'. Importantly, the conditions state explicitly that under no condition will reductions on rents be allowed and that tenants should pay 'free money'. That is, tenants were required to fulfill the contractual rent stipulated by the Abbey free from any taxes, whether regular or extraordinary and regardless of the unit to be taxed, and free from deductions in case of war or flooding (AM 71, 2; 74, 99).

In practice, the Abbey's engagement with its fixed-rent tenants was much more adaptive than the firm contractual stipulations suggest. It used several strategies to aid its tenants in periods of adverse conditions. In some cases, it deviated from the stipulations of 'free money' by allowing aggregate rent reductions. This occurred during the war years of 1542-43 wherein all rents were reduced to 50 to 60 per cent of the original rent, and in in 1551 when major flooding occurred leading to complete rent acquittals for tenants in the most affected parts of the Abbey's property (AM 74; Van Bavel 1993, 374). Such rent reductions were quite rare and usually connected to periods of general crisis. Another strategy of the Abbey was that it allowed tenants to build-up substantial arrears. The Abbey kept detailed notes of the payments and arrears of all its tenants, which provide rich information on such arrears. Arrears usually stayed within the range of one annual rent of the tenant, but larger arrears were not uncommon either. Such arrears were usually paid off over the course of a few years, or through labor services performed by the tenant. Only in case of arrears that substantially exceeded one annual rent would the Abbey arrange formal debt negotiations with its tenants—often through a debt contract that forced the tenant to redeem the debt with a few annual installments. Importantly, only the largest arrears seem to have had a negative effect on the probability of lease extensions.[26]

In addition to the allowance of arrears, the Abbey used even more direct strategies to contract investments with its fixed-rent tenants. The lease accounts provide several examples of the Abbey reimbursing tenants for improvements of the land or buildings. Often, such improvements were arranged at the start of the tenant's term. Such stipulations mentioned the improvements to be made, which parts would be directly financed by the Abbey—usually building materials and wages. In other cases, all improvements were financed and performed by the tenant and were followed by

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[26] See, for example the lease extensions of Heijman Janssoen (AM 72, 46)—although a small lease, his arrears were worth more than the annual lease sum—and Jacob Artssoen (AM 73, 36–38), a larger tenant with several plots totaling roughly 15 *morgen* in 1545 who, even though his arrears amount to 145 guilder in 1548, contracts another plot of 4 *morgen* and extends his largest plot of 9 *morgen* in 1548 and 1549.

a valuation of the improvements at the end of his contract. Such valuations were performed by four 'independent men' from the region, two chosen by the Abbey and two by the tenant.[27]

Although direct investments contracted by Mariënweerd Abbey and its fixed-rent tenants were not very common, it is clear that the Abbey used several practical solutions to solve contracting problems. Restricting the discussion of contracting problems that are often related to sharecropping—see Section 3.2—risk sharing, the combination of several unobservable inputs, and limited liability can all be observed in these stipulations to some extent. The lenience in collecting arrears portrayed by the Abbey, in combination with generic rent reductions in periods of crisis, reduced negative income shocks to tenants. In addition, the Abbey's policies allowed tenants to continue farming beyond the occasional bad years. Accordingly, these policies reduced income risk to tenants and limited liability as contracting problems. As discussed in Section 3.2, however, the theoretical literature suggests that share contracts become preferable only when these contracting problems are combined with moral hazard on the part of the tenant concerning inputs in the production process. There is no reason to suggest that the Abbey could perfectly observe tenant inputs. However, the Abbey possessed intricate knowledge of local agricultural opportunities due to its own involvement in farming and its long record-keeping. As we will see, the Abbey did use share contracts for some tenants but there is no apparent reason to suggest that these tenants were more prone to moral hazard.

Aside from the plentiful leases of plots, the lease practices of the Abbey that emerge from the accounts of 1531 (AM 70-71) suggest a sharp difference with the practices of the 15th century for two reasons. First, the accounts mention several large farms located within the 2,100 *morgen* of farmland near the Abbey, which were leased out including a farmstead, whereas only one such farm, but much smaller in size, existed in the 15th century. Some of these new farms seem to resemble the granges used by the Abbey in the 13th and 14th century (Curtis 2016a, 168-70). Most of these large farms, however, are newly established around 1530.[28] All of these large farms, including the old granges, are referred to as 'leased farm', or *bouhof* from 1530 onward, rather than

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[27] See, for example, the four independent men to value the improvements of Aert Claessoen (AM 71, 37) and Goessen Aertssoen (AM 71, 93).

[28] The only farm that is mentioned during the 15th century is 'De Thielwael', as a farm-house with 24 *morgen* (AM 69, 1). The 15th century accounts regularly refer to the old granges, such as 'De Gansheuvel' or 'De Haag' but they do so as geographic location rather than as farm. The plots in those locations are dispersed among many tenants lease plots, while some of the larger tenants have plots in several of these grange-locations (AM 67; 68; 69).

the regular references to individual plots. In total, Mariënweerd Abbey leased out ten such farms around 1530 located near the Abbey and constructed another one in 1546. In addition, its more distant landed property was arranged in nine more farms, but it used a very different lease policy for the outlying property—an overview of these large farms can be found in Appendix C.[29]

Second, share contracts were exclusively used for farms, although not all farms were leased on share contracts. Regardless of the type of lease contract, these farms are usually accompanied by long and detailed lease conditions in the account books—several full transcriptions of these extended descriptions for the large farms, and all of the sharecropped ones, can be found in Appendix B. Among the first known lease conditions for the ten nearby farms, five were clearly sharecropped, two started on fixed rent but switched to share contracts around 1535, and the other three remained on fixed rent contracts—although contract details for the last three are scant—whereas individual plots were always given out on fixed rent during the 16th century with relatively standard contractual stipulations.[30] Table 3.1 provides an overview of the lease conditions used for the farms over time. Per farm it lists the known lease conditions such as the starting date and duration of the contract, the size of the farm—which was not stable over time—and the lease price.

One of the lease accounts compiled by abbot Jan Sterck in 1531 provides elaborate general conditions for farms leased out on share contracts (AM 71, 2). These general lease conditions applied to share contracts starting in 1530, which had a duration of six years. The conditions state that the Abbey and the tenant supply the farm with livestock—horses, cattle, pigs, sheep and bees—at an equal rate and that costs and benefits of these animals are shared equally. Although there are no references to such expenses made by the Abbey, it is clear from the lease accounts that animals, especially horses, were quite expensive—the value of a horse between 1530 and 1535 ranged from 10 to 30 guilders (AM 70, 7). The produce of crops was also split among tenant

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[29] The outlying properties of the Abbey around were located around Nijmegen, Mill and Hall, and organized in several farms of constant size for which the Abbey received in-kind payments-. These farms were generally leased for much longer periods of up to several decades, and often to local ecclesiastical institutions rather than individual farmers. Accordingly, and in contrast to the rest of the Abbey's property, these leases do not seem to have been influenced by competition among tenants. See Appendix C.

[30] As mentioned in Section 3.3 there are a few instances of sharecropping recorded for the 15th century lease accounts. There is a sharecropper with 4 plots totaling 35 *morgen* registered in the first ledger (AM 67, 8 or 67-1-f7v), but his contract starts in 1453 whereas other leases in that ledger start during the 1430s. Accordingly, this lease is repeated in the next ledger with leases starting around 1450 (AM 68, 23 or 68-3-f3r). In total, there are four tenants with share contracts in that period, and all but one are leasing relatively large amounts of land—one tenant with 100 *morgen*, one with 40 *morgen* and one leasing just two small gardens.

Table 3.1: Chronology of the 10 *Farms*

District	Farm name	1520s	1530s	1540s	1550s
Mariënweerdse Veld	De Neust	1528/30 + ? ? |Share	1536 + 6 104.7 *mor.* |Share	1542+6 ? |Share — 1548+7 76.3 *mor.* |3.50 f./*mor.*	1555 + 8 85 *mor.* |?
	Gansheuvel	1529 + 6 81.8 *mor.* |Share	1532 + 6 81.8 *mor.* |Semi-share — 1538 + 8 87.8 *mor.* |Semi-share	1546 + 8 76 *mor.* |3.50 f./*mor.*	1554 + 8 79 *mor.* |4.50 f./*mor.*
	Hoge Sluis	1525 + 11 65.3 *mor.* |ca. 1.50 f./*mor.*	1535 + ? ? |Share	1544 + 8 75 *mor.* |4 f./*mor.*	1552 + 8 78 *mor.* |4.25 f./*mor.*
	Hoge Spijck		1530 + 10 80.5 *mor.* |3.50 f./*mor.* — 1536 + 6 80.5 *mor.* |Share	1542/45 + ? 55 *mor.* |4 f./*mor.*	1553 + 8 55 *mor.* |4.5 f./*mor.*
	(No name)		1530 + 6 55 *mor.* |Share — 1536 + ? ? |Share(?)	1544 + ? 58 *mor.* |4.50 f./*mor.*	1552 + 8 58 *mor.* |4.50 f./*mor.*
Deil	(No name I)		1532 + ? ? |Share	1546 + 8 78 *mor.* |3.75 f./*mor.*	1554 + 8 78 *mor.* |4.50 f./*mor.*
	(No name II)	1529 + 6 ? |Share	ca. 1535 + ? ? |Fixed	1547 + 8 77.5 *mor.* |3.75 f./*mor.*	1555 + 8 77.8 *mor.* |4.50 f./*mor.*
Buren	Asch		ca. 1534 + ? ? |Fixed	ca. 1540 + ? ? |Fixed — 1546 + 8 49 *mor.* |ca. 2.75 f./*mor.*	1554 + 8 50 *mor.* |ca. 2.75 f./*mor.*
	In de Hage		1530 + 12? 75 *mor.*? |150 f.	1542 + 7 70 *mor.* |ca. 3.60 f./*mor.* — 1549 + 10 70 *mor.* |ca. 3.75 f./*mor.*	1554 + 4 70 *mor.* |ca. 4 f./*mor.*
	De Vogelzang	n/t n/t	n/t n/t	1546 + 10 42 *mor.* |ca. 2.70 f./*mor.*	1556 + 8 46.5 *mor.* |ca. 3.50 f./*mor.*
Waardenburg	De Thielwael	1528 + 10 55.5 *mor.* |ca. 1.35 f./*mor.*	? ?	1547 + 8 55.5 *mor.* |ca. 3.20 f./*mor.*	? ?

Note: The Table provides an overview of the contractual terms of all the commercially leased large farms of Mariënweerd Abbey, ordered by district. It provides the starting date, duration of the lease, the size of the farm and the payment terms for each farm—either share, semi-share for the farm that combined shared livestock with a fixed payment for arable production (see main text), or a fixed rent with payments in guilders per *morgen*. Not all the information is available for each farm. See Appendix B for transcriptions of the extensive lease accounts. See Appendix C for a reconstruction of the information of all the large farms.

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and Abbey at an equal rate, even though the Abbey did not share in the production. In addition, these share payments were supplemented by several stipulations meant to stimulate production and to insure the Abbey against moral hazard. In terms of livestock, the tenant was supposed to ensure a stock of as much animals as the farm could carry, and was prohibited to sell animals without consent from the Abbey or to keep animals from other farms on his pastures.[31] In addition, the tenant paid a fixed amount of butter per cow, and a fixed number of sheep milk cheeses depending on the size of the herd. In terms of arable production, the contract stipulated a fine of 2 Golden guilders (2.8 guilder) for each unused *morgen* of arable land, and a fixed payment of one 'row' of grain per sown *morgen* per year. In addition, the tenant was prohibited to harvest and to thresh grain on his farm. Instead, the Abbey would send laborers to first harvest and thresh the Abbey's share of grain. The Abbey paid the wage bill only for its own share of the harvest, while the tenant was supposed to provide board to the laborers. As with the fixed-rent contracts, subleasing was prohibited and there were minor additional payments although those for share contracts were somewhat higher.

Clearly, the stipulations regarding share contracts are more extensive than those for individual plots in fixed rent. And whereas the Abbey simply mentions 'good behavior' of tenants and payments of 'free money' to insure itself against deceit, the conditions for share contracts are full of stipulations ensuring that tenants work their farm intensively and that they cannot cheat the Abbey in terms of their payments. These stipulations are repeated in the individual lease entries of the share tenants of the Abbey that have been recorded. Furthermore, these conditions are proven relevant when the lease of one of the sharecroppers—Reijer Hermanssoen, farming 'De Gansheuvel'— is terminated in 1531 because he himself harvested all the grain on the farm. Such stipulations are hardly necessary under fixed rent, where the tenant has strong incentives to maximize production and where rent payments are much more transparent.

Besides these straightforward differences, however, the share contracts do not mention conditions or contracting problems that suggest a clear rationale for the use of share contracts instead of fixed rent. A full reconstruction of the large farms during from the mid 1520s to the early 1550s is provided in Table 3.1—the reconstruction itself is provided in Appendix C, while transcriptions of several extensive lease contracts can be found in Appendix B. As is evident from Table 3.1, the five farms in the

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[31] These conditions can be found in AM 71, 2—for a full transcription see Appendix B. 'Inden yersten soe salmen alle beesten, het zijn peerden, koyen, vercken, scapen of byen half ende half setten ende die schaden ende baten salmen gelijck draghen' ('in the first place, they [tenants and Abbey] will provide animals, whether horses, cows, pigs, sheep, or bees, at an equal rate and the costs and profits will be shared equally'); 'Een bouman sal holden alzoe veel beesten als sijn bouwinghe vermach' ('a tenant will keep as many animals as his farm affords'); 'Gheen vreemde beesten' ('no foreign animals').

direct surroundings of Mariënweerd—in the *Mariënweerdse Veld*—provide the richest sequences of information. For one of these farms, the first known contract is a fixed-rent contract from the mid 1520s—before the raid on the Abbey in 1527 but after the war-prone period up to 1518. For four of them, a new contract around 1530 is accounted; one fixed-rent contracts and three share contracts. Notably the share contracts on 'De Gansheuvel', the farm were the previous share tenant was evicted because of his fraudulent payment of his arable production, is changed into a share contract with fixed payments for arable production—a semi-share contract. All five are commercial livestock farms. For two of them, one sharecropped, the other on fixed rent, the contracts starting in 1530 mention extensive construction works on the farm with labor and materials paid for by the Abbey. Furthermore, differences in farm size seem incapable of explaining contract choice too. The farms that are sharecropped in 1530 tend to be slightly larger, but the smallest farm of the five is sharecropped too. Finally, the two farms rented on fixed rent change to share contracts before their initial contracts expire—for the 'Hoge Sluis' the date of change is not entirely clear but seems to have occurred one year in advance, while for the 'Hoge Spijck' the switch to a share contract occurred with four years of the initial contract still remaining. Hence, all five farms in the *Mariënweerdse Veld* ended up with a share contract by 1536, and four of them return to fixed rent in the early 1540s—only one engages in a new share contract in 1542, only to return to fixed rent in 1548.

The two farms around the nearby village Deil were sharecropped but little information exists about these contracts or the farms. Both farms contain roughly 78 *morgen* in the 1540s but it is not certain that they were of the same size around 1530. Furthermore, no contract extensions are detailed in the lease accounts beyond the contracts of 1532 and 1529 until new leases in 1546 and 1547. Some payments can be found for the period in between and these suggest that the contracts of roughly 1538 and 1535 are already fixed-rent contracts—implying that these two farms returned from share to fixed rent earlier than those in the *Mariënweerdse Veld*. In particular, the payments of these farms list 'the splitting of livestock'—although with little additional detail—suggesting that the tenant and the Abbey accounted for the removal of the Abbey's part of the livestock on the farm. Subsequent payments still include payments of livestock but only as direct payments from the tenant to the Abbey, however—listing, for instance, a number of cows followed by the full price of the animals. Payments in livestock under share contracts often referred to the sale of an animal by the tenant to a third party, a description of the value of these animals, followed by the amount of money due to the Abbey—always being half the sales price. The three other farms in the slightly more distant villages of Asch, Buurmalsen and Neerijnen were leased on

fixed rent and remained on fixed rent throughout the 1530s—although information for the farm in Asch is scant.

Accordingly, observable differences between these ten farms seem unable to explain why some of them were sharecropped while others were leased out on fixed rent. The three farms in Asch, Buurmalsen and Neerijnen were slightly more distant from the Abbey, but still located well within 20 kilometers. Of course, other factors not accounted for in the lease accounts might explain the differences in contracts. An obvious candidate would be farm quality. Directly inferring farm productivity, or even the value of a share contract relative to a fixed-rent contract is impossible from the share contracts. However, if the fixed rental prices of the three farms for which information around 1530 exists are a good reflection of farm quality, then quality might be important. Whereas Peter Willemssoen on 'De Hoge Sluis' and Jan Claessoen on 'De Thielwael' pay a low rent of roughly 1.5 and 1.1 guilders per *morgen*, Stees Krijnen pays a very high rent of 3.5 guilder per *morgen*—these lease prices can be compared to the ca. 2 guilder per *morgen* for the normal plots given in Figure 3.1. These contracts, however, are dated in different years—1525, 1528 and 1530 respectively. A comparison of the rental prices of these farms in later periods, across years that are more comparable than 1525 and 1530, shows little difference between these farms. The expensive farm of Stees Krijnen is substantially reduced in size—from 80.5 to 55 *morgen*, now called 'De Hoge Spijck'—which is rented for 4 guilder per *morgen* in 1545 and 4.5 guilder in 1553. Plots that have been subtracted from this farm are generally leased out for 3 guilder per *morgen* in 1545, suggesting that the most valuable plots are retained in this farm. 'De Hoge Sluis' was also rented at 4 guilder per *morgen* in 1544 and 4.25 guilder in 1552. 'De Thielwael' was rented in 1547 for 3.25 guilder per *morgen* and thus somewhat cheaper, but the difference in lease prices is quite small. Furthermore, differences in fixed rents between the farms in the 1540s when all have fixed-rent contracts—and prices are observed for all farms—are quite similar.

The clearest difference in terms of sharecropping is, therefore, between the contracts of the large farms and the normal plots which are all leased out on fixed rent. As emphasized before, however, the lease practices of the Abbey concerning the fixed-rent leases of individual plots reflect the same contracting problems that are usually associated with sharecropping. Issues of limited liability and the risk associated with farming are strongly reduced by the Abbey's willingness to allow arrears and the use of generic rent reductions in periods of crisis. In addition, the Abbey contracted improvements of buildings and infrastructure with its fixed-rent tenants in the same way as it did with the large sharecroppers—with the Abbey stating the desired improvements and financing of materials and wages. What then explains the choice of share contracts

for the large tenants over a short period of time?

3.5   A rationale for the share contracts at Mariënweerd

The temporary use of share contracts by several tenants of the Mariënweerd Abbey is difficult to explain based on the theoretical predictions discussed in Section 3.2. Theoretically, share contracts are preferable to fixed-rent contracts when a landlord wants to combine productive incentives with limited risk to the tenant, when the tenant provides multiple contrasting inputs or when tenant and landlord both provide inputs, or when the tenant is credit constrained and therefore liable to engage in risky activities or unable to arrange upfront investments. Hence, sharecropping is more likely with poorer tenants, with farming of volatile or vulnerable crops—or trees or animals—wherein landlords need to monitor their tenants regardless of the contract choice, and in regions with relatively underdeveloped capital and insurance markets.

In the case of Mariënweerd, all these conditions fail to explain sharecropping. In particular, the actual sharecroppers are the largest tenants of the Abbey and capable of financing half of the required livestock for these large farms. Furthermore, the crops and animals produced by the sharecroppers were the same as those produced by the fixed-rent tenants. For these crops and animals, simple contractual stipulations in the fixed-rent contracts such as the prohibition to graze other animals on the farm, to break crop rotation and, possibly, clauses regarding grazing damages to fields and infrastructure were enough to insure the Abbey against moral hazard—in particular, against damages to the long-term productivity of the farm. Monitoring became an issue only once share contracts were adopted by the Abbey, as it needed several additional stipulations to ensure tenant productivity, the rightful splitting of harvests and income from animals, and proper maintenance of the Abbey's livestock on the farm. And, finally, the Abbey managed to borrow 4,760 guilders at modest interest rates in a few years, right after the Abbey had been pillaged—see Section 3.4 above. It is likely that tenants had less access to capital markets but this should especially affect the poorer tenants. Nonetheless, there is nothing to suggest that capital markets and insurance markets in the river area were underdeveloped.

Hence, why were share contracts Mariënweerd Abbey and some of its largest tenants? And why were they optimal for a short period of time—from the mid-1520s up to the late 1530s—and only for large tenants, even if not for all large tenants? Apparently, for the large tenants, leasing a large farm on a share contract was more lucrative than leasing the same farm on fixed rent during the 1530s. This is particularly evident from the two tenants of a farm switched to a share contract in the mid-1530s—

in addition to the five tenants that already had a share contract. And, given that all farm-tenants switched back to fixed-rent contracts around 1540, the reverse statement holds for the 1540s.

Accordingly, the proliferation of share contracts is chronologically closely associated with the Guelders Wars in the sense that share contracts were used in the wake of the devastating war-periods of the late 1510s and the mid 1520s. In 1516 and in 1527, Mariënweerd Abbey was attacked and pillaged. The sources provide no information about the damages of 1516, but those of 1527 were very high—see Section 3.4. As the reconstruction of the lease accounts suggests, the recovery of the Abbey's lease practices following the episode of 1516 was relatively slow, whereas the recovery from 1527 was more general although lease prices remained low for a couple of years. The final episode of the conflict in 1542-43 had little effect on the use of share contracts. In effect, the Abbey used generic rent reductions for these two years and for all its tenants. As previously noted, however, this final episode of the Guelders Wars was quite distinct from the previous episodes given the limited damages to the Abbey. It is this chronology of war-periods that is the most likely explanation for the use of share contracts. In addition, there are several arguments for the use of such contracts only for the farms.

The contributions levied from the Abbey during the pillaging of 1527 were roughly 3,400 guilders. This sum excludes damages to buildings, infrastructure, fields, foregone costs of farming and losses in farming capital such as animals and tools. Lacking clear estimates, one can only guess at the total damages to the Abbey. Nonetheless, it seems reasonable that the loans of 4,760 guilders contracted by the Abbey were at least largely spend on the contributions and direct reconstructions, with little left for restarting agricultural production—and, possibly, these loans may not have fully covered those costs. The low lease prices in the years 1528-30, as found in Figure 3.1, are a reflection of low productive potential of the plots leased out by Mariënweerd.[32]

The rural economy of the River Area had come to a full stop during these years, and the Abbey was unable to kick-start production through investments on its own. Without such investments and subsequent collections of normal rents payments from its tenants, the Abbey and the economy were set for a slow recovery. As emphasized before, the Abbey facilitated recovery on its fixed-rent plots by allowing tenants to accumulate substantial arrears and by paying them for small improvements contracted and executed by the tenants. In comparison to the lease prices and total lease income of the Abbey, such improvements were usually of small magnitude. The arrears in rent

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[32] Recall that the lease prices for these years are relatively precisely estimated. See the right panel of Figure 3.1.

payments, however, could be substantial and potentially limited the Abbey's financial recovery. Given an average lease price of approximately 2 guilders per *morgen* in 1530—see Figure 3.1—and approximately 2,100 *morgen* of land, the annual income from leases if all land would be at fixed rent was 4,200 guilders. This amount is comparable in magnitude to the total amount of loans contracted by the Abbey or the contributions levied from the Abbey in 1527. Accordingly, simply foregoing a large share of that sum for a couple of years by allowing tenants to accumulate arrears was a very expensive strategy to stimulate recovery.

The numerous tenants on normal plots—with usually up to 20 *morgen* in total— together leased roughly two-thirds to seventy per cent of the Abbey's landed property in commercial lease. The ten new farms of the Abbey roughly consisted of 600 *morgen* of comparable land around 1530 and together made up the other part of the commercially leased lands.[33] It is on those ten farms that the Abbey's main attempts in stimulating rapid recovery are to be found. In the three cases of known fixed-rent contracts, it did so either by charging exceptionally low annual rents or through direct investment on the farm. More importantly, the Abbey invested directly in many of the farms by financing half of the animals through share contracts. It did so on five of the ten farms from the outset, and two more in the course of the 1530s.

Of course, the financial situation of Mariënweerd Abbey was weak and it could stimulate recovery on most of its land only through passive support—i.e. allowing tenants to accumulate arrears and through financing their improvements ex post. The choice for expensive investments in livestock through share contracts seem hard to explain, at least from the perspective of the Abbey. This especially given the substantial costs of direct investment in livestock—such as the 10 to 30 guilders costs of a horse. Even though direct references to the expenses cannot be found in the accounts, some evidence can be distilled from the accounts concerning the splitting of livestock on the two tenants that move from fixed rent to share contracts. In 1535, for instance, the Abbey buys half the livestock on 'De Hoge Spijck' for a sum of roughly 280 guilders.[34]

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[33] Note that the total of the ten farms in the mid 1540s is 657 *morgen*, while excluding the new farm, 'De Vogelzang', leaves 615 for the original ten. No information on size for all of ten farms exists for the 1530s. However, there is information for the five farms in the *Mariënweerdse Veld*. These farms together totaled 387 *morgen* in ca. 1535 and 340 *morgen* in the second half of the 1540s. Hence, these five farms even decreased in size, making the 600 *morgen* in the main text a conservative estimate. Furthermore, recall from Section 3.4 that there are no obvious differences in rental prices—for the 1540s and 1550s wherein all contracts are fixed-rent again—to suggest differences in productivity across the farms or between farms and normal plots or between the fixed-rent and sharecropped farms.

[34] Note that this sum is equivalent to the exceptionally high annual rent of the farm. Similarly, half of the livestock on 'De Hoge Sluis' is worth 165 guilders in 1535 or more than 1.5 times the annual rent of this farm—see Appendix C. Note, furthermore, that it is not even clear that these amounts, represent half the value of the full livestock on such farms, because the farms might not be fully stocked yet.

As noted before, the costs of slow recovery to the Abbey were substantial as well. By financing a few large tenants directly, I argue that the Abbey ensured itself of three important outcomes. First, it ensured that a large part of its landed property would be leased out at all. Second, that this part of its property would recover relatively fast. After all, it stocked half of the animals of the farm itself and, furthermore, contractually forced the tenant to fully utilize the farm—as the tenant had to supply the other half of the animals and paid fines for unused land. Third, using share contracts ensured the Abbey of a steady stream of payments, because it received half of all income streams of its sharecroppers—hence, during each harvest and with every sale of an animal or animal product. These income streams might be somewhat limited during the first years when farm productivity was still recovering. Nonetheless, these income streams provided the Abbey with important liquidity while most of its tenants were accumulating arrears. Evidently, tenants on share contracts did not build up arrears at all.

Share contracts seem to have been preferred by the tenants of the large farms too. Besides the direct investments in animals of the Abbey, the share contracts provided tenants a relatively easy entrance into the farm as rental payments under share contracts are proportional to actual production. Of course, the contracts stipulated fines for unused lands and stated that the farm had to be stocked with an appropriately large stock of animals. However, the contracts did not stipulated how intensely the lands had to be sown or worked, nor when the stock of animals had to be of appropriate size. That share contracts were preferable for these large tenants can be inferred from the two tenants switching from fixed rent to share contracts in the mid 1530s. In one of these cases, investments in the farm buildings start only after this switch—on 'De Hoge Spijck' (AM 71, 76). Once farm productivity had recovered, however, share contracts were simply less useful and tenants returned to fixed rent, as most tenants did in the late 1530s or 1540s.

The alternative to the large sharecropped farmsteads for tenants and Abbey was to lease those lands in separate plots. Clearly, both the Abbey and the large tenants had alternative options as several large tenants used fixed-rent contracts. The farm sizes at Mariënweerd were not cast in stone as plots regularly changed in size. In addition, most of the large farms were new and, therefore, adjustable. In many cases, the farms indeed changed in size between ca. 1530 and 1550. Even in the case that most of the large tenants would have been unable to stock the same amount of land with animals alone, they would have been able to stock a farm of half that size. If fixed-rent contracts were preferable to share contracts for tenants or landlord, then there should be many tenants in that category. Of course, it is possible that the lease accounts conceal several

tenants that made such a decision although there were hardly any tenants individually leasing roughly half the size of the large farms. However, there is only one tenant in the years around 1530 leasing an amount of land with such a size: one Claes Gerritssoen with 47 *morgen* (AM 71, 36). Other large 'normal' tenants—i.e. tenants without a farm—in this period had at maximum 23 *morgen* of land (AM 71, 94), much less than half the average farm.[35] Hence, it seems unlikely that tenants simply preferred a smaller fixed-rent farm over a farm. For the Abbey, allowing its wealthier tenants to reduce the size of their leases would only cause problems. First, it would have had to find more tenants to get its lands leased. Second, new tenants would be unlikely to be better able to kick-start their farms than the wealthiest tenants that the Abbey already had.

3.6   Conclusion

Using the rich archives of Mariënweerd Abbey in the Northern Low Countries, this paper shows how share contracts could be useful as temporal investment and liquidity contract. In the wake of a damaging war episode in the mid 1520s, the Abbey organized about a third of its lands held in competitive lease in large farmsteads of 45 to 85 hectares of land—or 50 to 100 *morgen*. The tenants of these farmsteads were directly supported by the Abbey in rebuilding and restocking their farms. The clearest direct support occurred through the use of share contracts on five—and eventually seven—of its ten farmsteads, the Abbey and the tenant shared the costs of stocking the farms with new animals. The share contracts limited the burden of rent during the first years of the lease while farm productivity was still recovering from the damages of war. In addition, the share contracts generated a frequent stream of payments from the sharecroppers to the Abbey, as every source of income on the farm would be shared.

