Pandemics and Political Development: The Electoral Legacy of the Black Death in Germany

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Abstract

Do pandemics have lasting consequences for political behavior? We address this question by examining the consequences of the most deadly pandemic of the last millennium: the Black Death (1347-1351). Our claim is that pandemics can influence politics in the long run if they impose sufficient loss of life so as to augment the price of labor relative to other factors of production. When this occurs, labor repressive regimes (such as serfdom) become untenable, which ultimately leads to the development of proto-democratic institutions and associated political cultures that shape modalities of political engagement for generations. We test our theory by tracing out the local consequences of the Black Death in German-speaking Central Europe. We find that areas hit hardest by the pandemic were more likely to: (1) adopt inclusive political institutions and equitable land ownership patterns; (2) exhibit electoral behavior indicating independence from landed elite influence during the transition to mass politics.

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1 Introduction

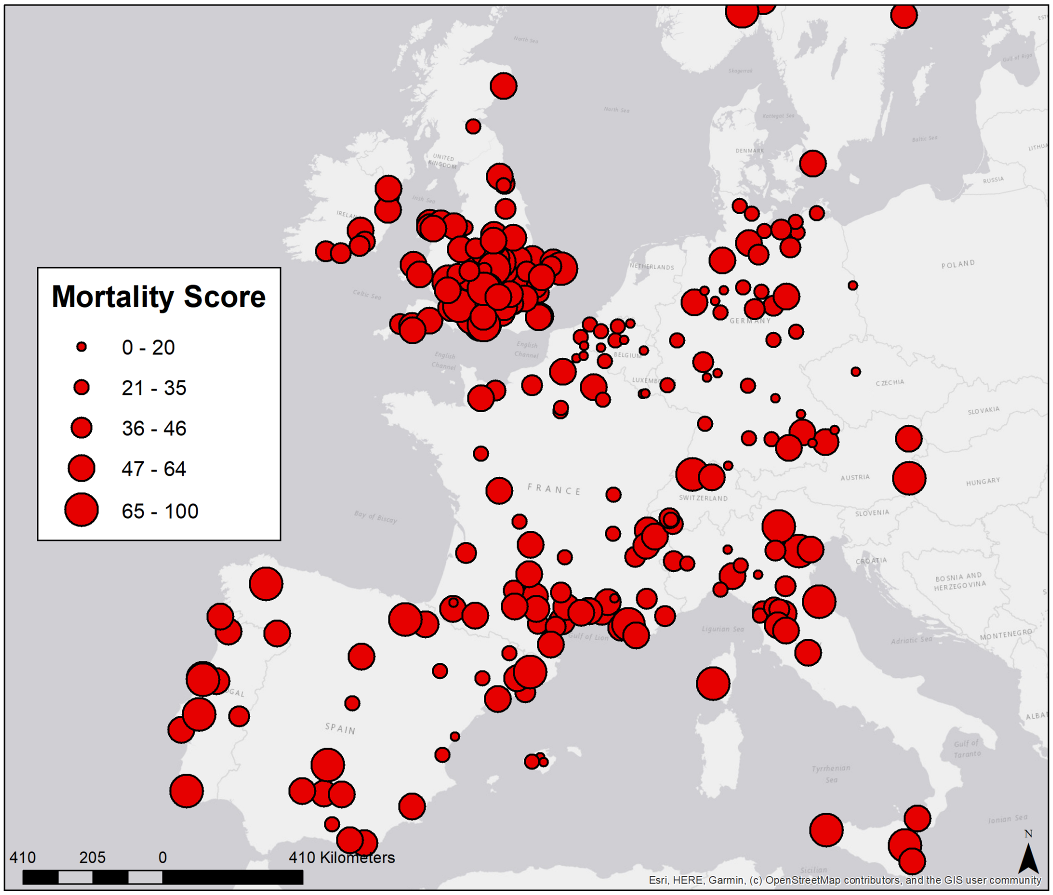
Pandemics have shaped the course of human history, felling tottering empires, influencing colonization patterns, and endowing populations with competitive advantages. In specific circumstances, they can also restructure labor markets, with potentially drastic consequences for inequality and social organization (Scheidel, 2017). Indeed, if the demographic shock imposed by a pandemic is sufficiently profound, it may fundamentally reconfigure the relative bargaining power of labor versus capital. This raises the possibility that pandemics may hold implications for the substance and conduct of politics in the long run.