Importantly, the Abbey used share contracts rarely throughout the several centuries of its lease practices. In the wake of the 1520s crisis, it used share contracts only for its largest tenants and only for ten to fifteen years, after which the tenants returned to a fixed-rent contract. In addition, they used the same mix of crops and animals as other tenants—from poor to rich fixed-rent tenants—while the Guelders River Area had relatively well developed markets. This is at odds with the theoretical literature that associates share contracts with poor tenants, vulnerable and risky agricultural production

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[35] Interestingly, Reijer Hermanssoen, the sharecropper whose lease of 'De Gansheuvel' was terminated because of a violation of the share conditions, leased a total of 35 *morgen* from the Abbey in 1541. This made him the second largest normal tenant of the Abbey. If this arguably untrustworthy tenant already had over 10 *morgen* more land than the other normal tenants, it is unlikely that the smaller tenants had similar means or capacities to rent a farm if they had preferred one.

processes, and with economically underdeveloped regions. This literature stresses the productive inefficiency of share contracts, and argues that the contract is useful only to solve particular contracting problems and limit moral hazard. Nonetheless, the lease accounts of the Abbey resonate with the contracting problems often discussed in theoretical literature, such as risk and credit-constrained tenants. The Abbey, however, used several strategies to cope with these problems but it did so in its fixed-rent contracts. The products produced in the River Area and the accessibility of markets seem to have limited the need for share contracts to solve these problems. Furthermore, the detailed accounting of the Abbey limited the opportunities for opportunistic behavior of its tenants. Hence, the standard explanations for sharecropping do not hold in the case of Mariënweerd Abbey. What is clear, however, is that once share contracts were adopted by the Abbey, the scope for deceit by the sharecroppers increased dramatically, forcing the Abbey to impose several additional stipulations to insure itself against deceit.

The theoretical literature on contract choice, and sharecropping in particular, provides clear predictions about the conditions wherein share contracts are useful. Direct empirical support, however, has remained weak and usually focuses on the efficiency of sharecropping in a given context rather than on explaining why share contracts exist in such a situation (Chiappori and Salanie 2003; Ghatak and Karaivanov 2014). The historical literature, in contrast, has generated detailed studies explaining the use of sharecropping in particular context, although within a limited range of cases and drawing on the theoretical literature for guidance. Ultimately, that theoretical literature can be used to explain why sharecropping is efficient in some cases but it does not provide an exhaustive overview of all these cases. More importantly, by following the theoretical literature too narrowly, the historical richness and relevance of sharecropping might be overlooked. After all, the use of sharecropping in the early modern Low Countries ranges from situations wherein share contracts are efficient in cases of poor, credit constrained tenants and particular crops, but also as efficient temporary crisis contract in flourishing economies.

This historical occurrence of share contracts as temporal investment and liquidity stimulant, fits within a rich literature on sharecropping in economic history. However, this literature has usually stressed the same contracting problems as the theoretical literature. It has emphasized that share contracts are, thus, used only in a limited number of specific situations such as in Mediterranean viticulture where landlords had to provide poor, credit-constrained tenants with reasonably diverse farms to allow the tenant a family income (Carmona and Simpson 1999; 2012). In contrast, this paper has shown that share contracts were often used in the Low Countries too, and in a variety of circumstances. The share contracts at Mariënweerd, arguably, are an extreme case

but share contracts were used in the wake of crises in other cases too, as discussed in Section 3.3. Accordingly, the findings in this paper call for more attention of the investment side of share contracts, and that this side of the contract does not align with current theoretical predictions. Importantly, these findings also suggests that without specific attention for the particular function of share contracts, simply observing share contracts in an economy provides but little information about the functioning of that economy.

Appendices

A   Additional tables and figures

Table 3A.1: Evolution of newly contracted lease prices per plot over time

	(1)	(2)
	Price	Price
Size	-0.000	
	(0.015)	
Duration	-0.032	
	(0.023)	
Constant	1.958***	1.688***
	(0.598)	(0.562)
1520	0.154	0.104
	(0.615)	(0.615)
1524	0.523	0.486
	(0.801)	(0.801)
1525	0.342	0.278
	(0.598)	(0.597)
1526	0.637	0.481
	(0.669)	(0.659)
1527	0.039	-0.041
	(0.659)	(0.657)
1528	-0.033	-0.079
	(0.601)	(0.599)
1529	0.643	0.601
	(0.575)	(0.574)
1530	0.640	0.593
	(0.583)	(0.582)
1531	0.881	0.816
	(0.698)	(0.696)
1532	0.742	0.748
	(0.623)	(0.623)
1533	1.408**	1.363**
	(0.665)	(0.665)
1534	0.186	0.156
	(0.837)	(0.836)
1535	0.531	0.609
	(0.634)	(0.632)
1536	1.278**	1.251**
	(0.623)	(0.623)
1537	0.791	0.826
	(0.620)	(0.620)
1538	0.994*	1.056*
	(0.593)	(0.590)
1539	1.808***	1.878***
	(0.569)	(0.566)
1540	1.897***	1.955***
	(0.587)	(0.586)

Year		
1541	2.594***	2.656***
	(0.608)	(0.607)
1542	1.157*	1.182*
	(0.627)	(0.627)
1543	1.911***	1.956***
	(0.615)	(0.615)
1544	2.058***	2.085***
	(0.588)	(0.587)
1545	1.610***	1.665***
	(0.585)	(0.583)
1546	1.845***	1.903***
	(0.573)	(0.571)
1547	1.796***	1.833***
	(0.575)	(0.575)
1548	2.287***	2.317***
	(0.700)	(0.700)
1549	2.080***	2.097***
	(0.639)	(0.639)
1550	2.270***	2.259***
	(0.620)	(0.619)
1551	1.918***	1.939***
	(0.618)	(0.618)
1552	2.421***	2.438***
	(0.579)	(0.579)
1553	2.079***	2.114***
	(0.619)	(0.618)
1554	2.799***	2.835***
	(0.608)	(0.608)
1555	2.692***	2.698***
	(0.648)	(0.647)
1556	3.564***	3.616***
	(0.814)	(0.813)
1557	3.003***	3.060***
	(0.802)	(0.800)
1558	1.948**	1.967**
	(0.808)	(0.809)
1559	3.169***	3.178***
	(0.804)	(0.804)
1560	2.947***	2.962***
	(0.693)	(0.690)
Observations	264	264
Number of Plots	79	79

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The results presented in the Table derive from a simple random-effects regression of the price for new leases per *morgen* on year-dummies and, in the first column, corrected for the duration of the contract and the size of the plot. Prices are measured in guilders—these are Rhine guilders also referred to as Carolus guilders in later periods. Lease prices expressed in other currencies, such as golden guilders (1.4 Rhine guilders) and Philipus guilder (1.25 Rhine guilders) are exchanged into Rhine guilders based on the exchange rate used by the Abbey, which remained constant throughout the period under study. Source: The results are based on the same of plots of constant size that can be consistently trace through the lease books. See the main text for an explanation.
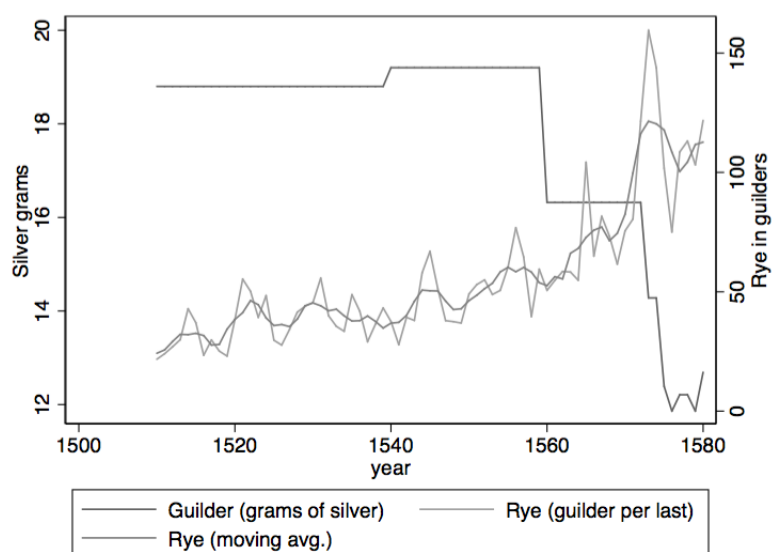
Figure 3A.1: Price inflation in the Western Netherlands

Note: The Figure presents two measures of price inflation: the silver content of the guilder (on the left axis), and the price of rye (on the right axis).

Source: J. L. van Zanden, *The prices of the most important consumer goods, and indices of wages and the cost of living in the western part of the Netherlands, 1450-1800.* http://www.iisg.nl/hpw/data.php

B Normative stipulations in the lease contracts

This Appendix provides direct transcriptions of the main lease contracts of Mariënweerd Abbey for the period under study. It provides general lease conditions for the period around 1530 for the standard fixed-rent tenants. This is followed by the general lease conditions for sharecroppers and a transcription of all the share contracts found in the lease accounts. The third part of this Appendix provides transcriptions of the full fixed-rent lease contracts of the large tenants, which can be compared to the share contracts used by other large tenants. These fixed-rent contracts include contracts dated around 1530—to compare across farms with share and fixed-rent contracts—and contracts of later dates for farms that were rented on share contracts during the 1530s.

General lease stipulations for fixed rent contracts, ca. 1530

AM 70, 1: General lease conditions, anno 1530

"Anno 1530

Gemeyn ordinantie om ons lant te verpachte

In manieren ende formen hijr nae bescreven soe wil

mijn eerweerdighe heer heer Jan Starck vanden Berghe

abt indertijt van Marienweert verhueren zijn lant op

Beest, Deyl, Eynspijck, Gelremalsen, Meteren, Hellu

Hijr, Weerdenborch, Rompt in onse polre of op

wat plaets dattet lant gelegen is. Inden yersten

soe salmen den pacht betalen Martini nae die schaer

die een helft ende te Kersmisse dair naevolgende vol

betalen op verborennisee vander hueringhe ende beter

nisse vanden landen. Nyemand en sal zijn land an-

derluyde verhueren of overgheven sonder consent myns

heren op verborennisse vander hueringhe ende betscap des lants.

Men sal gheven vrij gelt.

Men sal nyemant ander maet leveren

dan die alde boecken vermelden. Wye zijn lant

dryes ainvanckt die sal die twe leste iaren weder

om dryes overleveren. Men sal op elcke mergen een

volwassen hoen gheven.

Item die dat lant op die voirscreven ordinantien huert of ain-

vanckt ende nyet en voldiet nae waersinghen der

voirwerden den mach mijn heer zijn lant anderluyden

verhueren ende weder tot hem nemen."

AM 71, 2l: General lease conditions, anno 1530

"In manieren hyr nae bescreven wil mijn eerwerdighe heer heer
Jan Sterck vande Berghe abt van Sinte Marienweerde inder tijt verhueren
sijn lant op Culemborch inden landen van Bueren op Beest in onsen
polre, te Deyl, Eynspijck, te Rompt, Gelremalsen, Meteren, Weer-
denborch, te Hijr, te Hellu, te Nerinen of in wat dorpe dat gele-
ghen is. Inden yersten soe salmen den pacht betalen alle iaer eens dat
is te wetenen Martinii nae die schare die een helft ende te Kersmis
inden selven iaer daer naevolgende die anderhelft op verborennisse der hueren ende
beternisse vanden landen. Oick en sal hij dat landt nyemant overgheven noch
verhueren buyten mijns heren consent op verborennisse vanden huren ende beterscap
vanden
lande. Ende hij sal mijn eerweerdighe heer gheven vrij gelt, schotvrij, lotvrij,
dijckvrij, schatvrij, mergengeltvrij, watariaren vrij, oirlochiaren vrij, molengelt
vrij, nyet uutgescheiden dan hemelval. *Nota inferius*
        Item mijn heer die
prelaet en wil nyemants ander maet leveren vande landen dan nae onsen
boecken alsoe dair inne gescreven staet. Item alle dye ghenen dye haer lant
dryes aenvanct die sullen dat lant die twe leste iaren wederom dries overle-
veren. Item op elcken merghen sal men gheven een volwassen hoen.
Item van die molen in onse polren sal men van elcken mergen te verschot
gheven 2 stuiver ende bij alsoe dat mijn eerweerdighe heer noch een molen wolde
setten in den
polre soe sal men van ecken mergen totter onraet ende totter verschot gheven
4 stuiver. Item die 10 merghen lants hebben sullen gheven 2 pont
kruyts half peper half geynber. Item oft men op mijns heren lant opte
merghen enighe penninghen setten dat sullen die pachteners heel betalen
sonder enighe afcortinghe van mijns eerweerdighe heren nyet uutescheiden dat
alleen hemelval wat gelt dattet oick solden moghen wesen.
Soe wye hyr in enighe vander voirscreven articulen in ontginck ende nyet
achtervolgende alle iaer alsoe wael in minste als int meeste den mach
mijn eerweerdighe heer dat lant opsegghen ende wederom tot hem wert nemen sonder
yemants wedersegghinge. *Actum anno 1530* opte 29 dach Januarii.
Ende alle olde vonden [vorwerden] ende nye vonden [vorwerden] diemen opt lant

vinde of opt lant setten of bringen

mach in enigerleye wyse nyet uutgesondert gheen saken en sullem hem behulpe-

liken wese sy sullen al vrij gelt gheven ende nyet korten al welk wij goede lieve heer

hebben

dat niemant te korten solden soe en sullen zij nochtans nyet korten. Item noch is oeck

voorwaerdt dat een ygelick pachter bewaren sal den dijck

tot sijnen landt hij van ons in hueren heifft op sijnen cost bewaren

sal ende die scouwe daer oever leveren sal buyten ons cloysters

last ende auxt alle dynck sonder arch off list."

AM 74, 99r: General lease conditions in Land van Buren

Allen onssen landen ind goederen gelegen inden landen van Buren woe dye gelgen

moegen sijn als hier na bescreven staet. Gemenen woerweerden ind maniren onsse

landen to verhuren inden landen van Buren.[36]

Nota: Summa summarum onsse gansse lant gelegen int lant van Buren met dye drye

bouhaven ingerekent fatuunt to samen an mergentael drye hondert ind 13 mergen [313]

butenstaende het lant genaempt den hont gelegen bij Culenborrich an dye hat ind noch

tyn mergen meit vijff hont welick borgeren van Culenborrich van ons in huren hebben.

Dye hoffsteden int lant van Buren mede ingerekent.

In manieren ende formen van vorwearden hier naebescrevenen soe wyll

mijn eerwerdigen heer ende prelaet inder tijt abdt tho Marienwerdt ver-

hueren ende in jaerlicxen huyren uuytgheven alsulcken landt als gelegen is inden

Landt van Bueren op Eerichem op Linghenvelt inden Haghen op Tricht

Op Bosinchem op Selmonde op Malsen op Ascha off waer dattet

landt gelegen is dat uuytter hueren is ende nu ter tijt nyeuwe huyren

wyll maken. In allen verwaerden hier nabescreven. Ende wy daer gadingen

in dat landt heeft tho hueren die zall den rentmeester inder tijt

wesende voldoen nae uuytwijsinge deser voerwaerde ende huyr cedelen

dan van gemaeckt zijn. Ingaende altoes Petri ad Cathedram ende

Martini naedeschaer die helft vande pachte the betalen ende the

kersmisse daer nae volgende vol op betaelt inden selven jaer

sonder ennich verteeck offe argelist op verboertenisse vander huyringen

welcke huyr zall duyren acht jaer lanck. Tis te weten dat mijn

---

[36] Similar general lease conditions for plots in the Land of Buren dating from 1534 exist. This heavily
damaged text can be found in AM 77, 2r.

erwerdighen heer voerscreven dat landt wyl verhuyren bij Carolus gulden ad twyntich
stuver Brabants den gulden off anders guet Brabants payment in gelijcker
weerden inder tijt der betaelinge. Item van elcken merghen zall die
pachtenaer gheyven jaerlich een volwaschen hoen off een capoen nae
believen mijns heren. Ende die sess merghen hebben een pondt cruyts jaerlich.
Item men zall gheven vrij gelt, schot vrij, lot vrij, dijckvrij, schatvrij,
merghengelt vrij, waterjaeren vrij, ende oerloghjaren vrij, moelengelt vrij, nyet
uutgescheiden dan hemelfall ende allen alden vonden ende nijeuwe vonden diemen opt
landt
vynden off setten mach in enyger manieren off wyst niet uutgescheiden zij
woenen bynnen off buyten landt gheen dingen en zullen hemluyden behulpelinck
zijn zij syllen altesaemen vrijgelt gheven in gheenerley manieren t'corten al welde
oeck onse ygen heer van Bueren dat zij corte solden zoe en sullen zij nochtans
nyet corten off mijn eerwerdighe heer zal sijn landt weder tot hem moegen
nemen. Item zij sullen dat landt graven ende heynden op haren kosten
zonder eynighen cortinge off beleringe daer van t'doen int eynde van
haren jaren. Ende bij alsoe off enyge pachtenaers versuymelick waeren
die pacht alle jaren nyet en betaelde als voirscreven is zoe mach die rentmeester
van wegen mijns heren dat landt weder aenfangen ende verhuyren dat een ander
die welcke beter betalinge zall doen. Item nyemant en zall sijn landt dat hij
in huyren heeft oversetten off verhuyren wy alleen bij consent mijn eerweerdighen
heren op verboernisse der huyren vande landen ende alsdan salmen dat landt
aennemen alsoe men dat vyndt ombecroent sonder enych wederseggen ende
op wat dach die pachtenaers nyet en betalen zoe sall die rentmeester inder
tijt die verscheinen ende vorleden pachten uuytnamen met de gezworen pander
als des heren schulden vande landt van Buren ende wanneer die huyren vande jaren
uuyt zijn ende gheen nyenwe huyringe gemaeckt hebben zoe sall onsen rentmeester
dat selve landt ende erve aenfangen inde herft naede leste schaer buyten wyll ende
consent vanden selven die dat landt in huyren gehadt hebben. Ende het ge-
woentelick ransoen the weten ygelicken mergen negen stuuver Brabants zalmen betae-
len
bynnen den tijt van vierthien dagen der nyeuwe huyringen.

Share contracts; general stipulations and individual contracts ca. 1530

AM 71, 2r: General lease conditions for share contracts, undated (ca. 1530) "Een
gemeyn vorwerden om onse bouwinghe ten halven te verhueren

In manieren en formen hyr nae bescreven wil mijn eerwerdighe heer heer Jan Sterck vanden Berghe

abt van Marienweerde inder tijt zijnen bouhoven uutgheven oft verhueren ten halven 6 iaer lanck durende ende nyet langher. Ingaende anno 1530 ad cathedra petri.

Inden yersten soe salmen alle beesten het zijn peerden, koyen, vercken, scapen of byen half ende half setten ende die schaden ende baten salmen gelijck draghen. Ende die bouman en sal anders gheen vreemde beesten hebben sonder mijns heren prelaten consent.

Item een bouman sal holden alzoe veel beesten als sijn bouwinghe ver-mach ende van elcke koye sal hij gheven 28 pont botteren ende van een veerse 20 pont tsint Jansnisse volbetaelt. Ende alzoe veel scapenkeeskens als sij overcomen kunnen nae grootheyt der bouwinghe ende mennichte van scapen.

Item die bouman sal van elcker merghen dye hy weyt buyten betaelen 2 golt gulden de ponder of die weerde dair voir. Ende op elcke thien merghen sal hij gheven het zijn hoech of leech een pont kruyts half peper half ginber mit een pont suyckers.

Item op elcken 8 mergen sal die bouman gheven tot sijnen wijn een golt gulden de ponder. Ende hij en sal gheen beesten vercopen sonder mijns heren oirlof.

Item die bouman sal alle iair te Paesschen gheven 1 vetten koerhamel ende 200 eyer ende 1 botterwegghe wegende 10 pont. Ende van elcken mergen sal hij tsiaers gheven 1 volwassen hoen half capuynen half pachthoenre.

Noch zijnt voirwaarde dat mijn eerweerdighe heer voir uut sal hebben 1 koermergen uut den wynterkoern of somercoern langes of dwerswaert sijn eerweerdighe believen sal

behelteliken mijns heren thienden groot is of cleyne. Ende die bouman sal dat lant besayen ende mayen sonder mijns heren toedoen mer inde bou soe sal mijn heer setten eenen schoter dye dat koern tellen sal den sal mijn heren loonen ende die bouman sal hem die cost gheven. Ende die bouman sal gheen ongetelt coorn in vueren sonder mijns heren consent. Ende men sal mijns heren koorn yerst inde berch vueren eer dat hij zijn koorn aentasten sal. Ende als mijns heren coorne inden berch is soe sal die schoter weder int clooster komen ende mijn heer sal hem dan loonen of wil hem die bouman holden langher om sijn koorne in te bringen soe sal hem die bouman loonen mijn heer nyet.

Item die bouman en sal mijns heren koerne nyet dorsschen tensij saken dat mijns heer yerst eenen dorsscher teghen schickt om mijns heren coorne yerst af te dorsche. Ende die dorscher sal mijn heer loonen ende die bouman sal hem die kost gheven ende wanneer mijns heren koerne afgedorst is soe sal hij in clooster

komen ende die bouman sal mijns heren koorn op zijnen kost int clooster leveren ende bringen.

Noch zijnt vorwerde dat een yegelyck bouman van elcke mergen die zij bruychen hooch

ende leech een halfroye sullen laken ende graven op beyde die weteringhe te wetenen op elcken weteringe een halve roye ellick nae zijn ordinantie ende een yegelyck bouman sal zijn deel holden vande zeedijck nae mijns heren ordinantie ende haer holen ende stegen

op maken tot guetduncken mijns heren ende prijsen vande hemmerade. Ende op elcke mergen sullen sij gheven te verschot iairlicx buyten mijns heren costen want mijn heer noch

een nyeuwe molen setten wil tot profijt van onse landen inde polre ende diet gebruken. Oick waert saken dat enighe storm of wynt die molen omworpen dair god

voirzij ende marckeliken scaden dair aen dede soe sal een yegelike nae grootheit zijnre mergentael den scaden helpen betalen die der molen ghenyeten.

Voirt zoe sijnt vorwerden of quaem dat mijn goede lieve heer enighe penningen boven onse

privilegien hebben wolde daer wij te nyet teghen doen en mochten zoe sal dye bouman in gelde ende nyet korten nae older gewoenten ende mijn heer vrij gelt leveren als voirscreven staet nyet uutgeschieden dan hemelval waert oick afkomen mochten ofte dat sake waer dat dair enighe vercortinghe in onsen privilegien geschieden ende wij te hofe vervolgen sulx af te draghen ende kosten deden sullen alle die ghene die die privilegien begheren te genyeten helpen die kosten dragen sonder enich wederseggen.

Mijn heer prelaet sal eenen yegelijcken alsoe veel lants hoech ende leech gheven nae zijn guetduncken ende wes sijn eerweerdighe den bouman laet bruycken dair van wil sijn

eerweerdighe die olde granen op geworpen hebben ende gegraven ende geslijpt granen nae

boumans prijs ende dat lant bereyt wil hebben zoe veer hij mit hem overcomen kan. Ende yetelijcke taxie vande potinghe van iaer tot iaer drye iair lanck becleven leveren sonder enighe argelist.

Noch is voorwaarde dat die bouman mijn heer iairlicx dienen sal twelfmael twe mijlen omgaens mitte peerden ende wagens of van elcken dienst sullen sij mijn heren betalen eenen halven golden gulden of mijn heer gheven dienst behoefden.

Item dye bouman en sal gheen drieslant breecken buyten mijns heren prelaets."

AM 71, 6r: Lease contract of Goessen Ghijsberts, anno 1530

Goessen Ghijsberts ende Neel sijn huysvrouwe heeft aingenomen ten

halven te bouwen vanden eerweerdighe heer heer Herman van Rossem die bouwinghen

die zij een deel iaren ten halven gebouwet heeft mit alsulcke voirwerden.

Inden yersten sal hij iairlicx gheven een half aem wijns rijns. Ende 2 pont

cruyts half geynber half peper ende 13 capuynen mit 13 smaelhoenre.

Item te paesschen een botterwegghe mit 200 eyer. Item hij sal iairlicx paten hon-

dert poten ende die drie iaren halden becleven. Ende mijn heer sal dat huys

laten timmeren ende den thimerman loonen mer Goessen sal hem die kost ghe-

ven. Ende dan sal Goessen dat huys mitter schuer dicht halden gelijck hij dat

aynvanckt. Goessen sal oick sijn achterstellinghe penninghen t'sint Jans-

nisse betalen anno 29 die een helft ende die anderhelft cathedra pe-

tri anno 30. Ende hij sal een yegeliken gheven sijn gerechticheit.

Oick en sal hij gheen vreemden beesten holden dan mijns heren consent hem

gegeven waer. Voirts soe sal hij blijven bij zijn olde voirwerden. Ende dese hueringe

sal dueren 6 iaren lanck ingaende cathedra petri anno 1530 ende al

soe voert aen van iaer tot iair. Geschiet te Marienweerde in Die Bonifatii

anno 1529 in presentien van mijnen eerweerdighe heer Herman van Rossum, Claes
Spierinck

capellaen, Claes van Beest rintmeester ende tot meerder getuychenisse sijn

hyr van gemaect 2 celen [cedulen] gelijck holdenen uut malcanderen gesneden durch

litteren A B C.

Item om te wetenen wat schult van restanen dat zij schuldich waren bleven ind

iaren van 1529 ende van allen voirleden iairen soe hebben wij mit hem

gerekent vande voirscreven restanden mit die scrickenberghen [?] ende mitten

verschot vander molen ende dat Lingelt mitten weygelt vande voirleden iaren

zoe bleven sij schuldich 67 rijns gulden 7 stuiver den pacht vanden iaer 30 blijft

ongerekent buytenstaende zijn wijn ende cruyt vanden selven iare. Actum anno treces-
imo

opten derden dach mons julii presente redittus Nicolao van Beest.

Solvit...

AM 71, 7r: Lease contract for 'De Gansheuvel', anno 1526

Reijer Hermans ende Janneken hebben aingenomen ten halven te bouwen

die bouwinghe opten Gansovel [Gansheuvel] mitten lant hyr nae bescreven 6 iaer lanck
durende ingaende anno 1529. Inden yersten 12 mergen inde Omloep.
Item 5 mergen inde Wolterscampe. Item 15 mergen int Langewant, noch 5
mergen die Andries die Munter gebruyckt heeft ende 4 mergen die Aert die
Munter gebruyct heeft. Item 6 mergen inde Elrot. Noch 17 hont in die
Gansovel. Summa, beloopt 50 mergen min 1 hont hoochlants.
Item 5 morgen inden Gansovel ende 9 mergen inden Offercampe mit 6 mergen in
die Ramscampe. Noch 12 mergen inden Bothollen. Belopen tsamen 32
mergen leechlant. Maecken tsamen 82 mergen min een hont.
Dit voirscreven lant sal Reijer ten halven bouwen ende des lants oirber te doen,
te eren, te messen, te somervegen, te seyen, als een guet bouman schuldich
is te doen mit desen voirwerden. Te weten dat hij van elcken mergen dye hij niet
en eert of en hoyet sal iairlicx gheven 1 rijns gulden brabants. Item van elcke koye sal
hij iairlicx gheven 24 pont botteren roymeyssche botteren voir sint Jans-
nisse te leveren. Noch anderhalfhondert [150] aprilsche scapenkeesken. Item mijn heer
sal voir uut hebben een koermergen het zij winter of somerkoorn zoe dat mijn
heer dat believen sal. Item Reijer sal iairlicx gheven twe pont cruyts half geyn-
ber half peper. Item 12 capuynen en 12 volwassen hoenren. Ende 200 eyer ende
een botterwegghe te paesschen van 8 ponden. Item dat molengelt ende dat
Lingengelt sal hij half betalen. Item hij sal dijc veghen ende dat velt hel-
pen reecken ende hij sal oick geholden zijn tot des cloosters dienst alst van
nooden sal wesen. Item hij en sal oick gheen vreemden beesten holden noch gheen
ander lant bouwen dan des cloosters lant dant 8 hont op Tricht bij den Oest
dye te eeren ende dat koern dair af te halen. Item ist sake dat Reijer dit voirscreven
lant nyet en oirbert ende bouwet zoe mach mijn heer zijn lant wederom tot hem
nemen. Ende hij en sal dit lant nyet overgheven of yemands verhueren sonder mijns
heren oirlof. Inden bouw soe sal mijn heer eenen schoter setten ende Reijer sal hem
die kost gheven. Ende dat koern salmen opte velde deyle half ende half ende dat
elck in sijnen berch vueren behelteliken mijns heren thienden groot ende cleyne.
Ende die beesten salmen afsetten alst orber ende mit wesen sal. Ende die wol salmen
deyle half ende half vanden scapen. Item alsmen die messen uut sal vueren vanden
Gans-
ovel soe salmen Reijer uute cloester gheven een smael ton biers. Item of Reijer
binnen dese 6 iaer storf zoe mach mijn heer mitten erfgenamen rekenen ende geloifte
voir die schult nemen ende ainvangen die bouwinghe weder ain sonder
yemans becronen. Dit is geschiet in presentie vanden eerweerdighe heer heer Herman
van Rossem,

ende capellaen ende rintmeester anno 1526.
Gerekent...

AM 71, 9r: Lease contract for 'De Neust', anno 1536

Willem Jans ende Janneken zijn huysvrouw hebben ingehuert ende aingeno-
men een bouwinghe opte Oest vanden Eerweerdighen heer heer Herman van Rossem prelaet van
Marienweerde inder tijt om ten halven te bouwen in manieren ende formen hyr nae be-
screven mitten landen naevolgende 6 iaren lanck. Ingaende anno 1536. Inden yerste die
hofstat mit 4 mergen lants tsamen gerekent mitter hofstat. Noch 2 hont lants opten
Zennephof. Item 3 mergen lants buyten dijcx. Item 3,5 mergen lants bij dat Palmgat.
Item noch 6 mergen lants 2 hont streckende aenden zeedijck teynden de Zennephof.
Item 13 hont lants in die Kromme Start. Noch 11 hont lants bij die Zantfoirt.
Item 4 mergen lants opt Langewant. Noch 5 mergen lants inden Cortecampe.
Noch 7 mergen dair theynden. Item 9 mergen lants in die twe Garstcampen. Noch
7 mergen lants inden Grote Asselcampen. Item 7 mergen lants inden Cleynen Assel-
campen.
Noch 8 mergen inden Selligen campen. Item 5 mergen lants aenden Doornebosch. Noch 3
mergen lants bij die 7 mergen. Item 11 mergen in die Cortecampen. Noch 11 mergen lants in
die Zellige campe die Herberen die Wever gebruyct heeft.
Summa vande landen beloopt 104 mergen een hont lants
luttel min of meer te gueder maten nae uutwisinghe ons boecx.
Dit voirscreven lant sal Willem Jans voirscreven ten halven bouwen
als een guet bouman schuldich is te doen des lants oirber ende profijt, te eeren,
te messen, te somerveghen, te seyen, te meyen, te binden aen die vuumen te brenghe
ende mit vorwerden dat hij van elcken mergen lants die hij nyet en eert of besuyt mitter
ploech noch en hoyget sal gheven iairlicx 1 Carolus gulden. Item van elcke koye
sal hij gheven 20 pont botteren meysche. Ende vanden veersen 10 pont botteren le-
verenen tussen meye ende sint Jansdach inde somer. Noch sal hij iairlicx gheven
100 aprilsche scapenkeeskens ende een koermergen voir uut het zij winter of
somercoorn alst dat mijn heer believen sal. Hij sal iairlicx geven een half aem wijns
2 pont kruyts half peper half gingber. Item 12 capuynen ende 12 volwassen
hoenre. Te paesschen 200 eyer ende een botterwegge van 8 ponden. Item dat

molengelt ende dat Lingegelt sal hij half ende half betalen.

Item Willem sal dat velt helpen recken ende veghen ende tot des cloosters dienst
geholden zijn als die ander bouluyden nae older gueder gewoenten op verborennisse
der
hueren of halft toolter [?]. Hij en sal gheen vreemden beesten holden onder onse ende
zijn
beesten. Noch gheen ander lant eeren of bouwen mit sijnen peerden. Item ist sake dat
Willem dit voirscreven lant nyet en bouwet tot orber ende profijt des cloosters soe mach
mijn heer die prelaet sijn lant weder ainvangen sonder yemants becronen. Ende hij en
sal dat voirscreven lant oick nyemant overdoen noch verhueren buyten mijns sheren
consent
ende oirlof. In den bouwe sal mijn heer setten enen scoter dan sal Willem
die kost gheven. Ende dat koorne salmen op dat velt deylen half ende half ende mijns
heren gedeelte in mijns heren berge bringen ende men sal mijns heren thienden voir
uut tellen.

Men sal die beesten afsetten wanneer dattet profijt ende orber is. Ende die wol salmen
half

ende half deylen ende oick die beesten het zijn koyen, peerden, vercken, scapen, behel-
teliken
mijns heren beest smaeltienden voir uut. Item oft geboirden dat Willem voirscreven
binnen
dese 6 iaren storf dat god verbieden moet soe sal mijn prelaet mitten erfenamen reke-
nen ende geloofte voir die schult nemen of borghe ende van sijn bouwinghe mitter
lant voirscreven vrij weder aen sonder yemants bekronen of weder seggen nemen.

Dit is geschiet te Culemborch in bywesen mijns eerweerdighe heer Herman van Rossem
prelaet,
Aert die Man, Hubert Cornelliss scepen van Culemborch ende veel anderen guede
mannen in mijns heren huys te Lanxmeer.

Gerekent...

Fixed-rent contracts of large tenants

AM 71, 4r: Lease contract of Stees Krijnen, anno 1529

Stees Krijnen heeft in formen ende manieren hyr nae bescreven den
hof mitten landen in onsen polre mitten landen dat zijn vader te Beest gebruyct
heeft heel ende al als hij't gebruyct te wetenen 80,5 mergen lants inde polre

ende op Beest gelegen den mergen voir derdenhalven [2,5] golden gulden van gewichte
of ander guet payment dair men eenen golden gulden mit betalen mach, vry gelt.
In den yerste sal Stees iairlicx setten anderhalf hondert paten en die becleven
leveren. Ende hij sal iairlicx gheven een aem wijns ende 4 pont kruyts half
gingber half peper. Ende te Paesschen 200 eyer ende eenen botterweghe. Item hij
sal iairlicx gheven 20 capuynen ende 60 hoenren. Item mijn heer sal Stees bestellen
totter tymmeringhe vanden huyse ende kelre steen ende calck ende die dachuren betalen
ende Stees sal die steen ende calck op sinen cost halen ende den arbeyers ende die
metselers die cost gheven. Ende voirt soe sal mijn heer een geboyt aenden huys
leveren ende des sal Stees dat huys boven ende rontom dicht halden ende alzoe theyn-
den
zynen iaren overleveren de salmen Stees dat yerste iaer dye rys wijsen die hij
op sijnen kost sal doen halen. Item Stees sal ainvangen den berch die mijn heer toe
behoirt ende den theynden sijnen iaren soe guet weder overleveren ghelijck hij den
ainvenckt. Item Stees sal alleen die pechter wesen ind nyemant anders dat
mit mijns heren willen. Ende of Stees binnen dese iaren aflunch wordt soe sullen
sijn kinder of zijn erfgenamen of die naeste die mijns heren wil die bouwinghe
bruycken zijn iaren avt. Ende weert sake dattet huys verbranden dair god voer-
sij ende bij Steesen schult of zijnen boden of dienars geschieden soe sal Stees mijnen
heren dat huys weder op timmeren ende alzoe guet leveren ghelijck hij dat
aingevangen heeft des sal mijn heer Stees beschermen mitten privilegien ghelijck
den anderen huerlinck binnen den polre op mijns heren privilegien ind Stees
costen. Ende dese hueringhe sal dueren thien iaer lanck ingaende petri als
men sal scriven dertich ende soe voert iaer tot iaren. Ende die betaeldach sal wesen
martini nae die scharen die een helft ende petri ad cathedram dair naest vol-
genen die ander helft. Dair dit gheschieden zijn bij ende over ghweweest inde yersten
mijn eerweerdighe heer Herman van Rossem abt van Sint Marienweerde, heer Claes
Spierinck capel-
laen, Claes van Beest rintmeester, ende Stees Krinen voirscreven. In oirconden ende
getuychen-
isse der waerheyt zoe zijn dese twe celen [cedulen] gelijck holdende die een uut den
anderen gesneden dorch litteren A B C actum des fridachs post octas pasche anno 29.
Gherekent...

AM 71, 8r: Lease contract for 'De Hoge Sluis', anno 1524
Peter ende Agnes hebben ingehuert ons bouwinghe op die Hoochsluse

mit huys ende hofstat ende lant hyr nae bescreven. Inden yerste 9 mergen lants aen die weteringhe ende dair besiden noch 7 mergen. Noch aen die weteringhe 8 mergen aen tweecampen geheiten die Bottercampen. Item dair naest langhe die Steeghe 11 mergen 4 hont. Item dat Gheerken buytendycx omtrent 2 mergen. Item noch over dye Nye Steeghe 8 mergen in Stenenkamp. Noch dair theynden 5 mergen geheiten die Joffenscampe streckende aen die Prijsche Steghen. Item noch 9 mergen in die Asselcampen aen 3 campen. Item noch 6 mergen inden Ranscampe of Born-
campen. Maken tsamen 65 mergen lants ende 4 hont ter gueder maten nae den olden boecken voir hondert brabants gulden [100 fl.] iairlicx te betalen die een helft tsint
Mertijnsdach ende petri daer naevolgende die anderhelft op verborennisse vander hueren, des sal hij gheven een half aem wijns ende 2 pont kruts ende 12 capuynen ende 13 volwassen hoenre. Ende te paesschen 200 eyer mit 1 botterwegghe. Ende totten onraet vander moelen 2 stuiver hollants van elcken mergen sal hij gheven. Ende hij sal gehalden sijn totter gewoenliken dienst des cloost-ers.
Ende hij en sal de bouwinghe nyemants overdoen sonder mijns heren consent. Item Peter
sal oick hebben dat gebruyck vanden willighen vanden Spuelreshaghe nae willighes recht. Item Peter sal oick setten iairlicx 200 paten die beholdenen te cleven 3 iair lanck. Item Peter heeft noch ingehuert 3 mergen inde Asselcamp die Aert Wessels plach te bruycken tsiaers voir 3 goltgulden. Ende noch sijnt voirwerde dat Peter sal dat huys holden dackdicht ende wantdicht ende alzoe over leveren teynden sijn iaren.
Ende waert sake dattet huys verbrant bij oirloghen of vede dorch des cloosters schult soe sal mijn heer dat huys weder op doen timmeren. Ende waert dat huys dorch zijn schulden
of versumernisse of sijns huysgesinne soe sal Peter dat weder op doen maken op sijnen kost sonder mijns heren koste. Ende dese voirwerden sal dueren 11 iaer lanck ingaende
anno 1525. Actum anno 1524 inden 11 dach vande meert.
Gerekent...

AM 74, 97l: Lease contract for 'In de Hage', anno 1542
Buren: Den bouhoff inden Haghe

In nomine patris et filii et spiritus sancti amen. Nunc ind apebaer
is allen den ghenen dye dessen apen cedelen syen offt
horen leesen wij dat mijn eerweerdighe heer heer Johan Hoen van
Karttijls abt ind prelaet doer dye gratien godes to Mari-
enwerdt averkomen in hunre uut geghijven hefft al
sullicken bouhof geheiten inden Hage den eerbare
Gijsbrech den Keijsser seven iaer lanck ind sijn tijt sal
inaen Petri ad Cathedram anno 15 nostre salutis 42.
Item so ist gevorrewaert dat Gijsbrech vorscreven sinnen alde
besten orber doen sall to des landes profijt ind den mest
dye hij iaerlich maeck sal den vorscreven Gijsbrech op het lant
vuren al so wel het leste iaer als dat eeyrste. Item noch ist
geworrewaert dat Gijsbrech vorscreven sal iaerlich ghijven Marthini
eenen goeden Doertschen aem Rinschen wijn. Item noch
sal Gijsbrech vorscrevcen iaerlich ghijven sees [6] pont crudes hallef
genver ind hallef peper. Item noch sinnet voruoerden dat
hij noch sal ghijven 150 pont botteren. Item noch sinnent voruarde
dat Ghijsbrech vorscreven leveren sal vijfftich hoenderen 25 capunen
ind 25 smale hoenderen. Item noch sinnnent voruoerden dat
Gijbrech vorscreven sal beuaren den dienst ind dijken op sine last
buten mijns eerweerdighen kosten gelijck als hij tot noch to gewontelick
is geweest to doen. Item noch sinnent vorwoerden dat Gijsbrech
vorscreven mijnen eerweerdighe heer den abt sal ghijven allen iaren vrij gelt.
Item mijnen eerweerdighen heer den abdt sal allen nijen verken als mollen
sclusen ind dergeliken doen maken dan dye usantsij baven
ind beneden is. Item noch sal Gijsbrech vorscreven dye voer ransoenen
betalen op en ygelicken margen 11 stuiver Brabants. Item Gijsbrech vorscreven sal
ghijven den camerlinck ind opperkock eenen Carolus gulden.
Item noch sal Gijsbrech vorscreven iaerlich setten 200 willigen paten
ind dye becleven hoelden. Item noch sal Gijsbrech vorscreven sculdich
wesen to betalen mijnen eerweerdighe heer voer mijnen eerweerdighen heeren wijn
drye golden gulden eens. Item noch sinnent vorwaerden off het
zake waer dat dye bouhofft add brande in geheel offt in
deell bij hoer eygen vuer offt bij hoeren versumennisse so sall

Gijsbrech vorscreven dye huysinge weder om op sijnen kost op timmeren ind buten
allen schaden mijnen eerweerdighen heere. Item noch sinnent vorwoerden dat Gijs-

brech

vorscreven dye huyssinghe op sinen kost onder hoelden sal wantdicht ind
dack dicht. Item Gijbrech vorscreven sal iaerlich ghijven voer een ygelicken
marrighen tot 70 marrigen int getall uutgenamen dat lant het
welick van Derick van Buren angekomen is 3,5 Carolus gulden
ind vijff gulden Brabans iaerlich op den hoep. Item Gijsbrech vorscreven sall
sculdich sijn mijnen eerweerdighen heer iaerlich to betalen allsullick gelt gelijck
den eenen coepman ind den anderen coepman to harttigen hof ind
to horen van melkaneren scheiden welick terminnen sinnen hoer
betalingen Marthini et Petri ad Cathedram hier bij ind aver gewest
an Gijsbrech sijden Gijsbrech vorscreven ind Joestken sijn huysvrou ind Thonis
Goerts ind Coertsten Derrycks ind an mijns heeren sijt mijn heer
sijn eerweerdighe ind dye pastoer to Beest ind pastoer in Lanxmer Coelwagen
ind onssen reintmeister Reijner Becker. Actum anno salutis nostre 15-
42 in abbatia nostra.

AM 73, 198: Lease conditions for the Farms in 'De Polder' and Deil
In nomen patris et filii et spiritus sancit amen
Item dit nabescreven scryfft is inhoeldende dye vorwerden ind conventie
van allen onssen 5 bouhaven gelegen in onssen pelder nd oeck dye
2 bouhaven gelegen op Deyl indye selleffte formen ind manyren als
dye eene hielt so synnen dye anderen oeck mit ins averkome uutgena-
men als dat dye eene bouhoff van landes wegen beter is dan dye ander so
binne wij na pennincswege allene meitten lande nuet ten anders
over gekomen na mergentael anders niet uutgescheyden van enyge
vorwaerden nur na vorwarden dye eene bouman sal gyven
ind schuldich wesen to voldaen als ten anderen in mmanyren ind vor-
warden hier na bescreven welick mijne weerde voervader heer
Jan Hoen meit ten bouluyden overkomen is.

Lease conditions for 'De Hoge Sluis' used by Peter Willems[37]
In vorwaerden ind maniren hier na bescreven heeben wij heer Jan Hoen
van Karttijls bijde genaden godes abt ind prelaet to Marienwerdt
verhuert ind in pachten geghijven Peter Willems op dye Hoge Scluse

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[37] The conditions provided for the other large farms in the 'Mariënweerdse Polder' do not provide general stipulations, but only list the price, acreage and payments. Given this short preamble, the Abbey seems to have used the description of Peter Willems' contract as an example of the other contracts.

eenen bouhoff daer hij dus lange op gewont hefft. Inden eyrsten so
sal Peter vorscreven in huren beholden al sullicken lant als hij tot noch
to van ons gebrueckt hefft buten staende dye gyren(?) welick wij
weder an ons nemen, groet wesende desse bouwingen int getal vijff
ind seventich [75] mergen bij den hoep sonder maet ind hij sal gyven voer
ellicke merge tot vijffenseventich mergen to vier brabants gulden gevalueert
ind vrij gelts verschynende alle iaer sunct martsen [Sint Maarten] nader schare dye
eene helleff ind sunct peter ad cathedram daer na dye anderhellefft
ind hij sal noch ghijven op ellicker merge alle iaer tot vijffenseven-
tich mergen to vier stuiver brabants to mollegelt. Noch sal hij ghijven ind beta-
len alle iaer anderhallef hondert [150] pont botteren daer van alle iaer
hondert to leveren voer sunct Jan Baptiste ind dye ander vijfftich
voer Bamis [1 October] daer naestvolgende. Ind noch alle iaer eenen doertschen
aem goeden rinschen wijn sunct Martchen to betalen noch alle iaer
6 pont cruyts halleff genver ind hallef peper. Item noch sal hij
iaerlich gheven vijffentwintich capunen ind 25 smale hoenderen
ind dye capunen to leveren na sunct Martchen ind dye smale hoenderen
altijt to sijnen schoensten. Item hij sal op het leste van sijnen iaren
25 mergen weylants twe iarichgen drijs averleveren. Item Peter
vorscreven sal desse bouwinge to wete dye 75 mergen meit dye 3
mergen gelegen an dye huvelcampen anvangen ind in huren
gaen sunct Peter ad Cathedram anno 44 ind sal duren 8 iaer
lanck. Ind Peter vorscreven sal ghijven vrij gelt sonder van enyger saken

to kortten niet uutgescheyden behoeltelick offt Peter vorscreven bij eenygen
onbehoertelicken offt ongewoentelicken scattingen bijden voerst vander
landen offt sijnen officiren gesat woerdde om to betalen vijfftijn golden gulden
van gewycht offt daer aver so sal Peter vorscreven dye penningen verleggen ind
ons dye twe delen koertten ind Peter sal dat daerden deel daer van quijt
blijven. Item offt Peter vorscreven gesat woerde op 7 golden gulden offt hoeger tot
15 golden gulden to exclusive so sal Peter oeck dye penningen heel verleggen
ind dye helleft daer van quijt blijven. Item offt Peter vorscreven gesat woerde
op ses golden gulden offt daer beneden so sal Peter oeck dye penningen heel
verleggen ind heel quijt blijven. Item Peter vorscreven sal een dusent goede
paten setten binnen den vorscreven acht iaren ind dye selven twe iarich becleven
leveren ind voert ellick oelde villyge dye verdorret offt omme weirt
sal hij wederom in dye plaets twe paten setten. Peter Willems vorscreven sal

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sijn scapen ind verricken moegen laten hoeden tegens sins hoech lant all-
leen ind niet voerderen ind hij sal sijn verreken laten zuigen dye an den
dijck gaen sullen ind bij al so offt eenygen verreken an den dijck leit
hoeden ind niet gezuiget en waren so sal Peter gehoelden sijn den dijck
to maken weer als sijn bedrieff strecht buten onssen kosten so duck hij
dese van ons gesoecht sal werden oeck mede so hebben wij Peter vorscreven
to gelaten als dat onsse verrekens hierder dye verreken niet veerder
hoden en sal anden onlanden van dye Linghe dan so veer Peters gerijken
strecht tot den dijck to offt Peter sal onssen verreken moegen scutten wan-
neer hij ons dese eenmael verwijctigt hefft. Noch sal Peter ons oeck iaerlich
denen meit knechten wagen ind peerden tijn volle dagen tot wat
tijden wij sijnder begheren sullen offt voer ellicken dach sal hij ons ghijven als
hij niet en deent 14 stuiver brabants angereden gelde ind hij sal ons oeck denen iaer-
lich twe volle daghen meit sijnen ploeg peerden ind knechten op wat tijden
dat wij des begheren offt hij sal ons voer ellicken dach ghijven an gereden gelde
eenen Philips gulden so duenck hij des niet en doet behoeltelicken deis dat wij
hem des 2 offt 3 dagen to voren sullen adverteren. Oeck sal Peter geen
lantvoerder mogen verhuren buten onssen consent noch in scheyden ind
deijlen van onssen beesten sal Peter peerden ende kouen setten ind dan sullen
wij daer over loten mer angaende dye smale beesten verreken ind scapen
dye sal Peter setten ind wij sullen kysen. Item hij sal alle dye huyssinge
ind berrichgen dack dicht ind wantdicht hoelden ind onderhoelden op sijnen
kosten ind ten enden sijnen iaren al so weder averleveren behoeltelicken offt
daer eenygen nijen verreken velen to maken so sullen wij den meyster
loenen ind Peter sal hem dye kost ghijven. Ind Peter en sal ghenen nijen werrick
mogen laten maken buten onssen consent. Noch offt dye bouinghe int

geheell offt ind deill aff branden daer god nochtans voer behoelden will
doer versoemenis Peters offt sijn huysgesin offt bij sijnre selleffs vuer
so sal Peter dat weder om op tymmeren ind ons vergoederen op sijns
eugen kost ind last. Noch sullen wij weder om anvangen moeghen allen
hoechlant tot dye bouwinghe vorscreven staende so geringhe Peter vorscreven
dye leste schaer vanden acht iaren vanden landen hefft ind hij sal
moeghen daer ende sijnen oerber doen sonder yemants becroenen
ind hij sal oeck gheen meissinghe vanden bouwinghe moeghen vuren
offt vercopen ind hij sal ons oeck dat hoge lant niet uut bouwen
offt verderreven op dye lesten iaren offt in dien hij contrarie deden

dat sal hij ons vermogen in seggen van 4 onpertijgen bouluden
dye wij an beiden sijden daer to nemen sullen. Item Peter sal ons
hier van goede suffitienten borgen stellen voer scepens daer wij
an gehoelden sijn ind dat van iaer tot iaer ind ons daer van be-
seygeltheit to leveren. Noch hebben wij Peteren to gelaten nu
gegonnen offt het saken waer datter binnen dessen acht iaren inbreeck
van dijcken offt openwaren geweilt so dat Peter vorscreven sijn besiden
schaer hij als dan op dye bouwinghe hadde vanden selleffden water
vergenck ind dat quaem dat hij dat selleffde lant offt oeck anderen
in der selleffder bouwinghe hij oeck gepayt hadde to zeijen avermis
des water voer hallefft meij meitten ploech niet bekomen en kust-
om to bezeien sullen wij hem daer van doen als men baven ind
beneden daer van doet. Oeck mede ist bevorrewaert dat allen
actien dye Peter op ons polde moegen hebben in eenijger manieren
to dessen daghen to niet daer van uutgescheijden sullen to doet ind
to niet sijn alle dinck sonder arrich ind list offt bedroech. Actum desse
vorscreven huer inden iaren ons heeren dusent vijfhondert ind twe en
viertich [1542] den achten dach meij integenwoerdicheit heer Matheus
Coelwagen onsse cappellaen, heer Willem van Grunxveld pastor
van Ackoy, Reijner Beeker rentmeister, ind Henrick onssen camer-
linck. Ende dessen cedulen isser twe gelijck hoeldende van woerde to
woerde daer wij dye een van hoelden sullen ind Peter dye ander ind
so wye dat sijn cedel verleyt offt achterhoelt dye sal lijden an dye
andere. Ind oeck noch tot meeder sekerheit wille so hebben wij Jan
Hoen van Karttijls abdt to Marienwerdt ind Peter Willems
vorscreven ellick ons eijgen hant offt merrick huer onder op spatuum
beyden desser cedelen gesat ind voert gebeden Merthen Henricks
ind Gijsbert van Beest scepen to Beest meit Gijsbert Pyck
bastert geswaren fernier(?) to Beest dat meit ind aver onss tewillir
behauden meit hoer gewontelicken hant offt merrick het weluk
Marthen Henricks ind Gijsbert van Beest ind Gijsbert Pyck bastert
doer beden dese eerweerdighe heer ind prelaet ind Peter Willems gerne
gedaen hebben. Actum anno domini 1544 den 7 dach Martii.

C   Chronology of the farms

The chronology is centered on around a few particular years wherein many new leases started and lease books provide rich detail. These dates seem to coincide rather well with transitions between abbots and the war periods of the mid 1520s and 1542/43.

The first date is around 1531 when the new abbot, Jan Sterck van den Berghe, seems to restore order by administering two dense lease books (AM 70, 71). Many of the leases described in these two books have been initiated by his two predecessors, Hendrik van de Voort (1529-31) and especially Herman van Rossum (1513-29). Jan Sterck provides an overview of the leases initiated around 1530 and many new ones up to 1532 at which point most of the Abbey's land seems to be leased out. In addition, Jan Sterck keeps track of many payments in those initial years. In addition, he starts administering the new leases that—given the bulk of new contracts signed around 1530 with terms of 6, 8 or, mostly, 10 years duration—he initiates at the end of the 1530s. However, his term ends in 1539 at which point the accounting of the new leases is not finished—not in the initial lease books (AM 70 and 71), nor in his new lease book (AM 74). Ultimately, the accounting of Jan Sterck is more dense than structured, with large overlap between the different account books and several gaps especially for the large tenants.

The next abbot, Johan Hoen van Karttijls (1539-44), complains about the administrative legacy of his predecessor noting on several occasions in the lease books he continues, and his own detailed but incomplete lease book (AM 72), that Jan Sterck has apparently continued the leases orally without leaving any written note of such extensions (AM 72). This complaint corresponds with a clear difference between the new leases that Jan Sterck does administer and that of his followers: those signed by Jan Sterck often involve extensions of existing leases several years in advance of the end of the initial leases. Later abbots date the signing of new lease agreements predominantly in the year in which the new lease takes effect. Accordingly, the period from the mid 1530s up to the mid 1540s provide documentation of several leases and payments but do not allow a full reconstruction of the leases.

This is especially evident in the leases signed by abbot Peter van Zuyren (1545-61), which is the next abbot that generates clear administration of the leases (AM 73 and 74 and the continuation of AM 72). Apparently, Johan Hoen van Karttijls did not manage to update the lease accounts sufficiently before his death in 1544. The next two abbots, Jacob van Esden (1544) and Jan Korver van Goch (1544-45) die quickly after their appointments. Another possible explanation for this gap in the sources is the final upsurge of the Guelders Wars in 1542/43. The abbey is again affected by the conflict, but damages seem rather limited. Nonetheless, the Abbey acquits roughly half of the

leases for these two years. Administration of payments and new leases starts roughly in 1546/47. Another explanation is that the contracts arranged in the late 1530s were usually signed for 6 or 8 years, which implies these contracts would expire in the mid 1540s.

The five farms in 'het Mariënweerdse Veld'

'Op den Oest', or 'De Neust' .
<u>Willem Jans and his wife Janneken</u>

- 1528/1530? Share contract with unspecified conditions.

    - This seems to be a new farm given the long description of the individual plots of land, with mention of previous tenants, that are combined into this farm.

    - First payments from 1528. First effective contract from 1536. (No contractual terms in AM 70; First passage in AM 71 gives the 1536 contract, but later ones—scan 57r and 80r—refer to a previous contract from 1530, also a share contract.)

- 1536: share contract with 6 year duration for a farm of 104 $\frac{1}{6}$ *morgen*. The share terms refer to livestock and grains and to taxes, but after accounting for tithes—on grains and *smaltienden* concerning livestock (AM 71, scan 9r).

    - A fine of 1 Carolus guilder for each *morgen* that is not used.

    - For each cow 20 pounds of butter per year, 10 pounds for each heifer.[38] Per year 100 pieces of cheese sheep milk and for Easter another 8 pounds of butter and 200 eggs. One *korenmorgen*, half an *aem* of wine, 1 pound peper, one pound ginger, 12 capons and 12 grouses.

- Unclear: payments in 1545 from Willem Jans, but no contractual terms (AM 74, scan 2r).

    - Almost certainly a share contract, given split earnings from sales of livestock. Most likely: Willem Jans engaged in another 6-year share contract from 1542 to 1548.

    - Willem Jans dies in 1547. His children finish his term and have almost no debts. A final settlement is made in 1553 where the abbot acquits

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[38] A heifer (*vaars*) is a young cow that gave birth to at most one calf.

the children a debt of about 115 Carolus guilder because of unaccounted payments and services that they and their parents have performed to Abbot Jan Sterck vanden Berghe (AM 74, 3l).

Gijsbert Claes and his wife Woveken Willemsdochter (daughter of Willem Jans)

- 1548: Fixed rent contract with 7 year duration, of 76 $\frac{1}{3}$ *morgen* for 267 Carolus guilder, which will be expanded with another 11 *morgen* from 1551 onward (AM 73, 224l).

  – In addition, 150 pounds of butter, 1 *aem* of wine, 3 pounds of pepper, 3 pounds of ginger, 25 capuns and 25 grouses.

- 1555: Fixed rent and term of 8 years of 'Op den Hoest', which is now 87 *morgen*. The other contractual stipulations are unreadable due to damages to the manuscript (AM 73, 224l).

'De Gansheuvel'   .
Reijer Hermans and his wife Janneken

- 1529: share contract with 6 year duration for 81 $\frac{5}{6}$ *morgen*—49 $\frac{5}{6}$ highland and 32 lowland. The share terms refer to livestock and grains and to taxes, but after accounting for tithes—on grains and *smaltienden* concerning livestock (AM 71, scan 7r). Explicitly stating the sharing of wool.

  – This seems to be a new farm given the long description of the individual plots of land, with mention of previous tenants, that are combined into this farm.

  – Terms include a fine of 1 Brabantine guilder for each unused *morgen*. For each cow 14 pound butter and for Easter another 8 pound and 200 eggs, 150 cheeses of sheep milk, one *korenmorgen*, one pound pepper, one pound ginger, 12 capons, 12 grouses. Wine is not mentioned in the stipulations, but it is in the payments and the calculations of arrears. This is repeated, together with a not that requires the tenants to plant willows (AM 71, 57l).

- Reijer Hermans' lease is terminated in late 1531 or early 1532 because he had harvested and stored his grains without supervision of the abbey, which is a clear violation of the abbey's stipulations that they should provide the people to harvest, split and store the grains in case of share contracts (AM 71, 57l).

Rolof Aerts and his wife Gertruijt

- 1532: a share contract with 6 year duration concerning the same land as Reijer Hermans used to lease (AM 71, 78l).[39]

  - Interestingly, Rolof Aerts and Gertruijt have a fixed rent-imposition for land that they sow: they are required to pay 2 Philips guilder per sown *morgen*, suggesting that the abbey is keen on preventing new problems in the splitting of harvests—although they do not repeat this term for other share tenants.

  - No wine, pepper or ginger but a fixed sum of 13 Brabantine guilder for these instead.

- 1538: Another 'semi-share contract' of 8 year duration for the same land plus an additional 6 *morgen* (AM 71, 79l).

  - Per year, for each sown *morgen* 2 Philips guilder, 25 pound butter for each cow, 14 pound per heifer and during Easter another 8 pound of butter and 300 eggs, 150 cheeses (of sheep milk?), 25 capons, 25 grouses, for wine and spices 13 Brabantine guilder, and the usual *korenmorgen*. In addition, they are required to plant 150 willows per year. In return, the tenants receive a ton of beer from the abbey.

- Rolof Aerts does not seem to live until the end of their term, and Gertruijt does not seem to finish it by herself. Their son Wolter Rolofs continues the term, but the sources do not allow us to explicitly date this transition.

Wolter Rolofs (son of Rolof Aerts and Gertruijt) and his wife Elysabeth

- 1546: Fixed rent and term of 8 years for 'De Gansheuvel' of now 76 *morgen* for 3.5 Brabantine guilder per *morgen* or 266 guilder in total (AM 73, 217l).

  - Fixed lease implies that Woltor Rolofs pays the full amount of taxes. In addition, 150 pound butter, one *aem* wine, 3 pounds of pepper, 3 pounds of ginger, 25 capons, 25 grouses

- 1554: Fixed rent and term of 8 years for 'De Gansheuvel' of now 79 *morgen* for 4.5 Carolus guilder per *morgen* or 356 Carolus guilder in total (AM 73, 217l).

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[39] *Note that the contract includes a long note describing the imposition of an extraordinary taxation by the Estates of Guelders wherein the abbey is required to contribute, enraging the abbot.*

'De Hoge Sluis' .

Peter Willems and his wife Agnes

- 1525: Fixed rent and term rent of 11 years for 'De Hoge Sluis' consisting of 65 $\frac{2}{3}$ *morgen* for 100 Brabantine guilder (AM 71, 8r).

    - This seems to be a new farm given the long description of the individual plots of land, with mention of previous tenants, that are combined into this farm. This might explain the low rent per *morgen*.