This paper studies the long-term political impact of pandemic disease shocks by examining the localized consequences of the most deadly pandemic of the last millennium: the Black Death (1347-1351). The Black Death was an outbreak of plague that devastated Europe, resulting in a loss of life estimated at between thirty and sixty per cent of its total population. Figure 1 shows recorded outbreaks at the town level across the continent based on data by Jedwab, Johnson and Koyama (2019*a*).

Among its many consequences, the Black Death radically altered relative factor prices. By culling the labor force but leaving land and capital assets intact, it transformed labor from an abundant to a scarce resource. The economic consequences were immediate and long-lasting.[1] For Western Europe, the pandemic ushered in an era of higher real wages—lasting approximately 250 years—along with a lessening of the obligations imposed on peasants in the manorial economy (Hilton, 1969; Pamuk, 2007).

[1]The depth of the economic shock imparted by the Black Death may be unparalleled. Lead readings taken from an ice core in the Swiss-Italian Alps indicate that metal production during the Black Death outbreak was lower than at any other point in the last 2000 years of human history (More, Spaulding, Bohleber, Handley, Hoffmann, Korotkikh, Kurbatov, Loveluck, Sneed and McCormick, 2017).

Figure 1: Black Plague Death Sites Across Europe



The macro-level implications of the Black Death for economic development have been an object of inquiry for many years. Economic historians have argued that the Black Death led to the end of the Middle Age's so-called Malthusian trap, generating a shift from subsistence agriculture to economic production characterized by greater urbanization, increasing manufacturing capacity, technological development, and sustained growth (Herlihy, 1997; Postan, 1966; Voigtländer and Voth, 2013). These changes made possible the fiscal infrastructure necessary to support standing armies and create nation-states (North and Thomas, 1973). Given its epochal importance for economic organization, the Black Death is widely considered to have produced one of the most important critical junctures in recorded history. Indeed, it is thought to be the starting point for what ultimately became large divergences in development between Western and Eastern Europe (Acemoglu and Robinson, 2012) as well as Western Europe and China (Voigtländer and Voth, 2013).

Due to the pioneering data collection effort of Christakos, Olea, Serre, Wang and Yu (2005), the Black Death's local-level consequences have also recently become an object of scholarly inquiry. Researchers have traced out the long-run consequences of Black Death intensity for city growth (Jedwab, Johnson and Koyama, 2019*b*), the timing of the demographic transition (Siuda and Sunde, 2019), and the persecution of religious minorities (Finley and Koyama, 2018; Jedwab, Johnson and Koyama, 2019*a*). Others have examined the impact of plague shocks more generally on public goods institutions that shape the accumulation of human capital (Dittmar and Meisenzahl, 2019). These important advances notwithstanding, the Black Death's local-level consequences for political organization and behavior have yet to receive empirical scrutiny.

This is consistent with a general pattern of neglect within the discipline of political science. Despite the Black Death's prominent place in accounts of long-term economic development, it receives remarkably short shrift in treatments of the development of political representation and mass political behavior. For instance, the canonical investigation by Moore (1966) into the social origins of political regimes offers only a single passing reference to the Black Death (for the case of England); the foundational study by Rokkan (1970) of the origins of party politics in Europe ignores it entirely. The classic political histories of European state formation are similarly neglectful of the Black Death: Strayer (1973) and Tilly (1990) only mention it off-hand in general discussions of war and city growth. There are exceptions: Peters (2018) studies the consequences of credit market access for patterns of labor coercion in the aftermath of the Black Death. Yet consistent with earlier scholarship (Blum, 1957; Brenner, 1976), this work treats the Black Death as a uniform shock, concentrating its analyses on differences in initial conditions instead of the variegated impact of the disease.

We depart ways with existing scholarship by focusing systematically on the political implications of geographical variation in the loss of life caused by the Black Death. Specifically, using geocoded data on Black Death mortality rates, our paper examines the long-run socioeconomic and political consequences of localized Black Death exposure. We concentrate on the historical experience of Imperial Germany, tracing the consequences of the Black Death from the pre-Reformation period through the end of the 19th century.