    - In addition, half an *aem* wine, 2 pound spices, 12 capons, 13 grouses, 200 eggs and 8 pounds of butter for Easter. Full payment of taxes. In addition, he has the right to use the willows but has to plant 200 new willows per year.

    - They have rented another 3 *morgen* of land for 3 Golden guilders. This stipulation is necessary given that tenants are not allowed to rent additional land without the abbot's explicit permission—hence, all land that they rent is listed in these contracts.

    - The arrears of Peter and Agnes are increasing from 1532 onward, from 70.75 guilders in 1533 to 126.75 guilders in 1534. However, they have already agreed to switch to a share contract in 1535 by 1533 (see also AM 70, 11r).

- 1535: Share contract, but no additional information given for the new contract (AM 71, 57r).

    - As soon as the share contract starts, the abbey buys half of the livestock on 'De Hoge Sluis' with the result that Peter and Agnes are 39.40 guilders ahead of their payments.

- 1544: Fixed rent and term of 8 years for 'De Hoge Sluis' now 75 *morgen* for 4 Carolus guilder per *morgen* or 300 Carolus guilder (AM 73, 198l).

    - Full taxation, 150 pounds of butter, one *aem* wine, 3 pounds of pepper, 3 pounds of ginger, 25 capons, 25 grouses.

    - Has an additional 3 *morgen*.

    - Peter dies between 1547 and 1550 and leaves his heirs a debt to the abbey of 259 Carolus guilder.

Berent Goerts and his wife Wein (daughter of Peter Willems and Agnes)

- 1552: Fixed rent and term of 8 years for 'De Hoge Sluis' now 78 *morgen* for 4.25 Carolus per *morgen* or 331.5 Carolus guilder (AM 73, 198l).

  - The same additional payments as before.

  - Debt of 255 Carolus in 1551.

'De Hoge Spijck' .

Stees Krijnen and his wife Hilleken. Note that they do not yet have possession of the farmstead 'De Hoge Spijck'. Instead, they are supposed to build a new farmstead, which is paid for by the Abbey.

- 1530: Fixed rent and term of 10 years for 80.5 *morgen* for 2.50 Golden guilder per *morgen* or 201.25 Golden guilder—valued at 281.75 Brabantine guilder (AM 71, 7l).

  - Farm has previously been used by Stees' father.

  - Explicit reference to construction works. Abbey pays wages and materials, Stees pays for transportation of materials as well as room and board of the laborers.

  - Additional payments: payments free of taxes, planting of 150 willows, one *aem* wine, 2 pounds of pepper, 2 of ginger, 200 eggs and 8 pounds of butter for Easter, 20 capons, 60 grouses.

  - Minor debt of 67.5 Brabantine guilder in 1531. Debt fluctuates in the following years and is fully paid in 1534. Debt increases to 200 Gold guilder in 1535, which he pays mostly be selling livestock to the Abbey. Probably in preparation of his transition to a share contract, which should be dated at 1536 (when all these payments are made).

- 1536: Share contract starts but no specifications mentioned (AM 71, 76l).

  - Note that the Stees seems to start construction works only from this year onward.

- Unclear date, somewhere between 1542 and 1545, fixed rent and term of substantially reduced farm of 55 *morgen* for 4 Carolus guilder per *morgen* or 220 Carolus guilder (AM 73, 206l).

  - Many parcels of land brought in lease in AM 73 that were previously part of Stees' farm. These leases start in 1545 suggesting that indeed the above mentioned lease started in 1545 or before.

- – Payments, including to usual capons, wine and spices, up to 1547 at which point Stees has a debt of 163 Carolus guilder.
- – Stees dies between 1546 and 1549. His widow continues the lease and has a minor debt of 94 Carolus guilder in 1549.

Hendrick die Roy and his wife Hilleken (and Stees' widow)

- 1553: Fixed rent and term of 8 years of 55 *morgen* for 4.5 Carolus guilder per *morgen* or 247.5 Carolus guilder (AM 73, 206l).

  - – Has to pay 8 pounds of sugar in addition to the usual wine, spices, capons and stuff.

- 1561: Fixed rent and term of 8 years now including the farmstead 'De Hoge Spijck' and total of 59 *morgen* for 6 Carolus guilder per *morgen*. Will be expanded with another 6 *morgen* during this term (AM 73, 206l).

  - – The usual additional payments. For butter, wine, sugar and spices he pays 26 Carolus guilder per year.

The fifth farm, without a name .

Goessen Gijsberts and his wife Neel

- 1530: Share contract with duration of 6 years for a farm of 55 *morgen* (AM 71, 6r). The size of the farm is not mentioned in the initial contract but can be found further down in the lease book (AM 71, 77l).

  - – They have leased this farm before 1530 as well, also with a share contract. They have an outstanding debt of 67.3 Brabantine guilder.
  - – Explicit reference to construction works. Abbey pays wages and materials, Goessen pays for transportation of materials as well as room and board of the laborers.
  - – In addition, half an *aem* wine, 1 pound of pepper, 1 of ginger, 13 capons, 13 grouses, 200 eggs and 8 pound of butter for Easter, planting 100 willows.
  - – Minor debts of 47 Brabantine guilder in 1533

- 1536: Lease is extended, but conditions are unknown. (He pays his *ransoen* in 1538, AM 70, 7r.)

Cornelis Derricks and his wife Thonisken

- 1544(?): Fixed rent and term of 58 *morgen* for 4.5 Carolus guilder per *morgen* for the first 46 and 4 Carolus guilder for the final 12 *morgen* or 258 Carolus guilder (AM 73, 211l).

    – In addition, there is mention of a 104 Carolus guilder debt stemming from cattle and horses Cornelis bought from the Abbey (AM 74, 6r). Suggests that Cornelis has bought himself into this previously sharecropped farm.

    – Total debt over his first two years is 360 Carolus guilder, excluding wine, spices and the other usual additions (AM 74, 6r). In 1547, his debt has reached 555 Carolus guilder and even 569 Carolus guilder in 1550. Nonetheless, he extends his lease.

- 1552: Fixed rent and term of 8 years and the same 58 *morgen* 4.5 Carolus guilder per *morgen* or 266 Carolus guilder (AM 73, 211l).

    – Usual additional payment plus 8 pounds of sugar.

The two farms in Deil

Deil I, farm without a name .
Jan Huygens

- 1532: Share contract but the size of the farm is not given (AM 70, 179r). Judging from the number of capons and grouses, it is at least 40 *morgen*. The normal rate is one capon or grouse per *morgen* but several of the large tenants have much more land than suggested by the number of grouses and capons.

    – Has leased this farm before and agree on this new lease already in 1529 (AM 70, 179r). Has no initial debts and their debts remain negligible throughout this term.

    – Additional stuff: 1 *aem* wine, 1.5 pound paper, 1.5 pound ginger, 20 capons, 20 grouses and planting 200 willows.

- No new contracts found until an account of 1546 over the years 1543, '44 and '45 with Jan Huygens' children—they have a debt of 33.375 Carolus guilder at that point (AM 73, 185l). The accounts mention the 'splitting' of the livestock suggesting that Jan Huygens switched from a share contract to a fixed contract at the start of this term, approximately 1542 or '43 (AM 73, 185l).

Cornelis Jans and Cornelis Jans, sons of Jan Huygens

- 1546: Fixed rent and term of 8 year for a farm of 78 *morgen* for 292.5 Carolus guilder (AM 73, 185l).

  - Almost no debts. In 1550, the Abbey builds a new farmstead on this farm, costing 458.25 Carolus guilder (AM 73, 186l).

- 1554: Fixed rent and term of 8 year for the same farm of 78 *morgen* for 4.5 Carolus guilder or 351 Carolus guilder (AM 73, 185l).

  - Another 8 pounds of sugar in addition to the usual stuff—which we do not know for this farm.

Deil II, farm without a name   .

Willem (or Peter Willem) Rutgers and his wife Thonisken

- 1529: Share contract of 6 year for an unspecified farm (AM 71, 66r, 103l). No mention of capons or grouses and, therefore, hard to estimate the size of the farm.

  - No mention of the previous status of this farm.
  - In addition, 3 pounds of pepper, 3 of ginger and 6 six pounds of sugar.

- Lease continues after 1535 without explicit terms. However, the mention of the 'splitting of livestock' suggests that Willem Rutgers switches to a fixed rent contract around the year 1535 or '36 (AM 71, 103l). In addition, although he still sells cattle and horses, the accounting of such payments simply mention full valuations of such livestock instead of mentioning the value of the Abbey's share in the sale—as is normal in the accounting of the share contracts.

  - Thonisken, now widow of Willem Rutgers, accounts with the Abbey in 1546 over the years from 1542 up to 1546. She has a minor debt of 13 Carolus guilder

Henrick Everts and his wife Thonisken (widow of Willem Rutgers)

- 1547: Fixed rent and term of 8 years for a farm of 77.5 *morgen* for 3.75 Carolus guilder per *morgen* or 290.625 Carolus guilder.

  - Debt of 383 Carolus guilder in 1549, reduced to 122 Carolus guilder in 1552.

- 1555: Fixed rent and term of 8 years for the same farm of now 77.83 *morgen* for 4.5 Carolus guilder per *morgen* or 350.5 Carolus guilder.

– Additional payments: 8 pounds of sugar, next to the usual.

The three farms near Buren

The farm near Asch .

<u>Johan Pilgroms van Asch and his wife Marie(ken)</u>

- Unclear lease with first payments from 1534. No evidence to suggest a share contract (AM 70, 173. NB in Johan Hoen's handwriting). No mention of livestock

  – The size of this farm is unclear, but payments in kind in 1538 suggest a sizable farm (200 eggs, 60 pounds of butter). Another mention of 54 Brabantine guilders paid in 1538, which is only part of the lease sum of that year (AM 70, 173r).

  – Mention of a 'bouwing', 9 *morgen* op Asch and 8.5 *morgen* in 'De Polder' (AM 70, 174l).

- 1540: Fixed rent and term of 9 years for 9 *morgen* (previously used by Jan van Avezaet) for 15 Golden guilder and 9 grouses (AM 70, 174r).

- 1539: Fixed rent and term of 6 years for 8.5 *morgen* for 45 Brabantine guider, 2 pounds of spices and 8 grouses (AM 70, 174r).

  – Again, while accounting their debts, Johan Hoen mentions a 'bouwing' in addition to the 9 and 8.5 *morgen* (AM 70, 174r).

  – Johan Pilgroms dies somewhere in the early 1540s. Not clear when.

<u>Alert Gerits and his wife Marieken van Asch (Johan Pilgrom's widow)</u>

- 1546: Fixed rent and term of 8 years for the total farm of now 49 *morgen* for 136 Carolus guilder (AM 73, 182r).

  – In addition, 49 grouses and one *aem* wine.

  – Almost no build-up of debts.

- 1554: Fixed rent and term of 8 years for the farm of no 50 *morgen* for the same price (AM 73, 182r).

'In de Haghe' in Buurmalsen .

Gijsbert de Keyser and his wife Joesken (mentioned from 1542 onward)

- Unclear lease. Payments from 1533 onward, one payment of 2 cows but exclusively cash until 1535 otherwise (AM 70, 170r). More payments using livestock thereafter, but nothing to suggest share contract.

- 1540: Payment of 'rantsoen' suggesting that he has concluded a new fixed-rent lease contract. No specifications of this contract (AM 70, 171r). Next page mentions that in 1536 Gijsbert extended his contract by 2 years up to 1542 for the same fixed rent of 150 Brabantine guilder. (Most likely scenario is that this lease started in 1530 as fixed rent for 10 years but eventually extended up to 1542 instead of 1540.)

  - Note that a yearly fixed rent of 150 guilders contracted in the early 1530s is roughly consistent with a farm of ca. 75 *morgen*.

- 1542: Fixed rent and term of 7 years for a farm of 70 *morgen* for 253.5 Carolus guilder (AM 74, 97l).

  - Explicit that Gijsbert is required to put his livestock on this farm and use the manure throughout his term, during his last years as he had done in the first. Additional payments: one *aem* wine, 3 pounds pepper, 3 pounds ginger, 150 pounds of butter, 25 capons, 25 grouses, planting of 200 willows.

  - First account of arrears is made in 1545 with Joesken and her son Jan Gijsberts. They have a debt of 125 Carolus guilder. Gijsbert has died ietween 1542 and 1545. Debt has increased to 427 Carolus guilder in 1547. By 1549 they have a debt of just 49 Carolus guilder, the decrease of their debt is mainly explained by their expenses on construction works at the farm, for which they are compensated by the Abbey (AM 73, 179r).

Joesken, widow of Gijsbert de Keyser and Jan Gijsberts de Keyser

- 1549: Fixed rent and term of 10 years for the same farm of 70 *morgen* for 262.5 Carolus guilder (AM 73, 179r).

  - No further payments specified. Joesken dies in 1553 at which point Jan Gijsberts is apparently required to sign a new lease contract. At this point, they have a debt of 150 Carolus guilder

Jan Gijsberts de Keyser and his brother Jacob

- 1554: Fixed rent and term of 4 years for a farm of the same 70 *morgen* for 4 Carolus guilder per *morgen* or 280 Carolus guilder. In addition to his unknown additional payments, they are required to pay 8 pounds of sugar (AM 73, 179r).

The newly established 'De Vogelenzang' near Buurmalsen .

Jan Henricks and his wife Wijven

- 1546: Fixed rent and term of 10 years for a newly constructed farm with 42 *morgen* for 114 Carolus guilder (AM 73, 181r). No alternative payments specified.

- 1556: Fixed rent and term of 8 years now of 46.5 *morgen* for 162 Carolus guilder.

    - Additional payments now specified: 20 capons, 20 grouses, 3 pounds of pepper, 3 pounds of ginger, 8 Carolus guilder for wine.

One farm near Waardenburg

'De Thielwael' in Neerijnen .

Jan Claes

- 1528: Fixed rent and term of of 10 years for the 'Tyewael' including 32.5 *morgen* and an additional 23 *morgen* for 60 Philipus guilder (AM 70, 97r).

    - Additional payment pf 26 grouses and 26 capons (AM 71, 46r).

    - Enormous debt of 312.5 Holland guilder in 1531. This debt is so large that the Abbey forces Jan Claes to sign a debt contract.

    - The Abbey incurs a loss of almost 200 Brabantine guilder in 1532 because they buy 12 *morgen* full of unharvested wheat and barley from Jan Claes for 11 Hollands guilder per *morgen*. All the grain is destroyed in a hailstorm a couple of days later.

    - No payments are mentions after 1532 except for an undated note (in Jan Sterck's handwriting) stating that this lease was not accounted for 4 years (AM 71, 46r).

Willem Jans

- 1547: Fixed rent and term of 8 years for the farm of 55.5 *morgen* for 178.75 Carolus guilder (AM 73, 177l).

    - First payments recorded for this lease are from 1550.

– Chronology of this farm is untraceable in the lease accounts. It is likely that Willem Jans is a son of Jan Claes—although the name 'Jan' is far too common to be certain without any further confirmation.

The outlying farms in 'het Land van Cuijk'

Three farms in Hal and four in Mill .
The lease dealings at the farms in Hal and Mill follow a different pattern compared to the farms around the Abbey. Leases were generally longer, based on in kind payments of rye supplemented by a small annual payment and never specified in terms of size or other payments. They are mentioned in only one of the lease books (AM 74, 143-151) and in the financial accounts for 'het land van Cuijk' (AM 149-151). There is no evidence for the use of share contracts or the determination of competitive lease prices on these farms. The total yearly income from these seven farms in 1554 is 113 Golden guilders and 113 *malder* rye—a *malder* contains 130 liters (AM 74, 148r).

There are several possible explanations for this stark divergence from the otherwise competitive lease contracts used by the Abbey of Mariënweerd. Most likely, the difference results from the distance between the Abbey and these lands and possible administrative differences as this region around Cuijk used to be an independent fiefdom in the Duchy of Brabant. See also van Bavel (1993, 289-311), although he states that these farms were sharecropped in the 1540s. This, however, is not clear from the source he mentions (AM 151).

'De Weurt' and 'Broek aan de Teersdijk' near Nijmegen .
One of the lease accounts of the Abbey (AM 73) mentions two farms near Nijmegen. Both are initially pawned but they are freed by abbot Peter van Zuyren. The first farm he frees is 'De Weurt' in 1550, a farm of 120 *morgen* (AM 73, 176r). The other farm is 29.5 *morgen* and freed in 1559. Whereas the smaller farm is pawned in 1539 (AM 232), it is unclear when 'De Weurt' was pawned. The only other reference to this farm during the first half of the sixteenth century is a complaint by the sisters of the Maria Magdalena convent near Nijmegen written to the Duke of Guelders in 1534 (AM 187). In their complaint, the sisters claim that they have used 'De Weurt' for a long time but that the Abbey of Mariënweerd has resumed control without their consent. Given that there is no mention of the Abbey pawning this farm around the years 1530—for instance to pay for their debts as a consequence of the mid 1520s episode of the Guelders Wars—it seems likely that 'De Weurt' was pawned already in the fifteenth century.

Once the Abbey frees these two farms they are leased on fixed rent. In both

cases, the first rents are remarkably low—most likely because they are limited by the stipulations in the pawn contracts. For 'De Weurt', the first lease in 1551 stipulates a fixed rent of 200 Carolus guilder for the 120 *morgen*—less than 2 Carolus guilder per *morgen* (AM 73, 176r). Thereafter, the farm is split in two farms of 60 *morgen* with a fixed rent of 175 Carolus guilder for both farms (AM 73, 173-4).

Chapter 4

The Economics of Violence in Natural States

[1] Bram van Besouw, Erik Ansink and Bas van Bavel

Abstract. Violence is key to understanding human interaction and societal development. The natural state of societal organization is that a subset of the population, capable of mustering organized large-scale violence, forms an elite coalition that restrains both violence and coercive appropriation. We highlight key mechanisms underlying such natural states. Our results show that natural states either have a large elite coalition and a high tax rate, or a weak elite and a high level of appropriation by a large group of violence specialists outside the elite, termed warlords. When output elasticity of effort is high, it induces elite members to limit their tax rate, which in turn promotes warlordism. Only when the elite coalition is small but still able to control a sizeable share of production, as a result of its cooperative quality and a low decisiveness of conflict between elite and warlords, do we find comparatively high levels of production and producer welfare. Our results imply that almost all natural states experience continuous coercion exercised by elite members and violence between elite coalitions and warlords. We show that this is not a temporary out-of-equilibrium-situation but a permanent phenomenon, as can most conspicuously be observed in parts of Sub-Saharan Africa. Our model thus illustrates the rigidity of natural states.

4.1 Introduction

Violence is key to understanding human interaction and societal development. A society that is unable to contain violence will be disrupted and cannot be expected to sustain high levels of welfare, as is painfully illustrated by the current situation in Afghanistan, Libya or, perhaps most conspicuously, parts of Sub-Saharan Africa. Countries like Congo, Somalia, and Sudan are almost continuously torn up by extortion and coercion under the threat of violence, factional strife, and intermittent periods of open violence. Such conditions may destroy lives and capital goods, and deter interaction, exchange, investment, trade, and the benefits of specialization that come with trade, leading to significant welfare losses (Hirshleifer 1988; Skaperdas and Syropoulos 2002; North, Wallis, Webb and Weingast 2013).

The ways in which societies cope with the threat of endemic violence are intrinsically linked up with, or embedded in, their economic and political systems. These systems structure the distribution of rents in order to contain the destructive effects of unconstrained violence on production, the economy, and society at large (North, Wallis and Weingast 2009). One archetype of such systems is formed by open, democratic governments, combined with competitive economies, and a state monopoly on violence which limits the incidence of violence. Few societies, however, conform to this pattern. Instead, most historical and many contemporary societies are dominated by a ruling elite that combines exclusive economic, political, and military advantages over the rest of society. Theoretically, such an elite is willing to maintain order—i.e. restrict violence within society—when its benefits of taxing society exceed the cost of maintaining order and the opportunity costs of restricting its own direct appropriation (McGuire and Olson 1996; Acemoglu and Robinson 2006a). However, a high level of the required 'tax rate' and the difficulty of effectively limiting violence pose two possibly serious restrictions on economic development in these natural states.

These potential restrictions can be observed in many historical and contemporaneous societies that may be characterized as natural states, be it the feudal societies of medieval Europe and Japan, or present-day societies in much of Latin America, the Middle East and Africa. In such societies, the elite brings together their political parties, ethnic groups, patronage networks and associated organizations into a coalition, and commits to restrict violence among themselves and, possibly, towards the rest of society. This coalition, in fact, forms 'the state' in natural states, according to North et al. (2009), and in contrast to for instance Bates (2008) who views the elite as separate from the state. Doing so, the coalition can create and distribute rents. Rents may be created by way of monopolies, exclusive rights to trade, subsidies, redistribution of taxes, or privileges to exploit natural resources. Especially in resource-rich areas

such privileges are a crucial source of rents, and a potential source of conflicts, both in historical and present-day natural states, as in many parts of Sub-Saharan Africa, including the Democratic Republic of Congo, with its huge deposits of cobalt, oil, copper, diamond and tin (Olsson and Congdon Fors 2004; Kaiser and Wolters 2013). The latter case illustrates the potential problems for these natural states, as Congo is intermittently plagued by high levels of appropriation—through rent extraction and extortion—by the elite coalition and its rivals, as well as by outbreaks of open violence, as in the 1990s when various militias, political factions and ethnic groups militarily challenged the elite coalition (Bates, Greif and Singh 2002; Bates 2008; Kaiser and Wolters 2013). Arguably, the problem of endemic violence and outbursts of open violence is most severe in countries with abundant natural resources, as the gains from violence are higher and there is more to be reaped (Sachs and Warner 2001; Van der Ploeg 2011). Recent research indicates that the resource curse is not extricably linked to resource abundance, but can materialize in a context of low-quality institutions, where state competence, transparency and democratic accountability are weak; a situation found in natural states (Bulte, Damania and Deacon 2005; Mehlum, Moene and Torvik 2006).

In this paper we present a model to probe the existence of elite coalitions and the economic mechanisms of natural states. Thus, inspired by North et al. (2009), we address why elite members are unable or unwilling to effectively limit violence and, possibly, appropriation in natural states despite the evident welfare losses. We start from the assertion that there is no monopoly on violence in natural states, concentrated in a state. Instead, the capacity to exert violence—or coercion under the threat of violence—in an organized fashion and on substantial scale is concentrated in a small subset of the population, which opportunistically exploits this capacity to appropriate production from the producing population. In this we follow North et al. (2009) and other recent work. Francois, Rainer and Trebbi (2015) convincingly show that the ruling elite in countries in sub-Saharan Africa consists of a group of people who individually have substantial control over parts of the country's population. According to the same authors, the consequence of not incorporating a wide enough range of such individuals is that they might instigate a rebellion through their patronage network—we will refer to such individuals with the capacity to organize and orchestrate large-scale violence as 'violence specialists'. To North et al. (2009), a coalition of such violence specialists is the only possible social structure that limits violence in societies beyond the scope of foraging societies, and apart from modern open democracies.

There exists a substantial literature analyzing incentives in terms of production and offensive or defensive violence activities in anarchical states—in the absence of a

monopoly of violence—cf. Hirshleifer (1988), Skaperdas (1992) and Grossman and Kim (1995). However, the capacity for violence is often dispersed over the entire population. And if hierarchical state structures are considered, then the violence capacities of ordinary producers are often contrasted to an individual or monolithic elite with the capacity to control the entire population that, as a result, has the opportunity to operate as a Leviathan-like source of order (Usher 1989; Hirshleifer 1995; Grossman 2002; Bates et al. 2002; Acemoglu and Robinson 2006a; Konrad and Skaperdas 2012). We stress that the nature of violence considered in this paper, however, is of a higher level than the banditry-type of violence commonly considered in the literature on the economics of conflict and appropriation (for an overview of this literature, see Garfinkel and Skaperdas 2007; Konrad 2009), and that ordinary producers cannot therefore decide to become specialists in violence. In addition, the capacity for large-scale violence is the domain of a small, but substantial group of violence specialists who do not necessarily work together, instead of a monolithic elite or elite group.

Organized and large-scale violence comprises the possibility to assemble small armies of loyal troops or mercenaries. More generally, violence specialists have the skills to generate and maintain a patronage network of followers that can, in turn, be used to exert violence—or coercion—on others. In our model, violence specialists face a choice either to join forces in a coalition of equals wherein they accept to respect each other's privileges and income, or to individually exploit their violence capacities for appropriative activities as 'warlords'. This coalition—subsequently termed the elite coalition—provides elite members with relative security over specific sources of income. This, in turn, induces the elite members to take account of the effects of their appropriation on production. Warlords, in contrast, lacking the support and accepted privileges characterizing the elite, do not incorporate the effect of their appropriation on production into their decision and, thus, appropriate all production under their control. Warlords may be the leaders of guerilla movements (e.g. Colombia), revolting militias (Somalia) or the leaders of independent clans or tribes (Afghanistan). The number of warlords relative to elite members can vary over time. Examples of rapidly growing numbers of warlords are Liberia in the 1980s, as officers with their soldiers left the government army and turned predatory (Bates et al. 2002); or the earlier cited case of Congo in the 1990s (Bates et al. 2002; Bates 2008; Kaiser and Wolters 2013). Although warlords do not take into account how their appropriation affects production, their appropriation is not necessarily violent. Instead, their appropriation generally takes place as coercion under the threat of violence even though their rate of appropriation is unconstrained. The appropriation rate imposed by the elite is also based on coercion rather than violence, but is self-constrained—and, hence, referred to as a tax rate.

Coercive appropriation could, of course, turn into open violence. However, the real source of open violence and instability in natural states is violent competition between violence specialists, that is between the elite coalition and warlords.[2]

A direct implication of our discussion of violence specialists as capable of organizing large-scale violence is that entry and exit of violence specialists is exceptional. Depending on the specific setting, membership of the class of violence specialists is by inheritance, as with hereditary membership of a noble caste or the feudal nobility, or by a combination of wealth, inheritance, standing, and social networks. This is exactly one of the characteristics of natural states, where social mobility is highly restricted, in contrast to more open societies, including many present-day, Western ones (North et al. 2009). Sub-Saharan Africa is a case in point, as the chiefs and tribal leaders derive their vital positions in society from customary or hereditary sources, as most can claim a descent from elite lineages (Michalopoulos and Papaioannou 2013; Herbst 2000; Baldwin 2016). Entry is therefore very exceptional, while exit is possible, but in principle not attractive in view of the time and means invested in acquiring this position and the possible gains it brings. As such, our model is closer to real-world situations and departs from models where agents can switch between production and (offensive or defensive) violence activities (cf. Hirshleifer 1995; Grossman and Kim 1995; Konrad and Skaperdas 2012). In our model, producing agents have no means to engage in organized large-scale violence, and, simultaneously, they have no means to defend against such violence

A key feature of the model we present is the interaction between the behavior of violence specialists and production. Production is the domain of a separate class of actors in our model; the producers. Producers have no capacity for large-scale violence and, thus, are completely vulnerable to appropriation by both types of violence specialists. Producers do respond to appropriation by limiting their investment in production—which, in the model implies that they restrict 'effort', the sole input in the production function.[3] Contrary to the above mentioned literature on elite behavior in natural states, the appropriation rate faced by producers is the aggregate rate of appropriation—the combination of the tax rate of the elite coalition and the pure appropriation by warlords—determined by a group of individually optimizing violence specialists. That is, violence specialists compete over a single pool of production (as in

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[2] In our model we do not explicitly separate violence and coercion and take their effect on production to be similar. Of course, violence would lead to outright destruction and, thus, decrease production further than through producer incentives alone. However, including this effect simply decreases total production while not changing our results qualitatively.

[3] 'Effort' is a stylized term to capture several mechanisms through which appropriation leads to reduced production.

Skaperdas 2002) where the behavior of each individual violence specialist affects the payoff to other violence specialists. This is in sharp contrast to the theoretical work closest to ours by Konrad and Skaperdas (2012) who model competition among elites that have a private resource pool and emphasize the welfare losses associated with 'warlord states' compared to states of self-organized producers.

In a nutshell our model is as follows. Violence specialists either join the elite coalition or become a warlord. Depending on the relative size of each group, the elite and warlords each control a share of the total population, and production. Both elite members and warlords use their violence capacity to appropriate production, but they do so in distinct ways. The elite resembles a 'stationary bandit', by enforcing a tax on production to support their coalition, taking into account—in the spirit of McGuire and Olson (1996)—that a high tax rate deters production. This tax rate should be interpreted broadly as encompassing all possible forms of rent extraction, and the optimal level of rent extraction may be very high, for instance when the marginal effect of taxation on production is low. Warlords do not levy taxes but instead they exploit their violence capacity to appropriate as much production as they can. This advantage of warlords over elite members in terms of rent extraction is partly mitigated by cooperation of the elite in the coalition, based on the mutual agreement to respect each others' privileges and collectively defend these against warlords. In response to the expected level of appropriation, producers—modeled as one representative producer—decide how much effort to devote to production. The key outcomes of the model are the tax rate set by the elite, the ratio of elite members to warlords, and the production decision of the representative producer.

The model provides four main insights. First, the tax rate imposed by the elite depends on the output elasticity of effort. That is, the elite limits tax rates when output elasticity is high. This in turn, increases the tendency of violence specialists to become warlord, at least partially offsetting the effect of limited taxation on the aggregate rate of appropriation. Consequently, natural states generally face high rates of appropriation either by a strong elite coalition or, when a weak elite is willing to limit its appropriation, by a relatively high number of warlords. This result corroborates studies and theories highlighting the rigidity of natural states. Second, comparatively high levels of production and producer welfare occur only when output elasticity is high—inducing lower tax rates by the elite—in combination with strong cooperative quality of the elite and low decisiveness of conflict between the elite and warlords—allowing the small elite to control a relatively large share of total production. Only then will producers benefit from the lower tax rates set by the elite. Third, we provide intuition on some of the internal mechanisms of the elite coalition. This we relate to the

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organizational and institutional development of the elite coalition, following North et al. (2009) who describe a maturation process of elite coalitions.[4] The cooperative nature of controlling production and taxing the production under control could lead to conflict over distribution within the coalition, which we address in a first extension of the model. Furthermore, the collective nature of the coalition triggers questions over the optimal size of the coalition and possibly restrictions on the number of elite members, which we discuss in a second extension. The fourth main insight we provide, albeit more tentatively, is that our results suggest an 'institutions-induced resource curse', where low cooperative quality, and the context of the natural state more generally, can be equated with weak institutions, and high output elasticity of effort to the abundance of natural resources. In this interpretation, better institutions within the elite coalition (higher cooperative quality of the elite) allow society to benefit from natural resources (higher elasticity of output). However, the third necessary condition required for relatively high levels of production in our model is that the decisiveness of conflict remains low, which is difficult to reconcile with the point-based nature of natural resources as found in many regions of sub-Saharan Africa (Bulte et al. 2005; Mehlum et al. 2006).

4.2   Model

Consider a natural state with a population of fixed size. There are two subsets of individuals in this society. The first comprises producers, whose production decisions are aggregated in Section 4.2.3 into a single decision by a representative producer. The second comprises violence specialists, denoted by the set V. Each violence specialist is either an elite member or a warlord, denoted by the sets E and V, respectively. We have $E \subset V$ and $W = V \setminus E$. In our model there is no mobility between violence specialists and producers, with the exception of a model extension presented in Section 4.4.3. The two sets E and V are mutually exclusive and collectively exhaustive with respect to V, but we explicitly allow for mobility between them. Members of each set are homogeneous in all relevant aspects. We will often refer to elites and warlords by their number: $e = |E|$ and $w = |W|$.

Violence specialists $i \in V$ can appropriate production from the producers in two different ways, and this appropriation decision is determined by their choice of occupation. Elite members cooperate and appropriate by levying a jointly determined tax on their controlled production, while warlords appropriate by stealing all of their

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[4] Following North et al. (2009) in terms of the maturity of natural states, our model may apply more closely to fragile and basic natural states than to mature natural states.

controlled production. This model feature is a stylized representation of the warlords' uncertain power base which makes them myopic in their decision on how much to appropriate.[5] The two occupations exert negative externalities, because the amount of production appropriated by warlords decreases the production available for appropriation by elite members, and vice versa. As a result, elite members and warlords compete over the share of total production either side controls. From the side of the elite members, this can be interpreted as either (i) the share of society whereon they effectively impose order, or (ii) the extent to which they succeed in establishing order over the entire population.

Our main simplifying assumption is that we consider violence specialists as individuals, and their capacities as homogeneous. Recall that we consider violence specialists as having the skills to generate and maintain a patronage network of followers. Our assumption of violence specialists as homogeneous individuals has three main implications. First, homogeneity removes any reason to discriminate between violence specialists of the same occupation. As a result, payoffs are equal across elite members as well as across warlords. Second, we can abstract from the specificities of the formation and size of patronage networks. Third, we need not explicitly model entry and exit into the elite coalition although we discuss this in more detail in Section 4.4.2. In reality, as a result of competition over the tax rents within the coalition, the composition of the coalition may be continuously changing, as power relations between members change, and because members with negligible contributions are weeded out and, potentially, substituted for new members.

Given this simplification, our model allows us to focus on the violence specialists' choice whether or not to join the elite coalition, and its implications for production levels and producer welfare. This choice depends on the relative profitability of each occupation. Violence specialists can switch occupations without cost and do so until payoffs are equalized.[6] As a result, each equilibrium features a specific distribution of elite members and warlords. The occupation choice by violence specialists is the first stage of a three-stage model. The stages are shortly described below and worked out in detail in Section 4.3. In addition, In Section 4.4 we present extensions of the model with an additional stage that features conflict within the coalition, a discussion of the propensity of elite members to limit the size of the coalition, and an extension on the endogenous size of the group of violence specialists.

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[5] In addition, since warlords operate alone – and given a sufficiently large number of violence specialists – individual appropriation rates have only negligible impact on production so that myopic behaviour is not an assumption but an implication from the model introduced below.

[6] The model guarantees internal solutions featuring equilibria with strictly positive amounts of warlords and elite members, as we will demonstrate below.

4.2.1 Occupation choice (Stage 1)

Each violence specialist $i \in V$ decides to join the elite coalition or not. We denote this occupation choice by $\mu_i \in \{1, 0\} \; \forall i \in V$. If $\mu_i = 1$, the specialist joins the elite coalition. If $\mu_i = 0$, the specialist becomes a warlord. The outcome of these decisions is a vector $\mu = (\mu_i : i \in V)$ that partitions the violence specialists in two subsets: the elite coalition $E = \{i : \mu_i = 1\}$ and its complement $W = V \setminus E = \{i : \mu_i = 0\}$ comprising all warlords.

Control over producers by elite members and warlords is given deterministically by the ratio $\frac{e}{w}$ (see (4.17) below). We use the following function to determine the share of total production that is controlled by elites:

$$\rho(e, w) = \frac{\theta e^m}{\theta e^m + w^m}, \tag{4.1}$$

with $m \in (0, 1)$ and $\theta \in [1, \infty)$. It follows that the share of total production that is controlled by warlords equals $1 - \rho(e, w)$.

A few comments on (4.1) are appropriate here.

First, its functional form is borrowed from the rent seeking literature (Tullock 1980). Our specification of $\rho(e, w)$ is based on a modification of the ratio-form contest success function (CSF), inspired by the axiomatic characterization of group CSFs by (Münster 2009). Parameter m is conventionally interpreted as the decisiveness of conflict, and here we interpret it as the decisiveness of group size, where group refers to either the elite coalition or the aggregate of warlords. Given $m < 1$, there are diminishing marginal returns to group formation. Specifically, low m implies that a small elite coalition (group of warlords) is capable of controlling a relatively large share of production. Parameter θ represents a fighting asymmetry (cf. Usher 1989; Clark and Riis 1998) in favour of the elite coalition that we consider to be better organized than warlords, because of their commitment to respect each others' privileges. In the context of North et al. (2009) one could interpret θ as the cooperative quality, or maturity, of the coalition, with more mature coalitions capable of organizing and coordinating power more efficiently.[7] One implication of this functional form is that the elite may control a larger share of production, even if it is smaller in size than the aggregate of warlords.

Second, note that our interpretation of $\rho(e, w)$ is non-probabilistic in the sense that it represents a share, rather than a winning probability (although the two interpretations are equivalent under the assumption of risk neutrality). For a detailed discussion

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[7] See Van Bavel, Ansink and Van Besouw (2017)—see Chapter 5—who provide a historical exploration of the role of organizations on θ.

of CSFs and their interpretations, see Hirshleifer (2000) Garfinkel and Skaperdas (2007), and Konrad (2009).

Third, although we talk freely about the share of production that is controlled by warlords, the degree of cooperation by warlords has not been specified yet. In the functional form chosen in (4.1), warlords do work together, but have a fighting disadvantage compared with the elite, through θ. The alternative approach is to model warlords as operating alone, using the term $w \times 1^m$ rather than w^m, which would imply

$$\rho'(e, w) = \frac{\theta e^m}{\theta e^m + w \times 1^m} = \frac{\theta e^m}{\theta e^m + w}. \tag{4.2}$$

This alternative specification, however, has two disadvantages: (i) It would give the coalition a fighting disadvantage for any $m < 1$, so that the effects of m and θ may cancel each other out. This disadvantage is reversed for $m > 1$. In our model set-up, however, if θ is sufficiently large, then $m > 1$ leads to a corner solution where all specialists end up in the elite coalition.[8] This is a standard feature of the ratio-form CSF, discussed in detail by Hirshleifer (1995) and employed by Skaperdas (1998) to assess coalition formation in a different setting. (ii) Using $\rho'(e, w)$ to calculate the equilibrium ratio of elite members to warlords, as we do for $\rho(e, w)$ in (4.14) below, would lead to asymptotic behaviour of this ratio, including discontinuities and negative outcomes. Both features are undesirable and we stick to (4.1).

Fourth and final, our specification of $\rho(e, w)$ deviates from the standard approach in the economic literature on conflict and appropriation. Most importantly, this contest is deterministic in the sense that the outcome of the contest depends only on the ratio $\frac{e}{w}$. Notably, it does not depend on costly investments in violence capacity. That is, in the Stage 2 contest, elite members and warlords do not explicitly choose their violence level as is conventional in models that feature a trade-off between own production and appropriation (e.g. Hirshleifer 1988; 1995; Skaperdas 1992; Grossman and Kim 1995) or in rent-seeking models (Nitzan 1994). We focus, however, on organized large-scale violence and, in our model, the capacity for such violence is restricted to violence specialists, while production is the domain of the separate subset of producers. As a result, violence specialists are not confronted with this trade-off between own production and appropriation. In addition, motivated by the homogeneity of violence specialists, the only effect of adding costly investments in violence would be that payoffs of violence specialists would be reduced in the symmetric outcome of such a model, without any qualitative impacts on model results.

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[8] Specifically, a corner solution results if $(1 - \alpha)\theta \geq 1$, where parameter α is the output elasticity to effort introduced in Section 4.2.3).

4.2.2 Tax (Stage 2)

Given the outcome of Stage 1, the elite controls a share $\rho(e, w)$ and warlords jointly control a share $1 - \rho(e, w)$, which each of them can appropriate as they wish. Following the main features of the natural state as discussed above, elite members collectively determine their tax rate $\tau \in [0, 1]$, while warlords, by construction, choose to appropriate all production under their control.

4.2.3 Production (Stage 3)

In modelling production, we choose to aggregate production decisions by all producers into a single decision by a representative producer, although the same functional form would be obtained by assuming a set of homogeneous producers, each working a plot of land with symmetric concavity in effort. Given the outcome of Stages 1–2, the representative producer chooses its joint production level. We model production Y as a single-input production function.

$$Y(\phi) = \beta \phi^\alpha. \tag{4.3}$$

Parameter $\alpha \in (0, 1)$ denotes the output elasticity of effort. Parameter $\beta \in (0, \infty)$ reflects total factor productivity, which in the presence of only one factor of production, we will refer to as a technology parameter.

Appropriation by elite members and warlords reduces the amount of produce available for consumption. Producers maximize utility U which equals aggregate consumption – production net of appropriation – minus the cost of effort.

$$U = (1 - \tau)\rho(e, w)Y(\phi) - \gamma\phi, \tag{4.4}$$

with cost parameter $\gamma \in (0, \infty)$.

§

Given our main assumption that violence specialists are homogeneous, payoffs π_i are equal across warlords as well as across elite members. Incorporating all decisions made in Stages 2–4, this implies the following payoff functions to violence specialists:

$$\pi_i = \left(\frac{1}{e}\right)\tau\rho(e, w)Y(\phi) \qquad \forall i \in E; \tag{4.5}$$

$$\pi_j = \left(\frac{1}{w}\right)(1 - \rho(e, w))Y(\phi) \quad \forall j \in W. \tag{4.6}$$

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The stability concept that we use to evaluate outcomes of the model is a simple equilibrating mechanism that equates payoffs to elite members and warlords. That is, in Stage 1 violence specialists choose the most profitable occupation. They make this choice whilst taking into account (i) the optimal tax rate that will be chosen by the elite, and (ii) the optimal response in terms of production by the representative producer. Hence, in equilibrium, payoffs to elite members and warlords are equal. If not, then a profitable switch of occupation could be made by at least one violence specialist, while taking into account that changing occupations shifts the balance of power between warlords and the elite coalition with subsequent impacts on the outcomes of Stages 2–3. This equilibrating mechanism is reminiscent of more advanced stability concepts applied in alliance models and non-cooperative models of coalition formation (cf. Skaperdas 1998; Yi 2003; Garfinkel 2004).

4.3 Results

In this section we present the results of our model. Solving the model backwards, we analyze each of the four stages consecutively.

4.3.1 Production (Stage 3)

Given outcomes of Stages 1–2, the representative producer chooses ϕ to maximize utility (4.4):

$$\frac{\partial U}{\partial \phi} = (1 - \tau)\rho(e, w)\frac{\partial Y(\phi)}{\partial \phi} - \gamma = 0. \tag{4.7}$$

By the production function in (4.3) we have

$$\frac{\partial Y(\phi)}{\partial \phi} = \alpha \beta \phi^{\alpha - 1}. \tag{4.8}$$

Substituting this derivative into (4.7) and solving for ϕ, we obtain:

$$\phi^* = \left((1 - \tau)\rho(e, w)\frac{\alpha \beta}{\gamma} \right)^{\frac{1}{1 - \alpha}}. \tag{4.9}$$

Substituting this equilibrium level of effort into (4.3) and solving for Y, we obtain:

$$Y^* = \beta \left((1 - \tau)\rho(e, w)\frac{\alpha \beta}{\gamma} \right)^{\frac{\alpha}{1 - \alpha}}. \tag{4.10}$$

We will further assess Y^* in Section 4.3.4 below.

4.3.2 Tax (Stage 2)

Given the outcome of Stage 1, the elite coalition chooses the tax rate τ. Since violence specialists are homogeneous, there is no difference between choosing a tax rate that maximizes individual payoffs or one that maximizes the payoff to the coalition as a whole. Substituting (4.10) for Y, the coalition payoff π_E equals e times individual elite payoff (4.5):

$$
\begin{aligned}
\pi_E &= e\left(\frac{1}{e}\right)\tau\rho(e,w)Y(\phi) \\
&= \tau\rho(e,w)\beta\left((1-\tau)\rho(e,w)\frac{\alpha\beta}{\gamma}\right)^{\frac{\alpha}{1-\alpha}}.
\end{aligned}
\tag{4.11}
$$

We can now maximize and solve for τ to find:

$$
\tau^* = 1 - \alpha.
\tag{4.12}
$$

Our first result follows directly.

Proposition 1. *In equilibrium, the optimal tax rate τ^* decreases linearly in α, and is independent of β, γ, θ, and m.*

Given $\alpha < 1$, the equilibrium tax rate is strictly smaller than 1. The elite coalition abstains from fully taxing away its controlled production. When α is low the effect of the tax rate on production is small and hence, it is optimal to set a high tax rate. The opposite holds when α is high.

4.3.3 Occupation choice (Stage 1)

Each violence specialist chooses his occupation $\mu \in \{0, 1\}$ to maximize his payoff as given by (4.5) and (4.6), taking into account the effects of occupation choice on payoffs via (4.1) on the Stage 2 tax rate and Stage 3 production. Recall our equilibrating mechanism of equal payoffs to both occupations such that $\pi_i = \pi$ for all $i \in V$. Applying this mechanism, we equate (4.5) and (4.6) to find the equilibrium ratio of elite members to warlords, which is independent of production:

$$
\frac{e}{w} = \frac{\tau\rho(e,w)}{1-\rho(e,w)}.
\tag{4.13}
$$

By the specification of $\rho(e, w)$ in (4.1), the size of the elite-controlled production depends positively on the ratio of elites to warlords. Since our economy has a population of fixed size and does not allow mobility between violence specialists and producers, the number of violence specialists is also fixed at $e + w$: an increase in e implies a decrease in w of equal size and vice versa. We use this model feature and also substitute (4.1) for $\rho(e, w)$ in the equilibrium ratio (4.13). After simplification and substitution of (4.12) for τ we obtain the equilibrium elite-warlord ratio as a function of parameters α, m, and θ:

$$\frac{e^*}{w^*} = \big((1-\alpha)\theta\big)^{\frac{1}{1-m}}. \tag{4.14}$$

By substituting $w^* = |V| - e^*$, we also obtain e^* and w^* separately:

$$e^* = \left(\frac{\big((1-\alpha)\theta\big)^{\frac{1}{1-m}}}{\big((1-\alpha)\theta\big)^{\frac{1}{1-m}} + 1} \right) |V|; \tag{4.15}$$

$$w^* = \left(\frac{1}{\big((1-\alpha)\theta\big)^{\frac{1}{1-m}} + 1} \right) |V|. \tag{4.16}$$

The elite-warlord ratio (4.14) increases with the tax rate. It also increases with m when $(1-\alpha)\theta > 1$ which implies $\frac{e^*}{w^*} > 1$. In that case, the elite has an advantage in generating rents as a combined effect of controlling and taxing production. Given $m < 1$, profit per occupation, in both occupations, decreases with the size of the group. Therefore, equilibrium group size can be interpreted as the relative profitability of an occupation, with the larger group having an advantage in generating rents.

For the limit case where $\theta = 1$, and since $\alpha \in (0, 1)$, the elite-warlord ratio is strictly smaller than 1, approaching unity only in the limit where the tax rate (i.e. $1-\alpha$) approaches 1. Put differently, in absence of an elite fighting advantage, the number of elite members is never larger than the number of warlords. This cap on elite size follows from our model feature that warlords fully appropriate their controlled production, whereas elite members do not necessarily. Hence, elite members, by construction, have a disadvantage in terms of their capacity to generate rents, which can be offset only by their fighting advantage in case $\theta > 1$.

In Figure 4.1 we plot (4.15), the equilibrium number of elite members e^*, and (4.14), the equilibrium elite-warlord ratio, as a function of $(1 - \alpha)\theta$ for different values of parameter m. This figure illustrates that the presence of a sizeable elite coalition in a natural state is not trivial. An (almost) empty elite coalition is possible for low values of $(1 - \alpha)\theta$. In contrast, an elite coalition that contains (nearly) all
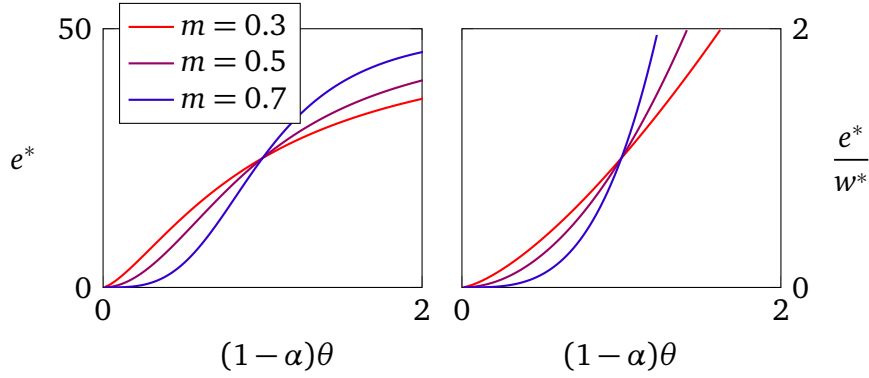
Figure 4.1: Equilibrium size of the elite coalition e^* and equilibrium elite-warlord ratio $\frac{e^*}{w^*}$ as a function of $(1-\alpha)\theta$ for $|V| = 50$, and different values of parameter m.

violence specialists is possible for high values of $(1-\alpha)\theta$ and high m. Note that, by the exponent $\frac{1}{1-m}$ in (4.15) and (4.16), these are limit results for $\theta\tau$ going to zero or infinity. Both e^* and w^* converge to, but will never reach, 0 or $|V|$, ruling out any corner solutions.

From (4.14) follows our next result.

Proposition 2. *In equilibrium, the elite-warlord ratio $\frac{e^*}{w^*}$ is:*
   (i) decreasing with output elasticity α;
  (ii) independent of the technology-cost ratio $\frac{\beta}{\gamma}$;
 (iii) increasing with elite fighting advantage θ;
 (iv) decreasing with the decisiveness of group size m if and only if $(1-\alpha)\theta < 1$.

Proof. The results follow directly from the relevant first order conditions to (4.14). □

4.3.4 Producer welfare (Back to Stage 3)

The negative relation between output elasticity α and the equilibrium elite-warlord ratio naturally results in the question whether and, if so, under what conditions the representative producer benefits from the presence of a large elite coalition. Our results show three countervailing effects with respect to α. First, there is a direct positive effect of α on production (see (4.8)). Second, there is an indirect positive effect of α on production via the tax rate (see Proposition 1). Third, there is an indirect negative effect of α on production via the elite-warlord ratio (see Proposition 2). We assess this combination of effects on production by evaluating the equilibrium production level through substitution of the equilibrium elite-warlord ratio and the equilibrium tax rate. From this equilibrium production level, we can then proceed to evaluate producer utility (4.4), our measure of producer welfare.

We first rewrite the CSF in (4.1) in terms of the elite-warlord ratio by multiplying both RHS fraction terms with $(\theta e^m)^{-1}$:

$$\rho(e,w) = \left(\frac{1}{1 + \left(\frac{1}{\theta\left(\frac{e}{w}\right)^m} \right)} \right). \tag{4.17}$$

We then proceed to substitute (4.14) for $\frac{e}{w}$ in order to obtain $\rho(e^*, w^*)$ in equilibrium:

$$\rho(e^*, w^*) = \left(\frac{1}{1 + \left(\frac{(1-\alpha)}{((1-\alpha)\theta)^{\frac{1}{1-m}}} \right)} \right). \tag{4.18}$$

Next, we substitute (4.12) for τ and (4.18) for $\rho(e,w)$ in the equilibrium production level (4.10), to obtain equilibrium production as a function of exogenous parameters only:

$$Y^* = \beta \left(\frac{\alpha^2\beta}{\gamma} \right)^{\frac{\alpha}{1-\alpha}} \left(\frac{1}{1 + \left(\frac{(1-\alpha)}{((1-\alpha)\theta)^{\frac{1}{1-m}}} \right)} \right)^{\frac{\alpha}{1-\alpha}}. \tag{4.19}$$

From (4.19) follows our next result.

Proposition 3. *In equilibrium, production Y^* is:*
   (i) *increasing with the technology-cost ratio $\frac{\beta}{\gamma}$;*
   (ii) *increasing with elite fighting advantage θ;*
   (iii) *decreasing with the decisiveness of group size m if and only if $(1-\alpha)\theta < 1$.*

Proof. The results follow directly from the relevant first order conditions to (4.19). $\square$

Note that Proposition 3 does not cover the impact of α on optimal production. The relevant first order condition to (4.19) can be solved analytically using logarithmic differentiation, which yields the following expression:

$$\frac{\partial Y^*}{\partial \alpha} = \beta \left(\frac{\alpha^2\beta}{\gamma\left(1 + \frac{(1-\alpha)}{((1-\alpha)\theta)^{\frac{1}{1-m}}}\right)} \right)^{\frac{\alpha}{1-\alpha}}$$
$$\times \frac{(1-\alpha)\left(2 - \frac{\alpha m}{(1-m)(2-\alpha)((1-\alpha)\theta)^{\frac{1}{1-m}}}\right) + \ln[\alpha^2\beta] - \ln\left[\gamma\left(1 + \frac{(1-\alpha)}{((1-\alpha)\theta)^{\frac{1}{1-m}}}\right)\right]}{(1-\alpha)^2}. \tag{4.20}$$

Clearly, this comparative static is ambiguous in sign and depends crucially on parameter values. Therefore, we evaluate (4.19) numerically for a wide range of values for β, γ, θ, and m below. First, however, a few observations on (4.20) can be made. The first term on the right-hand side—which is simply equilibrium output—is strictly positive and has a wide range that depends mostly on the technology-cost ratio $\frac{\beta}{\gamma}$.[9] The second term on the right-hand side of (4.20) features three additive elements, none of which has an unambiguous sign. The first logarithmic term is positive for sufficiently high β, while the second logarithmic term is positive—thus entering negatively in the total fraction—for sufficiently high γ. For high values of α and m, however, the first term turns negative and dominates the logarithmic terms.

To sum up, the comparative static effect of α on Y^* is small when the technology-cost ratio is low, but can increase substantially with α. However, it turns negative for high values of α. Parameters m and θ limit the magnitude of the comparative static effect and m increases the likelihood of a negative sign as well. This suggest a hump-shaped effect of α that becomes more pronounced for low m and θ and a high technology-cost ratio.

The numerical simulation reveals that, for most parameter combinations, production is not very sensitive to α; the countervailing effects identified in the beginning of Section 4.3.4 cancel each other out. There is one exception that confirms our earlier observations. When β is sufficiently large relative to γ and m is sufficiently small, production peaks for relatively high values of α. Example plots for different values of β and m are provided in Figure 4.2. The increase in production for sufficiently high α and β is largely driven by a peak in optimal effort (4.9), which is subsequently offset (for even higher α) by the effect of increasing appropriation. Figure 4.2 also illustrates that for low values of α, production may be decreasing in α. This is the case when the optimal level of effort chosen by the representative producer is below 1.

Substituting (4.12) for τ, (4.18) for $\rho(e,w)$, (4.19) for $Y(\phi)$ and (4.9) for ϕ in the representative producer's utility function (4.4), we obtain, after substantial simplification:

$$
U^* = \left((1-\alpha)\frac{\gamma}{\alpha} \right) \left(\frac{\alpha^2\beta}{\gamma} \right)^{\frac{1}{1-\alpha}} \left(\frac{1}{1 + \left(\frac{(1-\alpha)}{((1-\alpha)\theta)^{\frac{1}{1-m}}} \right)} \right)^{\frac{1}{1-\alpha}} , \tag{4.21}
$$

---

[9] To see this, note that the exponent tends to infinity for high values of α, whereas the base of the exponent is smaller than 1 for many parameter values. The base is a fraction where the numerator has $\beta \in (0, \infty)$ multiplied by $\alpha^2 \in (0,1)$, whereas the denominator has $\gamma \in (0, \infty)$ multiplied by a term that is strictly larger than one, although bounded by parameters θ and m.
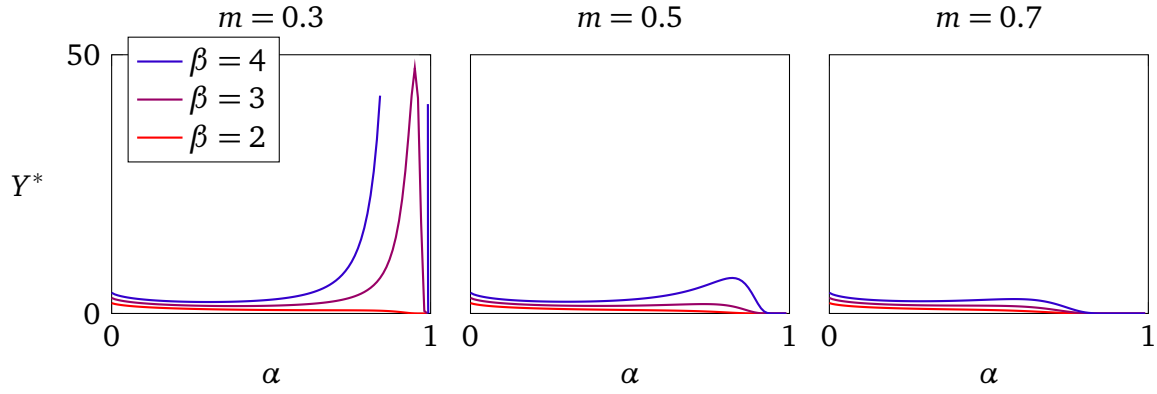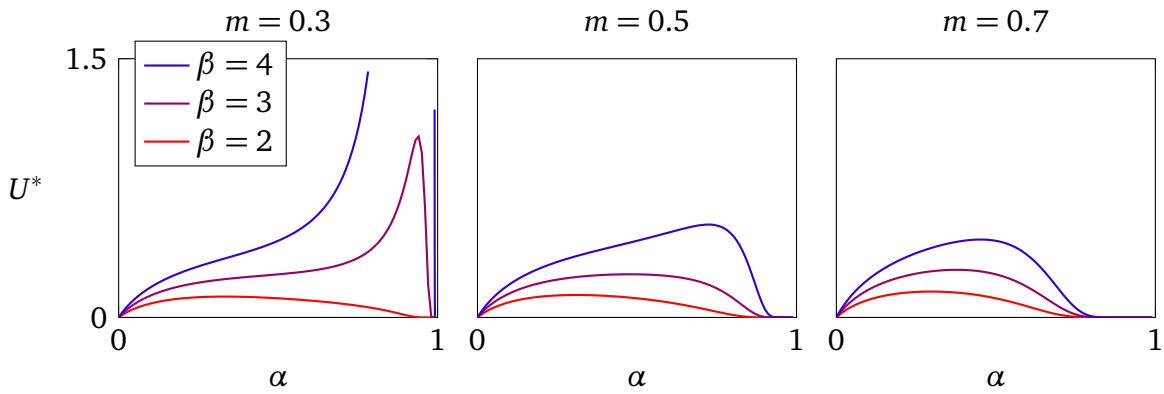
Figure 4.2: Equilibrium production Y^* as a function of α; example plots for $\gamma = 1$, $\theta = 2$, and different values of parameters β and m.

which is strictly positive under our parameter assumptions.

Based on (4.21), the effects of model parameters on producer welfare are summarized in our next result.

Proposition 4. *In equilibrium, producer utility U^* is:*

(i) *increasing with the technology-cost ratio $\frac{\beta}{\gamma}$;*

(ii) *increasing with elite fighting advantage θ;*

(iii) *decreasing with the decisiveness of group size m if and only if $(1-\alpha)\theta < 1$;*

(iv) *hump-shaped in the output elasticity α.*

Proof. The results for parts (i)–(iii) follow directly from the relevant first order conditions to (4.21). For part (iv), we derive the relevant first order condition to (4.21) using logarithmic differentiation, which yields the following expression:

$$
\frac{\partial U^*}{\partial \alpha} = \left((1-\alpha)\frac{\gamma}{\alpha}\right)\left(\frac{\alpha^2\beta}{\gamma}\right)^{\frac{1}{1-\alpha}}\left(\frac{1}{1+\left(\frac{(1-\alpha)}{((1-\alpha)\theta)^{\frac{1}{1-m}}}\right)}\right)^{\frac{1}{1-\alpha}}
$$
$$
\times \frac{1+((1-\alpha)\theta)^{\frac{1}{1-m}}+\frac{\alpha}{(m-1)\theta}}{\alpha(1-\alpha)\left(((1-\alpha)\theta)^{\frac{1}{1-m}}+(1-\alpha)\right)}. \tag{4.22}
$$

All terms in this expression are positive, except for $\alpha/(m-1)\theta$ in the numerator. As a result, negative values can only be obtained for high α (relative to θ), which implies a hump-shaped curve. $\square$

To illustrate the hump-shaped relation between α and U^*, we evaluated (4.21) numerically for a wide range of values for β, γ, θ, and m. Example plots for different values of β and m are provided in Figure 4.3, which is directly comparable to Figure 4.2.

Figure 4.3: Equilibrium producer utility U^* as a function of α; example plots for $\gamma = 1$, $\theta = 2$, and different values of parameters β and m.