The historical experience of the German cultural area is particularly apposite for evaluating the Black Death's long-term political consequences. There was significant regional variation in the mortality caused by the Black Death, making it feasible to identify distinct outcome patterns associated with differing levels of exposure to the outbreak. Equally important, there was no single, absolute ruler or other centralized political regime that governed the German-speaking territories. To the contrary, from the medieval period to the onset of Imperial Germany, German-speaking Europe was made up of a decentralized patchwork of principalities, duchies, free cities, and other administrative units. This high level of decentralization created a context in which local political cultures, borne from the initial reactions to demographic collapse, had sufficient space to implant themselves and become more distinctive over time.

Our central contention is that the long-lived regional political cultures attributable to the Black Death continued to shape patterns of political participation up until the early days of the German Empire's foundation. There are three steps in our argument.

First, differences in Black Death mortality led to differences in the persistence and depth of labor coercion during the early modern period (14/15th century to late 18th century). In areas where the Black Death hit hard, elites were forced to abandon serfdom for an incipient free labor regime. By contrast, in areas where the toll of the Black Death was relatively mild, customary labor obligations were maintained (or even amplified).

Second, regional differences in the use of labor coercion, in turn, led to a divergence in socioeconomic and political organization. In areas where serfdom receded, the new freedoms granted to laborers encouraged the development of institutions for (limited) local self-government, produced greater employment outside of agriculture, and led to greater equality in landholding. In areas where serfdom was maintained or became more onerous, the development of institutions for local self-government was inhibited, the agricultural economy remained dominant, and high levels of inequality in landholding persisted over time.

Finally, with the advent of mass electoral politics in the late 19th century, the societal conditions generated by the distinct legacies of labor coercion shaped the de facto autonomy of voters' electoral decisions. In the areas characterized by participatory institutions and relative equality, voters were inclined to reject the guidance of traditional elites, leading to weak support for conservative parties and stronger support for liberal parties. Contrariwise, in the areas characterized by less inclusive institutions and high inequality, voters were more inclined to defer to the directives of traditional elites, leading to strong support for conservative parties and weaker support for liberal parties.

Our empirical findings are consistent with these expectations. Using district-level electoral data from the 1871 legislative elections of Imperial Germany, we find that geographical variation in exposure to the Black Death is strongly and negatively related to the percentage of the vote won by the Conservative Party. Moreover, we find that areas least affected by the Black Death were characterized by societal conditions in which the Conservative Party was likely to thrive. In particular, we find that landholding inequality in the late 19th century was significantly greater in areas with mild exposure to the Black Death than in areas where it had a profound impact.

Additionally, we evaluate our claims about the proximate impact of the Black Death using outcomes from the pre-Reformation (pre-1517) period. We show that the intensity of Black Death exposure was positively associated with subsequent *changes* in political development. Specifically, we demonstrate that areas hit hard by the Black Death were more likely to experience the introduction of local participative elections from 1300 (pre-Black Death) to 1500 (post-Black Death) than areas that were not similarly affected. This gives us confidence that the Black Death encouraged the development of distinctive regional political traditions that shaped political behavior in the long run.

The remainder of this paper is organized as follows. First, we outline our contribution relative to existing studies of labor coercion and the long-run consequences of infectious diseases. Second, we provide a theory of how the Black Death impacted relative factor prices and the feasibility of labor coercion. Third, we introduce the empirical case and highlight the dimensions of greatest relevance to our study. Thereafter, we outline the framework of our empirical test. After a discussion of the results, we conclude and consider possible implications of our paper.

2 Pandemics, Factor Prices, and Labor Coercion

Pandemics impose death, sometimes at a massive scale. When a pandemic produces a major demographic collapse, it can also change relative factor prices: the economic returns to labor versus land and/or capital. This may lead to major changes in economic and political organization. It is widely appreciated that differences in factor prices shape economic inequality (Piketty, 2014; Piketty and Saez, 2014), which, in turn, affects both the incidence of democracy (Acemoglu and Robinson, 2006; Ansell and Samuels, 2014; Boix, 2003) as well as the quality of democratic representation (Bartels, 2008; Gilens, 2012; Uslaner, 2008).

In spite of the fact that factor prices are axes of social organization, pinpointing empirically how they shape political life can be challenging. As relative factor prices delimit the bargaining power of social groups, they both shape and are shaped by public policies (Beramendi and Anderson, 2008; Hall and Soskice, 2001; Rogowski, 1989). The same can be said for political institutions, which structure how public policies are made (Acemoglu, 2010; Persson and Tabellini, 2000).

Since the causal arrow relating factor prices to policies and institutions goes in both directions, isolating the influence of the former requires one to identify an appropriate exogenous shock. The Black Death offers a good historical example of such a shock. Caused by the bacterium *Yersinia pestis* and transmitted to humans by infected rat fleas (and subsequently via human-to-human contact in its pneumonic strain), the etiology of the Black Death was completely unknown to medicine at the time. Consequently, neither differences in rudimentary public health procedures nor preexisting levels of economic development appeared to determine its timing or intensity (cf. Christakos et al., 2005; Gottfried, 1983). Proximity to trade routes was clearly important, but *conditional on trade exposure* plague mortality was nearly random (Benedictow, 2004; Yue, Lee and Wu, 2017). Unlike contemporary pandemics, the Black Death did not overtly discriminate based on social status: it cut down both the wealthy and poor, claiming the lives of the King of Castile, large swathes of the clergy, and countless peasants. At the same time, the geographical intensity of the Black Death varied greatly.[2] These special features of the Black Death make it possible to discern

[2]The German cities of Bremen and Nuremberg illustrate this fact. Although both were roughly the same

the long-term influence of Black Death mortality, and, *ipso facto*, changes in relative factor prices, by employing a standard suite of econometric tools.

Our central claim is that by increasing the price of labor relative to land, Black Death mortality shaped patterns of labor coercion and the long-term development of local political cultures. Extant studies offer two competing approaches for thinking about the starting point of this argument: the effect of changes in factor prices on labor coercion.

The standard account can be classified as the theory of "Malthusian Exit." According to this view, shocks that generate a high level of labor scarcity (increasing labor's shadow price) catalyze a series of economic and social changes that move a society away from a subsistence economy based on labor coercion to one with manufacturing potential based on free labor (North and Thomas, 1973; Postan, 1966; Voigtländer and Voth, 2013). Specifically, the scarcity of labor improves the outside options of laborers, forcing elites to reduce coercive practices, which in turn creates greater and more variegated forms of consumption. As demand for manufactured goods increases, new technologies develop, urban areas expand, and the power of landed elites begins to wane. This theory is often invoked to explain Western Europe's development in the wake of the Black Death.

An alternative account can be classified as the theory of "Elite Reaction." In this account, elites respond to an increase in the scarcity of labor by doubling down on coercion (cf. Blum, 1957; Brenner, 1976; Domar, 1970). In particular, elites utilize greater amounts of coercion to arrest the wage increases and improvements in living standards that would otherwise follow a reduction in labor force size. The overall system of labor coercion remains in place, with labor obligations and the policing of labor becoming only more burdensome. The agrarian

size, Bremen lost between one half to two thirds of its population while Nuremberg only lost ten percent (Gottfried, 1983, 68).

economy remains supreme, technological innovation is suppressed, and the power of landed elites remains uncontested. This is the theory often invoked to explain the recrudescence of serfdom and underdevelopment in Eastern Europe in the wake of the Black Death.

In an important theoretical contribution, Acemoglu and Wolitzky (2011) present a framework integrating the mechanisms underlying both accounts. The framework's central implication is that the impact of labor scarcity on coercion depends on outside options of laborers versus the price of the landed elites' good. If the outside option effect dominates, then labor coercion will wane. However, if labor scarcity increases the value of the good produced by landed elites to a sufficiently high level, then coercion will become more intense.

Empirical studies that speak to the relative purchase of each theory are limited and offer contradictory findings.[3] In truth, much of the existing empirical work provides limited guidance for understanding the consequences of a labor market shock like that generated by the Black Death. This is because previous contributions are largely concerned with tracing out the consequences of variation in relative factor prices along the intensive margin, i.e. for small amounts of change within the respective society. The Black Death, by contrast, generated change along the *extensive* margin. Indeed, at an aggregate-level it was one of the largest labor market shocks in modern human history. As we will argue in the subsequent section, the depth of labor scarcity is important in understanding elite reaction to a labor supply shock. Reactions to minor shocks will not be the same as those to large ones.