This figure illustrates the combination of direct and indirect effects of α on utility – as discussed in the beginning of this section. Utility is low for both low α and, perhaps surprisingly, high α. Maximal welfare levels are reached for intermediate values of α. The explanation for this shape is largely found in the level of the appropriation rate and the return to investments in effort. The appropriation rate equals the sum of taxation by elite and full rent extraction by warlords. It can be expressed as $\rho(e,w)(1-\alpha) + (1 - \rho(e,w)) = 1 - \alpha\rho(e,w)$. From this expression follows immediately that the appropriation rate tends to 1 for low α. Also, by (4.18) we know that $\rho(e^*,w^*)$ is decreasing in α, which implies increasing rent extraction by warlords. Jointly, these effects make that the appropriation rate follows a U-shape with minimal appropriation for intermediate levels of α.

All in all, producers are best off with intermediate levels of α, where their production peaks through substantial investments in effort and the level of appropriation is still relatively low. Combined with low m and substantial θ, the elite coalition is not very large (see Figure 4.1), but is able to control the bulk of production. Lower levels of α would decrease the return to investments in effort — e.g. higher tax and lower elasticity of output, while higher levels of α would lead to increased appropriation by warlords. Both effects are detrimental to producer utility.

4.4 Extensions

In this Section we consider three extension to our model. So far, we have assumed a frictionless elite coalition which stands in sharp contrast to some of the real world examples that were referred to in the introduction. Two main sources of friction within the elite coalition are the distribution of the collectively generated tax rent, and possible barriers to entry for new members. These are just two possible examples of frictions

within the elite coalition, but they are important ones and discussed in the first two extensions. In Section 4.4.3 we endogenize the size of the group of violence specialists, dropping our assumption that violence specialists cannot switch to become producers – while maintaining that mobility in the opposite direction is restricted in natural states.

4.4.1 Within-coalition conflict

In the natural state, the elite emerges out of the pool of violence specialists, and cooperation of violence specialists in the elite coalition is not self-evident. Given the outcome of Stages 1–3, members of the elite coalition may engage in conflict over the tax rent. There are various ways to model such conflict and one could even argue that an appropriately designed sharing rule or voting procedure could eliminate the incentive for rent-seeking within the coalition. Indeed, the mere possibility of within-coalition conflict can be interpreted as characterizing an earlier state of development than the setting of our base model, where within-coalition conflict is absent by construction. One could, in the spirit of North et al. (2009), argue that the absence of conflict within the coalition is a result of a more developed 'rule-of-law' among the elite.[10]

We proceed with a simple conflict model that we include as stage 4 of our model. This simple set-up is sufficient to demonstrate the impact of the prospect of conflict on elite size. We do so using a ratio-form CSF, similar to (4.1). As with (4.1), the CSF can be interpreted as probabilistic or, assuming risk-neutrality, as defining shares to each elite member. Here we stick to the non-probabilistic interpretation. Each elite member receives a share $\sigma_i(\mathbf{s})$ of the tax rent, which depends on costly investments in conflict by all members, captured in the vector $\mathbf{s} = (s_i : i \in E)$:

$$\sigma_i(\mathbf{s}) = \frac{s_i^n}{\sum_{j \in E} s_j^n}, \tag{4.23}$$

with $n \in (0,1)$ being the decisiveness parameter for within-coalition conflict.

We update the payoff function 4.5 to elites:

$$\pi_i = \sigma_i(\mathbf{s}) \tau \rho(e, w) Y(\phi) - s_i \quad \forall i \in E. \tag{4.24}$$

Now, in Stage 1 violence specialists choose the most profitable occupation whilst taking into account not only the optimal tax rate and production, but also the severity of conflict within the coalition. Note that the combination of Stage 1 and Stage 4 resembles models of sequential inter- and intra-group resource contest (Wärneryd

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[10]We thank a reviewer for making this observation.

1998; Esteban and Sákovics 2003; Garfinkel 2004; Inderst, Müller and Wärneryd 2007). We add to this the interaction between violence specialists and producers.

Given outcomes of Stages 1–3, each elite member chooses s_i to maximize his payoff as given by (4.24):

$$\frac{\partial \pi_i}{\partial s_i} = \frac{\partial \sigma_i(\mathbf{s})}{\partial s_i} \tau \rho(e,w) Y(\phi) - 1 = 0 \quad \forall i \in E. \tag{4.25}$$

Note that we exclude the peaceful outcome where $s_i = 0$ for each agent. Such a peaceful outcome cannot be an equilibrium to the conflict since one elite member j could secure the complete resource with a small investment in conflict $s_j > 0$ (Garfinkel and Skaperdas 2007). This opportunity would not be left unexploited in equilibrium, which is why we exclude it from our analysis.

By (4.23) we have:

$$\frac{\partial \sigma_i(\mathbf{s})}{\partial s_i} = \frac{n s_i^{n-1} \sum_{j \in E \setminus \{i\}} s_j^n}{\left(\sum_{j \in E} s_j^n\right)^2}. \tag{4.26}$$

With homogeneous violence specialists, such that $s_i = s$ for each $i \in E$, we can simplify this derivative to

$$\frac{\partial \sigma_i(\mathbf{s})}{\partial s_i} = \frac{n(e-1)}{se^2}. \tag{4.27}$$

Substituting this simplified derivative into (4.25) and solving for s, we obtain:

$$s = \left(\frac{e-1}{e^2}\right) n \tau \rho(e,w) Y(\phi). \tag{4.28}$$

Note that we cannot proceed by substituting our previously found equilibrium values, since these may be affected by the prospect of within-coalition conflict on Stage 1–3 decisions. Specifically, this prospect affects the elite-warlord ratio. It does not affect our result on the optimal tax rate and it also does not affect our results on optimal production or producer welfare, except through this ratio. To show the effect of within-coalition conflict on $\frac{e}{w}$, we equate (4.24) with (4.6) and we substitute (4.28) for s_i, to obtain:

$$\left(\frac{1-n+n/e}{e}\right) \tau \rho(e,w) = \left(\frac{1}{w}\right)\left(1 - \rho(e,w)\right). \tag{4.29}$$

We can now rearrange terms to find the equilibrium elite-warlord ratio, which is again

independent from production Y:

$$\frac{e}{w} = \frac{(1-n+n/e)\,\tau\rho(e,w)}{1-\rho(e,w)}. \tag{4.30}$$

We substitute (4.1) for $\rho(e,w)$ and (4.10) for τ in the equilibrium ratio (4.30). After simplification we obtain the following elite-warlord ratio as a function of exogenous parameters and e:

$$\frac{e}{w} = \left((1-n+n/e)(1-\alpha)\theta\right)^{\frac{1}{1-m}}. \tag{4.31}$$

Comparing (4.30) with the related ratio in the standard version of our model (4.14), we see that an additional term $(1-n+n/e)$ has entered the solution. This term is driven by the anticipation of conflict in the fourth stage of the model (recall n is the decisiveness parameter for within-coalition conflict). For $n \to 0$, the effect of Stage 4 within-coalition conflict on Stage 1 occupation choice vanishes; the ratio $\frac{e^*}{w^*}$ converges to the ratio (4.14) of the standard version of our model.

We continue to assess the effect of within-coalition conflict on $\frac{e}{w}$ by substituting $|V| - e$ for w in (4.31) and using implicit differentiation:

$$\frac{d}{dn}e^* = \frac{(e-e^2)(|V|-e)^2}{n(|V|-e)^2 + (1-m)(|V|e^2)(1-n+n/e)^{\frac{m}{m-1}}((1-\alpha)\theta)^{\frac{1}{m-1}}} < 0. \tag{4.32}$$

All terms of (4.32) are positive, with the exception of the term $(e-e^2)$ in the numerator. Hence, $\frac{d}{dn}e^* < 0$. As a result, we find that the elite-warlord ratio $\frac{e^*}{w^*}$ is decreasing with the decisiveness of within-coalition conflict n. To illustrate this result we reproduce Figure 4.1 (which features no within-coalition fighting, hence $n = 0$), for the case where $n = 1$. Values for e^* and $\frac{e^*}{w^*}$ in Figure 4.4 are computed by solving (4.31) numerically for $n = 1$ using the Newton-Raphson method. Comparison of both figures shows that the limit case of the model with $n = 1$ implies a substantially smaller elite coalition in equilibrium. Choosing the elite occupation has become less attractive compared to the standard version of our model, because of the prospect of within-coalition conflict. Obviously, this difference in elite size is mitigated if we allow for the possibility that warlords also engage in a conflict over their appropriative rents. Such warlord conflict would obviously decrease warlord payoffs. The combined effect of warlord conflict and within-coalition conflict on elite size will depend on the relative decisiveness of both conflicts. Note that we will need much of the above derivations to introduce our results in the remainder of this section, which do not depend on the existence or not of
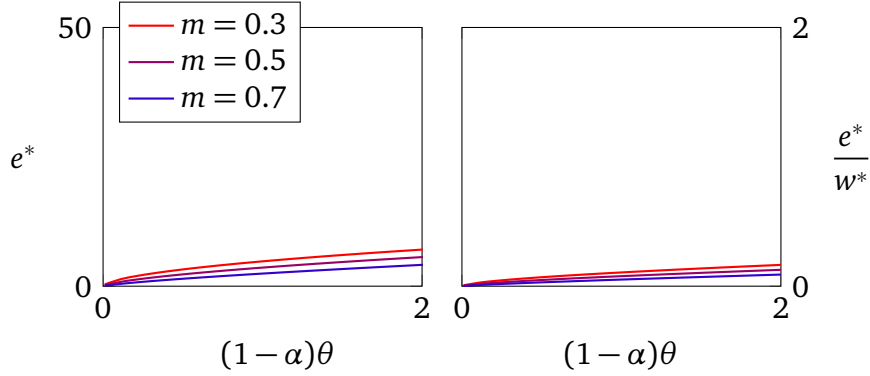
warlord conflict.

Figure 4.4: Equilibrium size of the elite coalition e^* and equilibrium elite-warlord ratio $\frac{e^*}{w^*}$ as a function of $(1-\alpha)\theta$ for $n = 1$, $|V| = 50$, and different values of parameter m.

Our analysis up to here allows us to assess the impact of the number of contestants on investments in conflict. A standard result from rent-seeking models (cf. Garfinkel and Skaperdas 2007) is that an increase in the number of contestants *decreases* individual investments in conflict. The intuition for this result is that in the presence of more competitors the expected return to investments in conflict decreases. In the context of our paper, this result would imply that the effect of an increase of e on s^* is negative. In conflict models with endogenous production, however, this result is reversed (Hirshleifer 1995): an increase in the number of contestants *increases* individual investments in conflict. The intuition is that, as the number of contestants increases, a smaller fraction of own production can be retained and hence investments in conflict become more attractive. Our next proposition shows that in the setting of our paper, where production is the domain of a separate subset of producers and subject to interaction with violence specialists, both results are combined. Specifically, an increase in the size of the elite coalition decreases the aggregate level of appropriation which provides incentives to increase production. Increased production, in turn, provides incentives to increase investments in conflict. This indirect positive effect may offset the direct negative effect of e on s^*.

Proposition 5. *In equilibrium, within-coalition investments in costly conflict s^* are hump-shaped in the size of the elite coalition e with a global maximum $\hat{e} \in \mathbb{R} : \hat{e} = \frac{2-Z(\hat{e})}{1-Z(\hat{e})}$, where $Z(e) = \frac{m}{1-\alpha}\big(1-\rho(e,w)\big)$.*

Proof. Using (4.28), we first derive the first order condition $\frac{\partial s^*}{\partial e}$ and solve for e which yields the implicit function

$$e = \frac{2(1-\alpha)(\theta e^m + w^m) - mw^m}{(1-\alpha)(\theta e^m + w^m) - mw^m} = \frac{2 - Z(e)}{1 - Z(e)}, \tag{4.33}$$

where $Z(e) = \frac{m}{1-\alpha}\big(1 - \rho(e,w)\big)$. The second derivative of (4.28) with respect to e is rather involved, so we confirmed concavity of $\frac{\partial s^*}{\partial e}$ numerically. Given the domains of parameters α and m and the function $\rho(e,w)$, we have that $Z(e) > 0$. By (4.33), $Z(e) > 0$ implies that $\hat{e}$ is unbounded (e.g. consider cases with α and m such that $Z(e)$ is close to unity).

To gain further insights into the effect of e on s^*, notice that e not only affects s^* directly but also through $\rho(e,w)$ and $Y(\phi)$, where ϕ depends on e through $\rho(e,w)$. To evaluate these effects separately we take the total derivative of (4.28) to e (to reduce notational clutter we write $\rho(e,w)$ as ρ and $Y(\phi)$ as Y):

$$\frac{ds^*}{de} = \frac{\partial s^*}{\partial e} + \frac{\partial s^*}{\partial \rho}\frac{d\rho}{de} + \frac{\partial s^*}{\partial Y}\frac{\partial Y}{\partial \rho}\frac{d\rho}{de}. \tag{4.34}$$

We find that these three effects are given by:

$$\frac{\partial s^*}{\partial e} = \left[-\left(\frac{e-2}{e^3}\right)n\tau\rho Y\right]; \tag{4.35}$$

$$\frac{\partial s^*}{\partial \rho}\frac{d\rho}{de} = \left[\left(\frac{e-1}{e^2}\right)n\tau Y\right] \times \left[\rho(1-\rho)\left(\frac{m}{e}\right)\right]; \tag{4.36}$$

$$\frac{\partial s^*}{\partial Y}\frac{\partial Y}{\partial \rho}\frac{d\rho}{de} = \left[\left(\frac{e-1}{e^2}\right)n\tau\rho\right] \times \left[\rho(1-\rho)\left(\frac{m}{e}\right)\right]$$
$$\times \left[\left(\frac{\alpha}{1-\alpha}\right)\frac{\beta}{\rho}\left((1-\tau)\rho\frac{\alpha\beta}{\gamma}\right)^{\frac{\alpha}{1-\alpha}}\right]. \tag{4.37}$$

The direct effect of an increase of e on s^* in (4.35) is negative (as long as $e > 2$), while both indirect effects in (4.36) and (4.37) are positive for $e > 1$. An increase in e increases both the share of production controlled by elite members as well as (indirectly) the production level. Both of these contribute to a higher tax rent, which makes fighting more attractive. Depending on parameter values the positive or negative effect dominates as stated in the proposition. □

4.4.2 Limiting access to the coalition

In this extension, we assess whether elite members have an incentive to limit entry into the coalition in order to avoid dilution of the tax rent. It seems reasonable to assume that access into the coalition should require consent of the coalition members. In the literature on coalition formation in games with externalities it is shown that the rules of coalition formation may impact the coalition size. Specifically, requiring consent on membership—called 'exclusive membership' in this literature—is one such rule. In a negative externality game (as we have here), it normally implies smaller coalitions
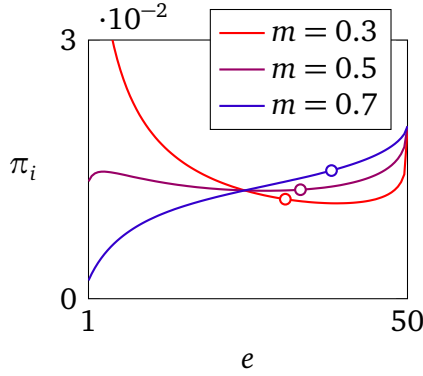
Figure 4.5: Scaled individual payoffs (as given by the first RHS term of (4.38)) to members of the elite coalition as a function of coalition size e for $|V| = 50$, $\alpha = 0.65$, $\theta = 2$, and different values of parameter m. Circles indicate the endogenous equilibrium coalition size for each m.

with higher payoffs per member compared to 'open membership' (Yi 1997).

Below we will illustrate that this result does not necessarily hold in our setting. We do not impose a specific membership rule but rather compare the impact of exogenous coalition size on individual elite member payoffs, and subsequently compare these payoffs to those under the endogenous equilibrium coalition size derived in (4.15). Doing so, we find that elite payoffs are not necessarily maximized at the endogenously determined coalition size (4.15). Instead, members of the elite coalition may prefer an alternative elite size, which may be smaller or larger, depending on parameter combinations in the model. The rationale behind this result follows form our equilibrium mechanism. The elite coalition cannot attract more (less) violence specialists because in equilibrium the payoff to the marginal warlord is even higher (lower) than that of the elite members. This result is illustrated in Figure 4.5 for different parameter combinations of α and m.

Figure 4.5 shows the elite payoff function (4.5). Substitute (4.12) for τ and (4.10) for $Y(\phi)$ and rearrange to obtain elite payoffs as a function of exogenous parameters and e:

$$\pi_i = \left(\frac{(\rho(e,w))^{\frac{1}{1-\alpha}}}{e} \right)(1-\alpha)\beta \left(\frac{\alpha^2\beta}{\gamma} \right)^{\frac{\alpha}{1-\alpha}}. \tag{4.38}$$

Only the first RHS term of (4.38) depends on e and only this term was used to construct Figure 4.5, the other terms being constant for given parameter values. The figure shows individual elite payoffs as a function of elite coalition size e for three values of m. It also displays, for each m, the equilibrium coalition size as the outcome of our model. Clearly, elite members could benefit from a change in the size of the elite coalition. For

$m = 0.3$, elite members would benefit from a decrease in elite size. Conversely, for $m = 0.7$, elite members would benefit from an increase in elite size. For $m = 0.5$, they are largely indifferent. Only when elite members prefer a smaller coalition they would benefit from implementing exclusive membership. When they prefer a larger coalition, then apparently the equilibrium payoffs to warlords are sufficiently large to keep them out of the coalition; no warlord has an incentive to switch and become a member. In such a setting, exclusive membership would not affect coalition size.

The dependence of preferred elite size on m is due to the extent of diminishing marginal returns to group formation for any $m < 1$. If m is low, group size becomes less relevant for the share of production controlled by the elite than if m is large. Hence, for low m, a smaller elite coalition would increase the tax rent per elite member. The opposite effect holds for high m.

4.4.3 Endogenous exit

One question that may arise is how the profitability of warlordism and elite membership affects the equilibrium number of violence specialists. As a final extension, we scrutinize this question by endogenizing the number of violence specialists, allowing them to switch and become a producer themselves.[11] Recall our argument in Section 4.1 that entry of violence specialists is exceptional for a variety of reasons while exit is possible though in principle not attractive. In assessing the impact of possible exit on model outcomes, we assume that the exiting violence specialist will earn a payoff equal to the representative producer. The alternative approach is to drop the assumption of a representative producer and, instead, explicitly model the size of the producing population. Both approaches yield qualitatively similar results, but one advantage of the current approach is that we need not make explicit assumptions on the size of the producing population. We assume that the number of violence specialists is small compared to the producing population, such that any switch will not affect aggregate production. As before, violence specialists choose their preferred occupation, elite member or warlord, but now the third option is to exit and become a producer. To assess the resulting equilibrium, we equate violence specialists' payoff from (4.5)–(4.6) with the representative producer's utility (4.4). In doing so, we account for the cost of maintaining a patronage network by subtracting cost parameter η from the specialists' payoffs. The resulting functions, with superscript x indicating the possibility of exit,

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[11] There is also a literature on endogenous alliance or coalition formation wherein multiple and competing coalitions can emerge. In our interpretation of the elite coalition as the state such an alternative coalition would in principle form an additional state. Co-existence of multiple coalitions is analyzed by a.o. Skaperdas (1998) and Garfinkel (2004).

are:

$$\pi_i^x = \pi_i - \eta \quad \forall i \in E; \tag{4.39}$$

$$\pi_j^x = \pi_j - \eta \quad \forall j \in W; \tag{4.40}$$

$$\pi_k^x = U \qquad \forall k \notin E \cup W. \tag{4.41}$$

An important observation is that these affine transformations of payoff functions do not affect the choice of effort ϕ nor tax rate τ in the base model of Section 4.2. In other words, for given parameter values, endoginizing the number of violence specialists does not change our model results. As a result, both equilibrium production Y^* and the equilibrium elite-warlord ratio $\frac{e^*}{w^*}$ remain unchanged, which also fixes the controlled share $\rho(e^*, w^*)$ and, finally, the representative producer's utility U^*. The only important question that remains is how the total number of violence specialists may be affected by key model parameters.

Consider a violence specialist that switches to become a producer. For illustrative purposes, assume this specialist belongs to the elite coalition. The exit implies that in the new situation we have $e' = e - 1$. The new producer will earn the representative producer's utility. Since production is not affected, by (4.41) each elite member will have a higher payoff as they can now share their appropriated production, whose level remains unchanged, among a smaller number of elite members; see (4.5)–(4.6) upon which (4.39)–(4.40) are based. The same result follows for warlords, since the equilibrium elite-warlord ratio remains unchanged and hence, in equilibrium, the number of warlords will adjust downward.

The equilibrium numbers of elite members and warlords can be derived by equating payoffs (4.39)–(4.40). We did so already for the ratio of elite members to warlords in (4.14). Next, we equate payoffs to elite members (4.39) and the representative producer (4.41), which yields the following implicit expressions for both e^* and – using (4.14) – w^*:

$$e^* = \left(\frac{(1-\alpha)\rho(e^*, w^*)Y^*}{\alpha\,(1-\alpha)\rho(e^*, w^*)Y^* + \eta} \right); \tag{4.42}$$

$$w^* = \left(\frac{(1-\alpha)\rho(e^*, w^*)Y^*}{\alpha\,(1-\alpha)\rho(e^*, w^*)Y^* + \eta} \right) \times \left(\frac{1}{\left((1-\alpha)\theta\right)^{\frac{1}{1-m}}} \right), \tag{4.43}$$

where $\rho(e^*, w^*)$ and Y^* take their equilibrium values from (4.18) and (4.19). Combining (4.14) with (4.42), we can calculate the numbers of elite members and warlords under endogenous exit. As an illustration we limit ourselves to assessing the impact of
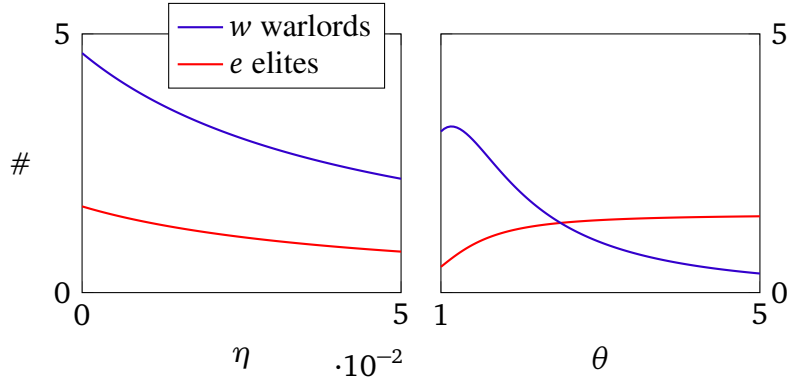
Figure 4.6: Equilibrium number of elite members and warlords as a function of patronage cost η (left panel) and cooperative quality θ (right panel) for $\alpha = 0.6$, $\beta = 2$, $\gamma = 1$, $m = 0.5$, $\theta = 1.5$ (left panel), and $\eta = 0.03$ (right panel).

two parameters on these numbers: patronage costs η and cooperative quality θ. Starting with η, since all terms in (4.42) and (4.43) are positive, we find that $\partial e^*/\partial \eta < 0$ and $\partial w^*/\partial \eta < 0$. Given the unchanged equilibrium elite-warlord ratio from (4.14), this implies that both the number of elite members and warlords decrease in patronage costs. The impact of θ is more nuanced, because it affects the equilibrium elite-warlord ratio. Since both $\partial \rho(e^*, w^*)/\partial \theta > 0$ and $\partial Y^*/\partial \theta > 0$, we have that $\partial e^*/\partial \theta > 0$. The number of elite members increases with cooperative quality θ. The number of warlords increases with θ too, but only for sufficiently high α and low θ; it decreases otherwise because the second term in (4.43) decreases in θ.

The effects of endogenous exit are illustrated in Figure 4.6 which displays the impact of patronage costs η as well as cooperative quality θ on the equilibrium number of elite members and warlords. As shown, when η increases it becomes more attractive for violence specialists to exit and become a producer. Note that the equilibrium elite-warlord ratio, equal to 1/4 for the parameter values in Figure 4.6, is independent of η. When θ increases, the elite coalition grows, but the impact on the number of warlords is ambiguous. The combined effect is that the number of violence specialists is first increasing, then decreasing in θ.

4.5   Discussion and conclusion

In this paper we provide economic intuition for the natural state by analysing the role of organized large-scale violence and how it is contained. We start from the observation that natural states lack a monopoly on violence concentrated in 'the state', but assert that the capacity for large-scale violence is concentrated in the hands of a small subset of the population that we term 'violence specialists' (North et al. 2009).

These violence specialists choose between exploiting their violence capacities for pure appropriative purposes or to impose a degree of order, where we refer to the former category as 'warlords' and the latter as 'elites'. The elite join forces in a coalition of equals where they agree to respect each others' privileges and rents, and to jointly fight off warlords. This relative security in turn allows the elite coalition to incorporate the consequences of their appropriative behaviour on total production in their appropriation rate—resembling the behaviour of stationary bandits in the fashion of McGuire and Olson (1996) and Usher (1989). Production is generated by a representative producer that responds to appropriation by limiting its investment in effort—the only variable input in our production function. In contrast, warlords operate alone and face continuous pressure on their rents from other violence specialists, which induces them to fully appropriate all production they control. The share of production controlled by warlords and elite members is determined by a deterministic standard contest success function, featuring diminishing returns in group size and a cooperative advantage to the elite coalition relative to warlords (Garfinkel and Skaperdas 2007; Konrad 2009). As a result, warlords have a natural advantage in appropriation rates over elite members, whereas elite members have a contest-advantage over warlords.

Our model illustrates the rigidity of natural states. We find that elite members are responsive to the economic consequences of their appropriation on production, thus restricting their tax rate, and more so the higher the output elasticity of effort. However, lower tax rates imposed by the elite increases the appropriative advantage of warlords over elite members. As a result, more violence specialists will opt to become warlord, in turn increasing the general appropriation rate imposed the violence specialists at large, and at least partially offsetting the elite members' appropriation decline following limited taxation. Due to this trade-off, our results indicate that the total appropriation rate of warlords and elite members combined is rather stable for most parameter combinations. Only when the output elasticity of effort is high—inducing low tax rates—combined with strong cooperative quality of the elite and a low decisiveness of conflict—allowing the relatively small elite coalition to control a relatively large share of total production—do we find comparatively high levels of production and producer welfare. These results point to a delicate balance between welfare and order as also observed by Bates et al. (2002). Dal Bó, Hernández and Mazzuca (2015) analyze this balance as a pre-institutional process, arguing that institutions play no role in explaining different outcomes across states. Their setting is different—i.e. a monolithic incumbent with given defense and growth capabilities owns a productive asset and is challenged by a predatory competitor—and we place our analysis in the specific institutional context of a natural state, but their conclusion is similar to ours: order

is a necessary condition in stimulating production and thus welfare, but welfare, in turn, decreases the likelihood of order. Like Dal Bó et al. (2015), our results challenge existing explanations for welfare that focus on the intentional design of institutions by the elite fostering or limiting production and appropriation (cf. Grossman 2002; Acemoglu, Johnson and Robinson 2005a).

Thus, our model is capable to answer, at least partially, why societies with an extractive elite emerge and persist. The traditional answer is that the elite is better off in an extractive and exclusive regime and powerful enough to maintain it (Sokoloff and Engerman 2000; Grossman 2002; Acemoglu and Robinson 2008). In contrast, we start from the assertion by North et al. (2009) that violence is an endemic threat to the stability of societies, arguing that a society with a small and extractive elite coalition is the natural social order since it guarantees a certain degree of order and stability, through a system of coercive rent extraction rather than by unconstrained, and potentially violent, appropriation. To this, we add that violence specialists have a potentially lucrative alternative option when they operate as warlords outside—and in competition with—the elite coalition. We view this as a permanent situation where the fragile elite coalition forms the *de facto* state while many violence specialists remain tempted by the warlord-option. Thus, even though incorporating a broad range of violence specialists into the elite coalition may be optimal for the leaders of the elite coalition—as is convincingly shown by Francois et al. (2015)—and for society at large, an elite coalition that completely eradicates warlords is unlikely in natural states.

In our extensions we further probe the relation between the average payoff for elite members and the size of the coalition. There are two alternative results put forward in the literature. Francois et al. (2015) argue that increasing the size of the elite coalition generally increases the average payoff to elite members, because the marginal elite member requires the highest reward for joining. In contrast, North et al. (2009) suggest that the elite coalition generally has an incentive to restrict its own size because this increases the average payoff per elite member. We provide two further insight into these results. First, in Section 4.4.2 we show that the relation between the average payoff per elite member and the size of the elite coalition is ambiguous, and crucially depends on the decisiveness of group size in conflict. When the decisiveness of group size is low (high) the endogenous ratio of elite members to warlords is too high (low) for the average elite member. That is the average elite might be better off limiting the number of coalition members when decisiveness is low. A reduction in the number of elite is problematic, because the option of becoming warlord at such an equilibrium is even less lucrative such that no elite members will willingly leave the coalition. Our second insight follows from the introduction of distributive conflict in

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the elite coalition discussed in Section 4.4.1. While a standard result in the literature on conflict and appropriation is that investment in conflict decreases with the number of contenders (Garfinkel and Skaperdas 2007), we show that this effect within the coalition is mitigated by the fact that an increase in the size of the coalition results in an increase of the contested resource pool—i.e. output under control of the elite coalition. This in turn stimulates investment in conflict. Which effect dominates depends on parameter values.

Our results imply that almost all natural states experience continuous coercion exercised by elite members and violence between elite coalitions and warlords, as is most conspicuously observed in parts of Sub-Saharan Africa. We show that this is not a temporary out-of-equilibrium-situation but a permanent phenomenon. There is an inherent tendency in natural states to have rebellions and competing factions, not necessarily because of ethnic or religious rivalries, ideological struggles or social injustice, but because of the fundamental economic mechanisms of the natural state resulting in numerous violence specialists opting for the 'warlord-option' (see for the role of economic factors on rebellions and civil strife also Collier and Hoeffler 2004; Blattman and Miguel 2010; Van der Ploeg 2011). Our model provides insights on how these economic mechanisms work. Specifically, we show that unrest is exacerbated when the output elasticity of effort is high, because it induces elite members to limit their tax rate, which in turn results in a strong tendency towards warlordism. We have also shown that this mechanism is partly mitigated by the cooperative quality of the elite coalition for two reasons. First, its cooperative quality allows a coalition of given size to control more production, inducing more violence specialists to join to elite coalition, further increasing its control over production. Second, in our last extension, we show that increases in the cooperative quality may even induce some warlords to abandon their capacities as violence specialist completely and turn to production instead.

More tentatively, our results on the interaction between order and production carry implications for the resource-curse literature that suggests that institutional quality determines to what extent natural resources are a curse—when institutions are inferior— or a blessing (Bulte et al. 2005; Van der Ploeg 2011). Output elasticity can be related to the availability of rich natural resources. Since the natural state can be equated with the weak or inferior institutions of the resource curse literature, this result underpins Bulte et al. (2005) and Mehlum et al. (2006) and others in showing that the effect of abundant resources on economic development is mediated by the quality of the institutional framework. Furthermore, the cooperative quality of the elite coalition can be related to the development of a rule-of-law among elite members and the development of

organizations within the coalition, as discussed in North et al. (2009) as part of the maturation process of natural states; we have probed this maturation process elsewhere using historical case material (Van Bavel et al. 2017). Indeed, our results suggest that enhancing institutional quality mitigates the resource curse. However, the third condition required to obtain comparatively high production in our model is that the decisiveness of conflict is low, which seems at odds with the availability of natural resources. Especially, natural resources can be geographically concentrated—often referred to as point-based natural resources—which brings about strong benefits for the party that controls the particularly endowed geographical areas. Consequently, we suggest that the point-based nature of natural resources increases the decisiveness of conflict and, thus, forms an important determinant of the resource curse. The more concentrated natural resources are in specific geographic areas, as is the case with many mineral resources in Sub-Saharan Africa, the more decisive conflict is, and the more unlikely a state is to escape the resource curse (Bulte et al. 2005; Van der Ploeg 2011).

Using the model developed in this paper, there are several promising avenues for further research. One concerns the spatial dimension of production and conflict in natural state, including the point-based nature of some natural resources, migratory behaviour of producers in the face of coercion and violence, and the spread of violence and coercion itself. The second concerns the formation of patronage networks and potential heterogeneity across violence specialists. The third concerns the link between the current paper's insights and the big question of how to escape the situation of violence and coercion in natural states towards open democracies. Our results suggest that such an escape may be even more problematic than conventionally thought, because of the economic mechanisms underpinning the rigidity of natural states.

Chapter 5

Understanding the economics of limited access orders: incentives, organizations and the chronology of developments

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Abstract. In 'Violence and Social Orders', North, Wallis and Weingast highlight the need of societies to control large-scale violence. In response to this need, a variety of social orders has emerged with differing institutional, political and economic characteristics. One of these social orders is the limited access order that was prevalent in most of history and still is nowadays. Taking the conceptual framework of North et al. as a starting point, we make three advances to their analysis of limited access orders. First, we analyze the incentive structure of actors involved, using a formal model of the main interactions in a limited access order. Second, we decompose organizations into two types and analyze their respective roles. Third, we use insights from historical research to scrutinize the chronology of the rise of organizations. Jointly, this allows us to refine and substantiate the insights gained by North et al., highlight the role of organizations and place the start of relevant developments earlier in time.

5.1 Introduction

One of the big questions in the social sciences is why societies display such large differences in economic and social performance, and which distinguishing features and characteristics at the societal level underlie these differences. In search of answers, many scholars have focused on the institutional characteristics of societies, either being forms of political organization, social structures or the rules that govern economic life. A large strand of works has looked, for instance, at the organization of exchange and allocation of goods and production factors, dominated either by kinship structures, communities, feudal systems, markets or states, whilst others have primarily focused on differences in the institutions that shape political life. To understand how such institutional characteristics affect economic and social outcomes, we need to clarify how they determine the incentives of the actors involved at the micro level. These incentives can be found, for instance, in the desire to generate or extract rents, find safety, better employ labor or land, or acquire freedom or political leverage. In addition, we should ask how the institutional structure of society is itself shaped by these incentives and how and why it develops over time, for instance, from less to more open systems of exchange or political interaction.

An important, recent contribution to this field is *Violence and Social Orders*, by North, Wallis and Weingast (2009, henceforth NWW). They stress violence as the crucial variable in understanding differences across societies. Specifically, they focus on the need of societies to control large-scale, organized violence and on their relative success in doing so. Violence may lead to destruction of lives and capital goods, and deter interaction, exchange, trade and the benefits of specialization that come with trade, leading to significant welfare losses (Acemoglu and Johnson 2005). This idea is the starting point of NWW in explaining and understanding the existence of specific social orders, which can be interpreted as archetypal societies, with specific institutions that emerge because of the necessity to control violence. They distinguish three social orders: the 'foraging order' that governed human life until the Neolithic Revolution, approximately 10,000 years ago; the 'limited access order' that was prevalent since then; and the 'open access order', which developed only some 200 years ago, in a handful of western countries.

The book by NWW is ambitious—as illustrated by its sub-title *A conceptual framework to understand recorded human history*—and it is widely acknowledged to be an important contribution. Its impact on actual research up to now, however, remains limited. Possible causes are that the book is conceptual and highly abstract, and that the economic logic behind some of the mechanisms of the social orders, and of transitions between orders is unclear. The authors have decided not to develop

a formal model or empirically testable hypotheses (p. xii). Instead, they provide a conceptual framework wherein violence is linked to political organization and economic performance, and, importantly, to the distribution thereof. However, the incentives of actors are mentioned but left implicit in their framework. In part, this is because they do not discuss the interaction between production and appropriation in a systematic way. As a consequence of not specifying incentives, it is difficult to comprehend what the problems, constraints and strategies of the agents are, as several reviewers have already remarked (e.g. Bates 2010: 755). NWW thus present to the social sciences an encompassing but abstract framework to distinguish between different societies through time and space, and understand the basic functioning of each of these societies. In this paper, we argue that the conceptual framework of social orders can be advanced precisely by specifying explicit relations between appropriation – based on the distribution of violence capacities—and production, following Van Besouw, Ansink and Van Bavel (2016), see Chapter 4. Then, using these relations, we further assess the interaction between violence, political organization and economic performance. Also, we further specify the role of organizations in this interaction; a role that is stressed by NWW (see below), but will be specified here by explicitly relating it to incentives and production. Doing so for the 'limited access order', we not only sharpen our understanding of the mechanisms of social orders, but also come closer to understanding societal development and major transitions of the type discussed by NWW—both political and economic—and to position them in time more accurately.

Before we present our additions in detail, it is necessary to discuss how NWW conceptualize the limited access order—alternatively called the natural state. A limited access order 'manages the problem of violence by forming a dominant coalition that limits access to valuable resources—land, labor and capital – or access to and control of valuable activities—such as trade, worship and education—to elite groups' (NWW: 30). Membership of this coalition is, by construction, limited to individuals with the capacity to muster organized violence. In the terminology of NWW, they are 'violence specialists'. Violence specialists form a subset of a society's population and are able to use large-scale, organized violence and to coerce others under the threat of violence. Their ability to do so is enhanced by patronage networks, social capital, human capital, physical strength, wealth, status or prestige, and these to varying degrees, depending on the specific context. The rest of the population, in contrast, has no capacity for large-scale, organized violence and is therefore in principle not able to join the elite. Violence specialists within the elite coalition use their power to collectively extract rents from the rest of the population; rents that are used to hold the coalition together. Although the coalition utilizes its coercive power against the rest of society, under

the threat of violence, it restricts open violence. The result is a social order with a strong elite that exercises its coercive power to extract rents from the rest of society. Although competition among violence specialists for the distribution of rents may exist, membership of the elite coalition entails a lasting, informal agreement to respect the privileges and rents of other members. On the other hand, the elite coalition competes, as a group, with violence specialists outside their coalition. Violence specialists outside the coalition—termed 'warlords' here—are those who refuse to commit to the coalition's agreements and those who are not allowed access to the coalition. As violence specialists, they have the capacity to extract rents from the ordinary population. They thus compete for control of the society's rents. In open access societies, by contrast, the states possess a monopoly on the legitimate use of violence, as they have consolidated military and police organizations which are controlled by the political system. All citizens have access to the political and economic systems, and they have the right to form organizations. These sustain impersonal exchange and allow all citizens to compete for political control and for economic rents, which are continuously eroded as a result of this political and economic competition (NWW: 21–23). By this definition of social orders, we follow NWW in arguing that almost all historical societies and most contemporary ones can be characterized as limited access orders.

We should note a marked difference between the 'elite coalition' in limited access orders as depicted by NWW, and of 'the elite' as set out in the literature that compares conflict and development in anarchy with some form of hierarchy (e.g. Acemoglu and Robinson 2006b; Bates, Greif and Singh 2002; Grossman 2002). In the latter, the elite is generally treated as a *monolithic* entity maintaining order among the rest of the population and levying taxes in return. In the limited access order, however, the elite coalition emerges from the pool of violence specialists, and cooperation of violence specialists in this coalition is not self-evident as there is always a threat of their not joining or of their leaving the coalition—the latter, in our view, is nothing more than a violence specialist choosing to no longer obey the coalition's agreements. Hence, the elite is a *composite* entity, and behavior of individual violence specialists is constrained by their relations with other violence specialists. This interpretation of the elite coalition strikes us as a major advance and brings reasoning closer to real-world situations, both historically and at present. One can, for instance, think of the situation in Western Europe in the Middle Ages. Here, the elite would be the feudal elite, with its members competing with each other for rents. Warlords, too, would be violence specialists, but operate outside the dominant feudal order as robber barons, captains of roving mercenary troops or noblemen with independent domains or territories. In the present-day world, in limited access orders like Burma, Cuba, Mexico, Russia and

many sub-Saharan countries, the elite would be the politicians and officials in power, who use the state apparatus as a personal fiefdom and compete with each other for rents, whilst the warlords would be rival factions and rebel leaders (see case studies in North, Wallis, Webb and Weingast 2013).

Whilst NWW take it as given that violence specialists prefer to be part of the elite coalition, here we will endogenize this preference by introducing production as an important variable. Depending on production levels and the size of the elite coalition, violence specialists may prefer not to enter the coalition, but to operate alone as warlords. The advantage of not entering is that warlords are not bound by any rules of the coalition regarding the use of violence and the rate of appropriation of production, and may therefore be able to generate a higher income than if they had joined the elite coalition. This implies that maintaining a stable elite coalition within limited access order is even more problematic than NWW already assume. Clarifying the incentives of violence specialists allows us to identify this trade-off and forms an important addition of this paper. In Section 5.2, we will explain such incentives in detail.

A second major advance made by NWW is the way they highlight the role of organizations. In *Violence and Social Orders*, organizations form a major element in the transition from limited access to open access orders. As such, they are analytically separated from institutions, a distinction sometimes considered as one of North's main contributions to the literature (Wallis 2015). In his earlier work, North had treated organizations as manifestations of institutions, which already was an advance over the neoclassical focus on individuals. By defining institutions as the rules of the game and means of enforcement, and then separating the rules from the organizations that actually play the game, it became possible to have a dynamic relationship between the interests and incentives facing the organizations and the structure of the rules.

This separation, and the resulting role of organizations, is particularly evident in *Violence and Social Orders*. The limited access orders are divided there into three ideal types: fragile, basic and mature orders, each typified by the role and structure of their organizations. These ideal types together form a spectrum along which organizations become more durable, more complex, less bound to personal power, more numerous and less dependent on the dominant coalition (NWW: 20–21 and 41–49; North et al. 2013: 10–14). Even though there is no teleological progress, since mature limited access orders can regress and revert again, the organizations thus form a vital component in development, as they do also in the transition from mature forms of limited access orders to open access orders. In the latter, the number of organizations is large, they can be freely founded by all citizens, and access to them is an impersonal right of all

citizens. An elaborate system of rules, and checks and balances on powerful individuals and on impersonalized organizations sustain the open access order. The number of open access orders, by their definition, so far remains small.

Our paper is inspired by NWW's *Violence and Social Orders*, but we claim to make three advances, by further scrutinizing and adjusting important parts of it. First, we stress the trade-off violence specialist's face in deciding to join the elite coalition as a crucial force of inertia in the political and economic development of limited access orders. Second, we discuss the role and importance of organizations within this framework. That is, we discuss the importance of organizations in developing more stable configurations of violence specialists, and the effect of organization on production—as a stylized representation of economic development. We do this for what has been in most parts of history, and in most parts of the world still is, the most widespread order: the limited access order. To this end, in Section 5.2, we use a formal model, based on Van Besouw et al. (2016)—see Chapter 4—that includes production as a variable, in order to gain insight into the incentives of the violence specialists. Next, we follow North in his stress on the role of organizations and assess their role in relation to incentives and production. As a first step, in Section 5.3, we discuss what these organizations actually are, using the historical record. This discussion will establish the need to go beyond the single category of organizations considered by NWW. Whilst the organizations discussed by NWW are top-down, and dependent on the state, we suggest including a category of bottom-up organizations. In Section 5.4, we will use the model to stress the importance of distinguishing between these two types of organization. In Section 5.5, we will return to the historical record and discuss what these results imply for the chronology of developments as pictured by NWW. Our third, and final contribution is that our analysis leads us to suggest that this chronology needs to be revised and that relevant developments started earlier in time than NWW argue. Section 5.6 summarizes our main findings.

5.2 Clarifying economic incentives: a model of the limited access order

In order to gain more insight into the incentives of actors, we represent the limited access order by means of a model, based on Van Besouw et al. (2016)—see 4—and we refer to the Appendix of the current paper for technical details of the model. Here, we single out the elements of the limited access order that are central to our discussion, and we focus on how these elements interact with each other through the behavior of violence specialists and their interaction with the rest of society. Accordingly, we

make three simplifications. First, we model a society in isolation. This means that, abstracting from reality, no exchange or interaction with other societies exists. Doing so, we follow NWW, who also largely leave this interaction outside of their analysis. This omission is noted in various papers discussing how developments within a limited access order can be influenced by its interactions with other societies (Frankema and Masé 2014; Grimmer-Solem 2015). Abstracting from this interaction, however, allows us to focus on the central elements here: the internal consistency of a limited access order, the incentives of violence specialists and production.

Second, we model the interaction between the distribution of violence specialists and the level of production in a static model. Thus, we do not account for potential long-term effects of particular distributions of violence specialists on the production process in the model. Such effects are, of course, possible when the relative size of the elite coalition has effects on factors such as population size—relative to the number of violence specialists—technological progress or institutional arrangements. Although these long-term effects are plausible, we would stress that the inherent instability of the coalition and the strong tendency of violence specialists towards warlordism—as will be demonstrated below—limits the scope for structural change in the economy of limited access orders. Instead, we follow NWW in suggesting that organizations are the vital ingredients for fostering long-term developments—and discuss these from Section three onward.

Our third simplification is that we consider violence specialists as individuals and, for reasons of practicality, their capacities as homogeneous. The latter has two technical implications. First, it allows us to ignore the specificities of the formation and size of patronage networks of each individual violence specialist. Second, we need not explicitly model entry into and exit from the elite coalition because violence specialists are identical and face identical choices. Of course, heterogeneity across violence specialists, and the balance of power within and composition of elite coalitions are important elements of a limited access order but we argue that this is not crucial for understanding the main interactions between violence specialists and producers in a limited access order but rather adds to explaining empirical variation across countries.

The central element in our model is the violence specialists' behavior in terms of their choice between joining the elite or becoming warlord, and its implications for the rest of society, notably its interaction with production levels and welfare. The choice between coalition membership and warlordism depends on the relative profitability of joining the elite—and, accordingly, to remain an elite. In other words, is it profitable enough for violence specialists to join the elite and thereby settle to abstain from violence, at least vis-à-vis other coalition members? The mechanism that we employ to

model this choice is that specialists will choose the most profitable 'occupation'. As a result, violence specialists continue changing their occupation until the payoffs of both groups are equalized.

Adding to NWW, we proceed by formulating the payoff structure and resulting incentives for two different types of agents—see Van Besouw et al. (2016) or Chapter 4 for a full discussion, and the Appendix to this paper for technical formulations. The payoff to violence specialists, the first type of agent, is determined by the total size of production, the relative share of production they can access and the rate at which they appropriate production from this accessible share of production. We assert that the appropriation rate of warlords and elite members differs crucially. Coalition members, by virtue of their commitment to respect the coalition, have relatively secure access to some share of total production. This allows these individuals to expect future benefits of their choices. Accordingly, they can decide to limit their rate of appropriation when this would increase production. Warlords do not have a secure access to a share of production and, therefore, appropriate as much as they can. This implies that warlords have a clear advantage in terms of their appropriation rate. Members of the elite coalition have a relative advantage in fighting warlords by virtue of their cooperation. We refer to this advantage as the 'cooperative quality' of the elite—see Section 5.3 for a discussion. The second type is a representative producer. The producer optimizes consumption, which is total production net of production costs and appropriation. Production requires costly investments governed by decreasing returns, which induces the producer to reduce his production in response to appropriation.

Violence specialists and the representative producer interact with each other in three subsequent stages of our model. In stage 1, violence specialists choose their occupation, which yields a specific distribution of elites and warlords. Each group controls a share of society and its production, determined by the relative size of both occupational groups and the cooperative quality of the elite—this is formalized in what we call a control function.

In stage 2, given the outcome of the control function, elite members decide on their level of appropriation—termed 'tax rate' in what follows—whilst taking into account that a high tax rate may deter production such that reducing the tax rate might increase the elites' payoff—as in McGuire and Olson (1996). By construction, warlords exploit their violence capacities in order to appropriate all production that is under their control. In the model, we find that this optimal tax rate depends negatively, and only, on the marginal product of investments in production.

In stage 3, given the outcome of the control function and the elite tax rate, the representative producer chooses his production level. The level of production is

governed by a conventional production function.

We assume that each agent—i.e. elites, warlords and the representative producer—maximizes his payoff (see Appendix). For the representative producer, this is simply production net of production costs (in terms of productive investments) and net of appropriation by warlords and the coalition. For elites, this is the tax income. For warlords, this is the, deterministic, appropriation income. This results in a static model in which all agents make optimal decisions, taking into account the decisions made by the other agents (see Van Besouw et al. 2016 or Chapter 4 for a detailed analysis of this model).

To analyze this model, we need to specify both the control function and the production function. We choose to adopt very intuitive properties for both functions. For the control function, we assume that the share controlled by the elite increases (decreases) in the size of the elite coalition (the number of warlords)—with diminishing returns to elite size—as well as in the elite's cooperative quality, i.e. the extent to which the coalition cooperates and thereby has a strategic advantage in fighting warlords for the share of total production controlled by each group. We assume that profits within the occupational groups are distributed equally among members, as a consequence of our decision to model homogeneous violence specialists. For the production function, we assume a standard one-input production function (see Appendix). As a result, the tax rate and investment in production are jointly determined in the model and an equilibrium emerges where violence specialists switch to the most profitable occupation until payoffs to both occupations are equal—implying that no violence specialist can improve his payoff by switching occupations.

We emphasize three important mechanisms that result from the model. First, incentives for producers to invest in production arise from low appropriation rates and from the marginal product of effort. Second, the elite are willing to impose low tax rates, stimulating producers to increase investments in production, when the returns on such investments are high. Third, incentives for violence specialists to join the elite decrease when the tax rate is lower, implying that net appropriation is generally rather inert and high. The tendency of violence specialists to opt out of the elite follows logically from the fact that the production enhancing effect of a lower tax rate generates higher output and, thus, benefits violence specialists in both categories. On the one hand, this result derives clearly from the assertion advanced by NWW that we need to think of the elite as a composite entity, but, on the other hand, means that we need to emphasize that there is no straightforward incentive for violence specialists to join the elite—contrary to the assumption of NWW. In other words, production and order are trade-offs and the total level of appropriation is generally high, ranging from a situation

Figure 5.1: Number of localities with mortality data per year
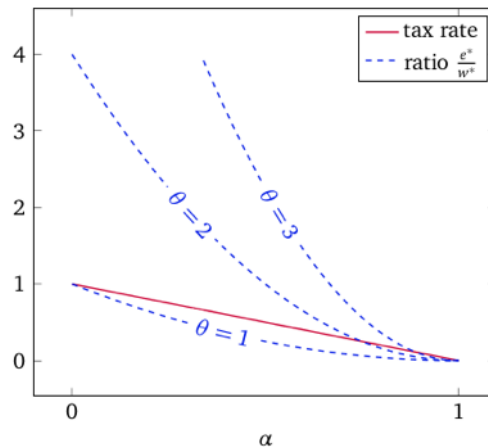
Note: Tax rate and elite-warlord ratio as a function of the output elasticity of effort α and elite cooperative quality θ (based on model and parameter values introduced in the Appendix).

with high taxes imposed by a strong elite to a situation with low taxes combined with a large group of warlords. These last two results are summarized in Figure 5.1, with the output elasticity of effort depicted by parameter α and elite cooperative quality by θ (see the Appendix for details).

The model indicates that the interaction between the distribution of violence specialists—as a representation of political order—and economic productivity indeed provides a rather inert system, as one would expect following the argument of NWW. The political configuration allows little room to enhance production—which is discouraged either by high taxes or high appropriation by warlords. Furthermore, our discussion here illustrates that a more productive environment is in itself not a solution to escape this ordeal, since it will induce a tendency towards lower tax rates and, thus, a disincentive for violence specialists to join the elite coalition, resulting in increased insecurity and a negative effect on production.

This apparent deadlock would be avoided as limited access orders mature, or progress towards open access. At the same time, such progress is far from evident, given the internal consistency of limited access orders, so importantly stressed by NWW. Following the discussion in this Section, however, the enhancement of the cooperative quality of the elite could play a key role in breaking this deadlock, as it would enable limited access orders to combine political order and welfare to a larger degree than otherwise would have been possible. This is because enhancement of cooperative quality allows a relatively small coalition to control a disproportionally large share of production, and foster production due to limited tax rates at the same time (more extensively: van Besouw et al. 2016 or Chapter 4), which results in relatively

favorable outcomes in terms of production and payoffs. This line of argument fully links up with the emphasis placed by NWW on the institutionalization of organizational structures of the elite, which, they argue, decreases the instability within the elite coalition. In Section 5.4, we will further develop this argument, and suggest that the main effect of a more stable elite coalition in limited access orders is that it generates an advantage to the elite in fighting warlords. Before doing so, however, we contrast the interpretation of organizations offered by NWW with the historical record.

5.3   Introducing organizations

One of the main points made in *Violence and Social Orders*, and in North's later work more generally, is the identification of organizations as a separate category of analysis, distinct from institutions. More specifically, the book highlights the role of organizations as crucial elements in the transition from limited to open access orders, which is posited to have taken place in Britain and the United States by 1850, in France by 1880 and in some other Western countries even later. In limited access orders, the number, complexity and size of organizations is limited. In addition, access to these organizations is mostly restricted to the elite. In open access orders, by contrast, the number of organizations is much larger, the right to form them is open to all citizens and access to them is an impersonal right that all citizens possess.

Likewise, organizations play a crucial role in the development of limited access orders, which move back and forth within a spectrum from fragile to mature types. Three ideal types of limited access orders are discerned by NWW (p. 21; see also North et al. 2013: 10–14), each characterized by a different structure and role of organizations. Fragile types have very few organizations that persist over time, including the government, and these are all linked to the personality of their leadership, with the leaders personally connected to the dominant coalition. In basic limited access orders, the government is durable and is the main organization, but some non-government organizations exist, mainly formed and staffed by elite members, and closely and personally linked to the government and the dominant coalition. In the mature form of limited access orders, many organizations outside the government exist and have longer lifespans, but access is still limited to organizations supported by the government and which allow the dominant elite to create rents. So, if a society were to progress along these lines, organizations would become more impersonal, longer lasting and access would become more general, outside government intervention, a process solidified when the transition to open access orders takes place.

What is the exact type of organizations that we are considering here? We follow

NWW in their focus on contractual organizations. These are organizations that use both self-enforcing agreements and third-party enforcement, as some contracts between the members may not be incentive-compatible at all points in time and thus need to be enforced. The contractual organizations mentioned by NWW include units of government (states, municipalities); but also business organizations, corporations and partnerships, religious and charitable organizations, and cooperatives.

We diverge from NWW, however, regarding two of their additional assumptions. First, they assume (p. 20) that in limited access orders these contractual organizations are normally founded by the elite and require the structure of consent established within the elite. Second, they argue that contractual organizations rely on third-party enforcement and function only with the explicit support of the state (p. 7).

We question both assumptions on the basis of the recent historical literature. First, it is becoming increasingly clear how organizations, according to the same definition, may also have developed, and actually were developed, from below. That is, by ordinary producers and not only by violence specialists. Examples of such organizations are guilds, town communities, village communities and charitable organizations, which have been founded by the thousands in Western Europe from the 11th century onwards (De Moor 2008; Epstein 1991: 50–62 and 130–135). The main actors within these organizations were merchants, traders, retailers, craftsmen and peasant farmers. They mostly owned the means of production (land, capital goods) and worked independently but were not violence specialists or elites. Their organizations, operating at the local and regional level, and sometimes forming regional networks, were perpetually lived, contractual organizations. Enforcement was organized internally through elaborate systems of administration and jurisdiction for those—relatively numerous—instances where cooperation within the organization was not incentive-compatible for all members. This is testified by the huge numbers of archives containing all kinds of complaints, conflicts and forms of litigation between members of these bottom-up organizations and settled by internal bodies or community authorities. The guilds (which are surprisingly not discussed in NWW, apart from one single sentence) often took on responsibilities in contract enforcement, a role supplemented or sustained by that of the public authorities of town communities (Epstein 1991: 80–91). Some scholars stress the independence of the guilds in fulfilling this role (Greif, Milgrom and Weingast 1994), whilst others stress the reliance of the guilds on town communities (Ogilvie 2014: 177–179). Both cases, however, support our point because the same town communities in Western Europe were in fact also largely bottom-up organizations, with elaborate systems of enforcement that mostly did not require the intervention of the state even if many rules were not incentive-compatible at all times.

This leads us to the second observation, that the roles of these organizations were not always dependent on the support of the state. This is in contrast to the assumption by NWW. A clear example is given by town communities in late medieval Western Europe. Their structure and role are incompletely represented in Violence and Social Orders. Town communities are discussed by way of a single case—Lille in the 18th century—using a single study. On the basis of this, NWW suggest that these communities were only halfway on their path to perpetual life as corporate identities and impersonality, and only functioned as a result of the recognition and support of the state, in this case the French king (NWW: 70–71). Their example is ill-chosen, however, because it is from an era in which the heyday of the independent town communities—that is, the 13th to 16th centuries—was long over and from an area—the north of France, just conquered from the Low Countries, or Flanders more precisely—where interference by the king was relatively strong. It would have been more logical to have looked at the town communities in the Low Countries and Italy, which acquired a large degree of independence in law-making, jurisdiction, fiscal affairs, finance and administration (Van Bavel 2010: 110–117). These town communities, just like village communities, acted as independent, perpetual legal bodies, with legal personhood. Their capacity to sell rents and perpetual annuities provides evidence, and it is clear that they did so in large numbers from the 13th century onward (Tracy 2003; Van Bavel 2010: 186–187 and 190–191).

Returning to our attempt to structure the main elements of the limited access orders: bottom-up organizations had a multiplicity of goals, one of which was to shield parts of production from appropriation by violence specialists. For example, the guilds often had the explicit objective of protecting the living standards of craftsmen in order to enable them to have a decent standard of living, that is, substantially above subsistence level. They did so by strengthening their position vis-à-vis producers who were not members, but also vis-à-vis the extraction and claims by violence specialists (the first stressed more by Ogilvie 2014: 187; the latter by Greif et al. 1994: 749 and 753). To withstand the claims by violence specialists, they acted as pressure groups and coordinated forms of collective action, using boycotts, strikes and embargoes, and they also formed alliances with other groups and organizations. Town communities were perhaps even more successful in shielding their members from appropriation by violence specialists than guilds, as they negotiated with the dominant elite about the taxes to be paid by their inhabitants and used all forms of economic pressure, obstruction and political cunning to keep these taxes low (Blockmans 1997).

5.4 Impacts of two types of organizations

The preceding discussion suggests the relevance not only of top-down organizations formed by elites and explicitly supported by the state, but also of bottom-up organizations, formed by producers and independent of such state-support. In this Section, we return to the trade-off between production and order in limited access orders, or the 'deadlock' situation observed in Section 5.2. We do so by illustrating the distinct effects of the two types of organizations in limited access orders, using the model of Section 5.2. We stress that our simple parameterization of organizations is meant to illustrate their impact, abstracting away from many other potential effects, rather than to provide a comprehensive analysis of organizations. As argued at the end of this Section, alternative interpretations are possible, but these do not impede the current one. We refer to the organizations as described by NWW (p. 20), as 'top-down organizations'. These are modeled as enhancing the relative fighting advantage of the elite coalition—parameter θ in the control function (see Appendix). An increase in θ increases, the share of production controlled by the elite coalition vis-à-vis warlords—who cannot establish organizations, following from our assertion that their access to resources is insecure because it is contested by the elite coalition and because warlords do not systematically accept and protect each other's resource base. An alternative type of organizations is 'bottom-up' organizations. These can be modeled as shielding a proportion of production from appropriation—which is modeled using parameter η, which enters the payoff function of the representative producer (see Appendix). An increase in η increases the share of production that is secured by the representative producer and which therefore cannot be appropriated by the elite or by warlords.

The model outcomes that we focus on include (a) the level of production, (b) payoffs to producers as a proxy of total welfare in society, (c) the total appropriation rate and (d) the size of the elite coalition. Figure 5.2 illustrates our model results. Values of key model outcomes are displayed as a function of the size (or, alternatively, the maturity) of organizations. The left panel in the figure represents the impact of top-down organizations and the right panel that of bottom-up organizations.

Figure 5.2 shows that both types of organizations have a comparable impact on key model outcomes. Summarized, as θ or η increases, (a) production increases, (b) welfare increases and (c) the appropriation rate decreases. Yet, there are also two important differences. First, the size of the elite coalition, and therefore the share of production controlled by the coalition, is responsive to θ, but not to η. That is, top-down organizations facilitate an increase in the size of the elite coalition, whilst bottom-up organizations do not. Second, production and welfare increase at a decreasing rate with θ but at an increasing rate with η. That is, top-down organizations boost production

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Figure 5.2: Number of localities with mortality data per year
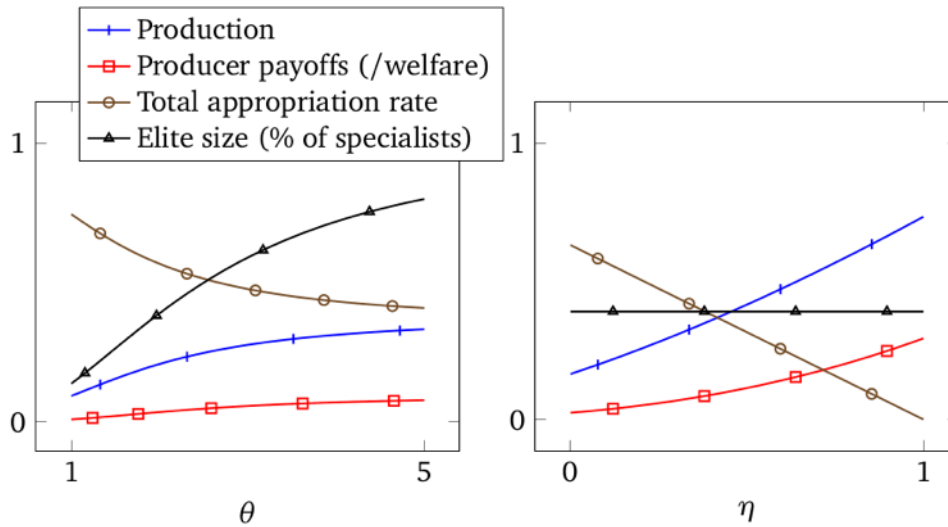
Note: Impacts of introducing organizations on production, welfare, appropriation and elite size. Left panel: top-down organizations (parameter θ). Right panel: bottom-up organizations (parameter η). The model, its main functions and parameter values used are provided in the Appendix.

but their impact reduces as organizations grow. Conversely, bottom-up organizations become more important for production and welfare as they grow.

A third difference between the two types of organization is illustrated in Figure 5.3, which displays violence specialists' payoffs under the two types of organization. To appreciate the figure, recall that by our mechanism for occupation choice, elites' individual payoffs are always equal to warlords' individual payoffs. The figure illustrates that these payoffs increase in θ but are hump-shaped in η. That is, growth in top-down organizations always facilitates higher payoffs for the elite coalition (and warlords) but at some level of bottom- up organizations, violence specialists' payoffs start to deteriorate. Before this point is reached, however, specialists' payoffs also increase in η, thus forming a possible component in clarifying why violence specialists would allow the rise of bottom-up organizations, at least to a certain extent. Eventually, specialists' payoffs converge to 0 as η tends to 1. The explanation is that, at this point, the entire production is secured by the representative producer and cannot be appropriated by the elite or by warlords. Since elites and warlords have no other sources of income in our model, zero appropriation implies that their payoffs drop to 0.[2]

We stress four final considerations in the interpretation of our model results. First, note that the increase in payoffs to the elite coalition as top-down organizations

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[2] The comparative static results of the output elasticity of effort and technology in this slightly adjusted analysis, compared to Section 5.2, are rather intuitive—increasing production, welfare and payoffs for violence specialists—and are therefore not treated in the text.

Figure 5.3: Number of localities with mortality data per year
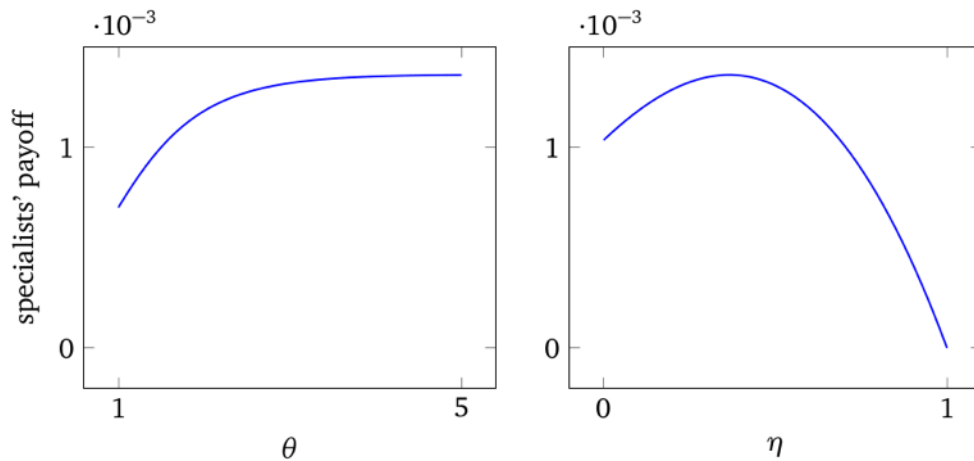
Note: Impacts of introducing organizations on violence specialists' payoffs. Left panel: top-down organizations (parameter θ). Right panel: bottom-up organizations (parameter η). The model, its main functions and parameter values used (similar to those in Figure 5.1) are provided in the Appendix

grow—or the increase in producer welfare as bottom-up organizations grow—does not imply that this growth will happen automatically. The development of organizations is costly and so there is a trade-off between costs and benefits of organizations. Adding a specific functional form for costs of organizations would allow us to derive a specific value for organization size; i.e. a specific point on the horizontal axis of Figures 5.2 and 5.3. Second, in the long run organizations may alter the production structure, thereby affecting the functional forms or parameterization of the production functions. Third, note that we have assessed top-down and bottom-up organizations separately, whilst certainly a combined analysis is possible. A full treatment of one or both considerations would not, given the simple setup of the model, provide insights beyond those presented here. Fourth, it is evident that alternative interpretations of our organization parameters are possible, or even attractive. The cooperative quality of the elite θ, may increase with an elite-advantage in conflict technology or political legitimacy. Also, the capacity of producers to shelter part of their produce—η—would be influenced by geographic conditions and their choice in producing certain crops or goods. Obviously, such alternative interpretations do not impede our interpretation, but they are relevant for comparative research on limited access orders.

5.5 Organizations and the historical chronology

The preceding Section substantiates the large effects of organizations, in line with NWW. It brings us closer to understanding the role of organizations in forming the

incentives of the agents and structuring the maturation of some limited access order societies and their transition to open access. This transition, according to NWW, was confined to some Western countries and happened only late in history, the first societies undergoing this transition being Britain and the United States, and not until 1850. It is only then that organizations, that is contractual organizations, have become impersonal, perpetual and with general access, and outside governmental interference. The transition, in their view, was preceded by a process in which mature limited access orders with more complex organizations, growing impersonality and more general access into the coalition developed in the 16th–18th centuries, as in England and France (pp. 69–72).

Section 5.4 endorses the stress put by NWW on the important role of organizations, but what about their chronology of the development of organizations? As discussed in Section 5.3, the historical evidence suggests that their chronology on this point may not be entirely correct, as a result of two assumptions they make. According to their view, (a) the contractual organizations are in principle founded by the elite among the violence specialists and (b) they function with the explicit support of the state (NWW: 7 and 20). In Section 5.3, we have used the recent historical literature to question these assumptions and to introduce a second category of organization: the bottom-up organizations formed by producers. Introducing them not only has large effects on the outcomes (as shown in Section 5.4), but also affects the chronology of developments.

We have noted that guilds, town communities, village communities and charitable organizations were founded in large numbers in Western Europe from the 11th century onwards (De Moor 2008). Their heyday as independent bodies was in the 13th to 16th centuries, and can be situated more specifically in Italy and, next, the Low Countries, where their position was strongest and most pronounced, as most clearly with the town communities (Jones 1997; Van Bavel 2015).

It is here that we can also best observe the independence of bottom-up organizations. The position and capacities of the town communities were often only recognized by some overlord in order for him to pretend to have some position, at least nominally, but without practical effect. The cases where the overlord tried to really effectuate some nominal or pretended right often led to long resistance and even open violence, and in most cases, the town communities were the victors (Blockmans 1997: 259–267). It is relevant to note that ordinary producers, organized within guilds, town communities and village associations, and acting by way of an organization or a collective, sometimes came to muster large-scale violence. As such, they became able to withstand violence specialists, especially those outside the dominant coalition but at times even

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the dominant coalition itself. A major example is the Battle of the Golden Spurs, in 1302, as the feudal coalition headed by the French king and 2,500 well-trained noble knights were defeated by a Flemish army composed of militias of guilds, towns and villages (Van Bavel 2010: 120–121). This victory in its turn greatly strengthened the legal and military position of the guilds and the autonomy of the town communities; an effect radiating through all of the Low Countries.

The Battle of the Golden Spurs was a spectacular event, but more generally the period of the 11th to 14th centuries saw a multitude of smaller events or mutinies, as organizations formed by ordinary people were able to withstand violence specialists and dominant elite coalitions (Prak 2015: 102–110; Van Zanden 2009: 50–53). In this way, producers were still not violence specialists, but they were sometimes successful in establishing organizations that develop the capacity of large-scale organized violence.

The subsequent period, the 15th to 18th centuries, saw various developments that gradually and intermittently changed this picture. We will tentatively indicate these developments here. First, the military balance started tipping to the princely overlords, that is, to the dominant elites, who were able to deploy ever larger financial and military power (Blockmans 1997: 267–271). This is where the changes in military technology and warfare of the 16th and 17th centuries, labeled the military revolution (Parker 1996), come in. They were not a driving force that inexorably led to state formation or changes in social orders (as noted by NWW: 177–181, on this point arguing against Tilly 1993) but it was a factor that in this specific context gave dominant elites and their state organizations an edge over bottom-up organizations. This happened even across state boundaries, as in Northern Italy, where town communities were defeated by the large armies of the French and Spanish kings and subsequently were eroded by new royal rule (Tilly 1993: 77–79). Second, town communities and guilds, or their leaders, in many instances became co-opted by or integrated into the dominant elites. Third, some bottom-up organizations and their leaders increasingly acted as violence specialists themselves, able to organize large-scale violence, and aimed at appropriating shares of production at the expense of rural producers—e.g. the town community of Florence and its leaders versus its rural surroundings—or competitors—the Hanseatic League versus rival merchants (Greif et al. 1994: 773; Ogilvie 2014). One could argue that an intermixture of top-down and bottom-up organizations took place in this period.

As observed in Section 5.4, and suggested by our reading of historical developments in this Section, the rise of bottom-up organizations had large effects on the economy; more specifically, it led to a decrease in appropriation and a large increase in production and welfare; and this to a much greater extent than with the rise of top-down organizations. This sped up the developments discussed by NWW and already from a

much earlier period than they suggest. An earlier chronology of the economic effects is fully compatible with the newest estimates of GDP per capita. These show that (a) structural, long-run economic growth in Western Europe started much earlier—in the Middle Ages—than assumed in the older literature; (b) growth in the medieval period was most evident in, first, Italy and, next, the Low Countries, that is, the areas where independent non- elite organizations were strongest and (c) growth stagnated where and when centralized, top-down organized states became strongest—e.g. France, Spain, after 1450—and where the originally bottom-up organizations were integrated into the dominant elite or started to manifest themselves as extractive violence specialists— Italy, after 1400 (Van Zanden 2009: 240–266; for the newest GDP figures: Bolt and Van Zanden 2014).

Our revision of the role and origin of organizations has consequences for the chronology and the location of the development of limited access orders into gradually emerging open access orders. NWW do not attempt to trace the historical development of limited access orders, since they feel that insufficient historical information is at their disposal (p. 18, note 22). Their discussion of different types of limited access orders with varying degrees of complexity is therefore largely time-invariant, and is based on a distinction between fragile, basic and mature orders (p. 41). Still, they do suggest a chronology, as mature limited access orders with more complex organizations, distinctions between public and private spheres and governments with monopoly control over violence in their view only developed in the 16th to 18th centuries, as in England and France (pp. 69–72). More generally, their story is very much focused on England, a country that progressed from a fragile order in the 11th century to a mature order in the early modern period and made the transition to becoming an open access order first, in the first half of the 19th century (pp. 77–109 and 213–219). Even though NWW stress that their story is not a teleological one, their description for England still breathes some of this air. The preceding discussion here, however, would require us to look more closely at developments elsewhere in Western Europe, where both the rise of independent, contractual organizations and economic growth occurred earlier than in England. It would also require us to focus less on the state level and government or king, and more on bottom-up movements and the organizations formed by ordinary people.

5.6   Main findings

Our paper follows *Violence and Social Orders*, by Douglass C. North, John Joseph Wallis and Barry R. Weingast in its focus on the need of societies to control large-scale,

organized violence. It concentrates on one of their social orders, the 'limited access order', that was dominant in most of history and still is today. In this order, the violence specialists within the elite coalition use their power to extract rents from the rest of the population, and use these rents to hold the coalition and the associated organizations together, whilst they restrict open violence. We follow NWW by discussing the elite as a composite entity and we follow them in their emphasis on the role of organizations.

Next, this paper tried to offer a better understanding of the incentives of the actors, the role of *organizations* in this process and its chronology. It does so by using a formal model, inspired by the conceptual approach by NWW and by including an explicit treatment of production. The results show that incentives to produce for the representative producer are a bottleneck for reaching high welfare levels in a limited access order. More specifically, in this order, high welfare levels are only possible with high levels of production and a low total appropriation rate. However, these two properties are trade-offs within the limited access order: high levels of the output elasticity of effort entail high rates of appropriation.

Introducing organizations into the model has large effects on outcomes, thus confirming the emphasis put by NWW on the role of organizations in development. We diverge from NWW, however, regarding their classification of organizations in limited access orders. Based on historical research, and contrary to NWW, we argue that these (contractual) organizations are not always founded by the elite and do not always rely on third-party enforcement and function with the explicit support of the state. This reading of the historical record has led us to introduce another type of organizations, the bottom-up ones, which are founded by non-violence specialists and shield parts of production from appropriation by violence specialists, aside from the top-down ones presented by NWW.

Making the distinction between organizations developed by the elite and bottom-up organizations, has important implications for the incentives of violence specialists and production levels. The organizations of the elite improve their position relative to warlords—the violence specialists operating outside the elite coalition—whereas organizations of producers are used to shield part of their production from appropriation by violence specialists. The two types of organization have a comparable impact in terms of increasing production, increasing welfare and decreasing appropriation rate, but there are also two important differences. First, top-down organizations facilitate an increase in the size of the elite coalition, whilst bottom-up organizations do not. Second, top-down organizations boost production but their impact reduces as organizations grow, whilst bottom-up organizations become more important for production and welfare as they grow.

These insights lead us to adjust the account and the chronology of developments discussed by NWW. In their account, they focus mainly on England, a country that progressed from a fragile limited access order in the 11th century to a mature limited access order in the early modern period and made the transition to open access in the first half of the 19th century. In this process, they argue, top-down, contractual organizations, linked to the state, played a main part. Other countries, including the United States and France, followed this path somewhat later.

We would say, however, that alongside this process, there was an important role for bottom-up organizations, including town communities and guilds, which developed all over Western Europe, including England, from the 11th century onwards. They operated at the local and regional level and functioned often to a large extent independently of the state, as seen most conspicuously in the Low Countries and Italy during the 13th to 15th centuries. This is exactly where, according to the newest GDP per capita estimates, economic growth in this period was most pronounced.

In the 15th to 18th centuries, connected to the rapid changes in military technology of the period, these independent bottom-up organizations largely lost out to the princely overlords and the state organizations. In Northern Italy, this happened due to the interference by the French and Spanish kings, showing how different limited access orders could interact across state boundaries; an aspect that in future research would deserve more attention. In this period, the bottom-up organizations largely lost their independence from the state and their leaders were often integrated into the dominant elites. Where this happened most conspicuously, as in France, Italy and Spain, the economy in this period most clearly stagnated. At that point, the Netherlands and England diverged from the rest of Western Europe. This divergence happened both in terms of economic development, with the two countries sustaining their high levels of welfare whilst other parts of Europe experienced low and declining welfare—Europe's little divergence discussed by Allen (2001)—as well as political development, with especially England becoming an open access society along the lines sketched by NWW. Even though the latter stages thus conform to the reconstruction of NWW, we would thus stress that bottom-up organizations played a crucial part in the maturation of limited access orders. More speculatively, we would suggest that the same was the case in the transition to open access orders in the 19th and 20th centuries, as there was a similar wave of formation of new and independent, bottom-up organizations, including trade unions, cooperatives, mutual insurance companies and political organizations, which fulfilled a similar role, not only at the local and regional but now also at the national level, within the framework of the nation state.

Our contribution thus strengthens the more abstract and conceptual reasoning

by NWW. It has, using a formal model, substantiated the insights gained by NWW, especially concerning the incentives of actors and the implications of organizations. Furthermore, our discussion of the historical record refines their treatment of organizations by demonstrating the prevalence of bottom-up organizations independent of the state that reduced the rate of appropriation by violence specialists. This leads us to suggest that the start of relevant developments within limited access orders in Western Europe must be dated earlier in time, more particularly in the 11th century.

Appendix

A   Appendix

In this Appendix, we first describe the maximization problem for the agents in our model and the equilibrium solution. We subsequently provide functional forms for the control function and the production function which we use to derive the results illustrated in Figures 5.1 and 5.2. Both functions satisfy the properties described in Section 5.2. We refer the reader to Van Besouw et al. (2016) or Chapter 4 for additional model details and a detailed equilibrium analysis.

In the model, violence specialists and the representative producer interact according to the three stages described in Section 5.2

1. Violence specialists choose their occupation, elite or warlord;

2. The elite coalition collectively decides on the tax rate;

3. The representative producer chooses its production level.

Using subgame-perfection, we solve the model backwards, with each agent maximizing his payoffs. The payoffs of elites and warlords depend on the share of total production they control, and the rate of appropriation they can impose over this share. Following from our assumption that violence specialists are homogeneous, total income of the elite (warlords) is distributed equally over all elite-members (individual warlords). The payoff of the representative producer depends on the share of his production that is not appropriated minus the cost of effort. The payoff functions for elites, warlords, and the representative producer are as follows:

$$\pi_{elite} = \frac{1}{e}\tau\rho(e,w)(1-\eta)Y(\phi)$$

$$\pi_{warlord} = \frac{1}{w}(1-\rho(e,w))(1-\eta)Y(\phi)$$

$$\pi_{producer} = (1-\tau)\rho(e,w)(1-\eta)Y(\phi)+\eta Y(\phi)-\gamma\phi$$

Parameter τ is the stage-2 tax rate, η is the share of output Y protected from appropriation by bottom-up organizations and parameter γ is the cost of effort to the representative producer. The variables are the number of elites e and the number of warlords w. What is left are the control function $\rho(e,w)$ and the production function $Y(\phi)$, where ϕ denotes effort.

The control function $\rho(e,w)$ takes two variables and two parameters, the decisiveness of conflict m and the cooperative quality of the elite θ, as inputs and gives the share of production controlled by the elite coalition as output. Given $m < 1$, there are diminishing marginal returns to group size. Given $\theta \geq 1$, the elite has an advantage over the warlords in their contest to control production:

$$\rho(e,w) = \frac{\theta e^m}{\theta e^m + w^m}$$

The production function $Y(\phi)$ takes one variable and two parameters, a linear technology parameter β and the marginal product of effort α, as inputs and gives the level of production by the representative producer as output. Note that this one-input production function is functionally equivalent to a Von Thünen production function, which in turn, is equivalent to a linearly homogeneous Cobb–Douglas production function with inelastic supply of labor (Lloyd 2001), with exponent $1-\alpha$ for labor and α for capital. However, our one-input model does not require these restrictive conditions on the production function as would be required for an extension to a two-input Cobb–Douglas function. Our specification allows us to focus on the representative producer's decision variable, effort ϕ which is elastically supplied:

$$Y(\phi) = \beta\phi^\alpha$$

A solution to this model follows from optimizing the decision variables, effort ϕ for producers and tax rate τ for elites, which yield response functions with which the model can be solved. For $\eta = 0$, that is, excluding the impact of bottom-up

organizations, Van Besouw et al. (2016) provide analytic results of this model. Key outcomes are the elite size and production level. The equilibrium elite size is a function of parameters α, θ and m, as well as parameter $V \equiv e + w$, equal to the (fixed) number of violence specialists:

$$e^* = \frac{V}{[(1-\alpha)\theta]^{\frac{1}{m-1}} + 1}$$

The equilibrium production level is a function of parameters α, β, γ, θ and m:

$$Y^* = \beta \left(\frac{\alpha^2 \beta}{\gamma}\right)^{\frac{\alpha}{1-\alpha}} \left(1 + (1-\alpha)[(1-\alpha)\theta]^{\frac{1}{m-1}}\right)^{\frac{\alpha}{\alpha-1}}$$

From these key model outcomes, other outcomes like producer payoff and the total appropriation rate can be derived. In the current paper, with $\eta \geq 0$, we choose to support our arguments graphically using numerical simulations. Our illustration of results in Figures 5.1-5.3 uses parameter values $\alpha = 0.6$, $\beta = 1.2$, $\gamma = 1$, $m = 0.5$, $V = 100$, $\eta = 0.1$ (left panels) and $\theta = 2$ (right panels).

Samenvatting

De oorsprong en verdeling van economische groei is een van de belangrijkste thema's binnen de economische geschiedenis. Het is duidelijk dat de economische structuur van een samenleving of regio, oftewel de manier waarop de productie en de allocatie van goederen en productiefactoren georganiseerd zijn, een bepalende rol speelt in deze vraag. Ook is duidelijk dat de economische structuur niet los gezien kan worden van diepliggende politieke verhoudingen in de samenleving—oftewel de 'politieke orde'. Tezamen zijn economische structuur en politieke orde in grote mate bepalend voor individuele welvaart, veiligheid en vrijheid. Ze zijn ook van groot belang in het ontstaan van ongelijkheden in deze zaken, en vervolgens voor hoe dergelijke ongelijkheden de economische structuur en politieke orde beïnvloeden. Desalniettemin bestaan er nog altijd enorme verschillen in individuele welvaart, veiligheid en vrijheid, zowel binnen moderne landen als tussen verschillende landen. Historisch gezien zijn individuele welvaart, veiligheid en persoonlijke vrijheid grotendeels 'recente' verschijnselen. Een belangrijke vraag is daarom hoe en waarom de economische structuur en politieke orde in sommige regio's veranderde op een manier waardoor deze uitkomsten mogelijk werden.

Deze dissertatie analyseert een aantal aspecten van de interactie tussen economische structuur en politieke orde door de economische gevolgen van oorlogsvoering in vroegmodern Noordwest-Europa te bestuderen. Ook al was oorlogsvoering uiterst destructief en de directe gevolgen ervan ontwrichtend, er zijn meerdere redenen om te veronderstellen dat oorlogsvoering positieve gevolgen kon hebben op economische structuur en politieke orde. Zo resulteerde oorlogsvoering in vroegmodern Europa volgens sommige recente studies in verstedelijking, een proces dat vervolgens tot veranderingen in economische activiteiten en veranderingen in lonen en prijzen van kapitaalgoederen leidde (Voigtländer en Voth 2013; Dincecco en Onorato 2016). Verstedelijking had ook mogelijke gevolgen op de politieke orde in vroegmodern Europa. Steden vormden een veel gevallen onafhankelijke politieke entiteiten die op sociaal, politiek en economisch vlak de strijd aangingen met grotere territoriale staten. Binnen territoriale staten vormden steden vaak een belangrijke rem op de macht van autocratische heersers (Stasavage 2011). Een derde belangrijke consequentie van oorlogsvoering was dat het staten dwong om zich efficiënter te organiseren. Succes in de oorlogen in vroegmodern Europa werd immers in grote mate bepaald door de omvang, bevoorrading en discipline van legers. Staten die meer en efficiënter belasting hieven en een betrouwbaar bestuursstelsel optuigden—en daardoor ook beschouwd werden als meer betrouwbare crediteurs—waren bijgevolg eerder succesvol tijdens oorlogen (Tilly 1992).

Vroegmodern Noordwest-Europa is bij uitstek een geschikte regio voor dit onderzoek. De vroegmoderne periode werd gekenmerkt door een veelvoud aan oorlogen. Daarnaast vonden er belangrijke verschuivingen in politieke orde en economische structuur plaats die door velen geassocieerd worden met de eerdergenoemde uitkomsten in termen van welvaart en vrijheden, zoals de opkomst van efficiënt georganiseerde overheden en parlementen. Dergelijke trends vormen echter nog geen bewijs voor het bestaan van positieve gevolgen van oorlogsvoering in de vroegmoderne tijd. In Noordwest-Europa bestond namelijk een grote variatie zowel in de mate van oorlogsvoering als in de uitkomsten ervan. Zelfs waar verstedelijking plaats vond of overheden beter georganiseerd werden—al dan niet als gevolg van oorlogsvoering—zijn de uitkomsten niet vanzelfsprekend. Sterkere staten of machtige steden konden namelijk even goed een rem vormen op economische activiteit en politieke vrijheid (Epstein 1991). Ook is het maar de vraag hoe de verwoestingen ten gevolge van oorlogen economisch hersteld konden worden, zeker in de gevallen waar staten veel belastingen besloten te heffen om zich voor te bereiden op de onvermijdelijke volgende oorlog.

Een obstakel in ieder onderzoek naar de gevolgen van oorlogsvoering is de sterke samenhang tussen de economische en politieke gevolgen van oorlogen. Bovendien spelen dezelfde politieke en economische factoren ook een grote rol in de manier van oorlogsvoering zelf. Welke staten oorlog voeren, waar en wanneer, en op welke manier, heeft immers ook te maken met bestaande politieke verhoudingen—tussen staten en steden, maar ook tussen staten onderling—en economische factoren—die zoals gezegd in grote mate bepalend zijn voor het verloop van de oorlogen zelf. Deze dissertatie bestaat daarom, naast een inleidend hoofdstuk, uit vier hoofdstukken die allen een specifiek element van de complexe interactie tussen oorlog, politieke orde en economische structuur uitlichten en bestuderen.

In Hoofdstuk 2, concentreer ik me, in samenwerking met Daniel Curtis, op de gevolgen van oorlogsvoering op sterfte van burgers en verschillen daarin tussen stad en platteland. Zoals eerder aangehaald bestaat een van de veronderstelde positieve effecten van oorlogsvoering uit het stimuleren van urbanisering. Dit effect vond plaats door directe migratie van het platteland naar de veiligere steden. Een andere reden voor migratie van platteland naar de stad was het relatief hoge loonpeil waarmee steden de hoge sterftegraad door ziektes konden goedmaken. Oorlogen waren mogelijk een belangrijke factor in de verspreiding van ziektes en zeker in de verspreiding van onveiligheid op het platteland. In beide gevallen zouden we dit terug moeten zien in een verhoging van het absolute sterftecijfer in de stad. Dit zou komen door ziektes en dus een hogere sterftegraad en door migratie en dus een grotere bevolking met bijgevolg meer sterfte.

De empirische bevindingen beschreven in Hoofdstuk 2 tonen geen verband tussen oorlogsvoering en verschillen in sterftecijfer tussen stad en platteland. Er is wel sprake van hoger sterftecijfer in de nabijheid van oorlogshandelingen in het algemeen, maar dit effect is beperkt in omvang. Onze resultaten zijn gebaseerd op een econometrische analyse van de gevolgen van nabijheid van oorlogshandelingen op veranderingen in het absolute aantal doden voor ruim 500 plaatsen in de zeventiende-eeuwse Lage Landen, waarbij we gebruik maken van een nieuwe data voor zowel oorlogshandelingen als sterftecijfers. Met andere woorden, de relatie tussen oorlogsvoering en verstedelijking wordt niet gesteund door onze analyse.

In Hoofdstuk 3 onderzoek ik hoe de destructieve gevolgen van oorlogsvoering hun weerslag vonden, maar ook opgevangen werden, in een kleine rurale economie. Dit hoofdstuk is gebaseerd op een gedetailleerde studie van het archief van de Abdij van Mariënweerd, gelegen in het Gelderse Rivierengebied, tijdens de eerste helft van de zestiende eeuw. De omgeving van de abdij werd gekenmerkt door competitieve pachtmarkten, marktgerichte veeteelt en door intense militaire strijd tussen de Hertog Karel van Gelre en Keizer Karel V tijdens de eerste drie decennia van de zestiende eeuw en met name in het midden van de jaren twintig van de eeuw. De economische gevolgen van de oorlogshandelingen zijn duidelijk terug te zien in de pachtsommen en -bepalingen. Desalniettemin waren de Abdij en haar pachters in staat om deze gevolgen het hoofd te bieden middels allerlei flexibele contracten.

In Hoofdstukken 4 en 5, beiden in samenwerking met Erik Ansink en Bas van Bavel, concentreren we ons meer op de relatie tussen oorlogsvoering en politieke orde. In Hoofdstuk 4 presenteren we een model voor de verhouding tussen economische activiteit aan de ene kant en politieke en militaire stabiliteit aan de andere kant. We modelleren een samenleving zonder neutrale staat met een geweldsmonopolie, maar met een kleine groep 'geweldsspecialisten'. Het model toont vooral een duidelijke spanning tussen productie en stabiliteit. Stabiliteit kan alleen gegarandeerd worden door het heffen van hoge belastingen door de geweldsspecialisten in ruil voor het garanderen van veiligheid. In het geval van lage belastingen gebruiken veel geweldsspecialisten hun capaciteiten opportunistisch—door middel van geweld en afpersing—waardoor productie gedrukt wordt. Deze bevinding benadrukt de gespannen verhouding tussen staatsvorming, oorlogsvoering en economische ontwikkeling.

In Hoofdstuk 5 concentreren we ons op een component van de economische structuur en politieke orde in vroegmodern Europa, te weten: formele organisaties. Het belang van dergelijke organisaties, zoals gilden, armentafels en dorpsraden, is al door veel historici benadrukt. In dit hoofdstuk bespreken we de rol van organisaties in de gespannen verhouding tussen staatsvorming, oorlogsvoering en economische

ontwikkeling—dit hoofdstuk bouwt voort op het model uit Hoofdstuk 4. Organisaties fungeerden in veel gevallen als een manier om 'geweldsspecialisten' op afstand te houden. Door hun vermogen en belangen te bundelen in formele organisaties konden gewone mensen vaak zelfs invloed uitoefenen op politieke besluitvorming en zich tot op zekere hoogte beschermen tegen allerlei vormen van uitbuiting—door staten, legerbendes en aristocraten. Dergelijk organisaties bestonden echter vaak al sinds de Middeleeuwen. De vroegmoderne tijd resulteerde vervolgens zeker niet in een algehele versterking van de positie van organisaties. In veel gevallen gingen oorlogsvoering en de groeiende macht van staten ten koste van zulke organisaties.

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