The empirical findings of our paper about the long-term legacy of the Black Death contribute to a prominent literature on the economic and political consequences of infectious diseases. The incidence of infectious diseases has been tied to low levels of labor productivity

[3]For instance, the findings of Naidu and Yuchtman (2013) and Klein and Ogilvie (2017) are largely consistent with Elite Reaction theory, whereas those of Dippel, Greif and Trefler (2016) and Ardanaz and Mares (2014) are consistent with the logic of Malthusian Exit theory.

and investment, and ultimately to the emergence of 'poverty traps' in tropical areas (Bonds, Keenan, Rohani and Sachs, 2010; Gallup and Sachs, 2001; Sachs and Warner, 1997). Exposure to disease for populations in utero creates developmental disabilities that reduce levels of educational attainment, an important contributor to economic growth (Almond, 2006). In a long-term perspective, diseases may also determine the composition and behavior of the ruling elite. According to Diamond (1998), the immunological advantages conferred upon Europeans by living in proximity to livestock (and suffering through repeated disease waves) partially explain the ease with which they were able to conquer the Americas.

More directly related to the current paper, Acemoglu, Johnson and Robinson (2001) argue that the disease environment at the time of colonization determined the types of institutions colonizers implanted in their colonies, thereby shaping the quality of government and prospects for economic development in the long run. Our paper can be seen as a natural complement to theirs. Whereas they demonstrate that diseases can affect political development via the external imposition of institutions, we demonstrate that diseases can also catalyze processes of institutional change that are internal to societies.

Our paper is also linked to Sellars and Alix-Garcia (2018), who study how disease-driven demographic collapse in colonial Mexico shaped land tenure patterns. Contrary to the tenor of our findings for the Black Death, the authors find that landed estates in 1900 were more prominent in areas that had previously experienced a population collapse. We attribute the differences in our findings to distinct starting points: Whereas indigenous communities held substantial land in early colonial Mexico, the manorial economy (dominated by landed elites) was more or less a constant in medieval Europe. Given the uniform land ownership structure encountered in German-speaking Europe, it follows that the primary consequence of population decline would be an increase in the bargaining power of labor.

In examining how demographic change reshapes social and political organization in agrarian societies, our paper also contributes to the study of landed elite power and its implications for democracy. Historical investigations of political change have long emphasized that the economic and political power of the landed elite tends to delay or preclude the transition to democracy (Moore, 1966; Rueschemeyer, Stephens and Stephens, 1992; Ziblatt, 2008). Moreover, for countries that have already made the transition, the presence of a powerful landed elite fundamentally shapes the manner in which elections are contested.

Practices such as clientelism and vote brokerage are held to be especially effective in contexts in which landed elites employ a large segment of the labor force (Anderson, Francois and Kotwal, 2015; Scott, 1972; Stokes, Dunning, Nazareno and Brusco, 2013). Consequently, in agrarian settings with dominant landowners, voters are often induced to vote for the politicians that elites prefer, typically conservative politicians inclined to defend the extant property rights regime (Baland and Robinson, 2008; Gingerich, 2020; Gingerich and Medeiros, 2020; Mares, 2015). Our contribution to this literature is to endogenize the sources of landed elite power in a long-term historical perspective. Specifically, we show how exogenous shocks to the labor supply can undermine the landed elite's political influence. In so doing, we offer a novel account of the historical genesis of programmatic versus clientelistic linkages between citizens and politicians (cf. Kitschelt and Wilkinson, 2007).

3 The Long-Term Implications of Labor Supply Shocks for Electoral Behavior

In this section of the paper, we explicate the theoretical mechanisms tying labor supply shocks to long-run electoral behavior. Our starting point is the premise that the magnitude of the initial shock is crucial. Labor supply shocks that are sufficiently profound create a new institutional equilibrium that recasts the relationship between lord and peasant, producing more inclusive political traditions that, in the long run, structure mass political behavior. Labor supply shocks that are weaker lead to a retrenchment of socioeconomic hierarchies and obligations, producing exclusionary political traditions that also structure mass political behavior, albeit in a very different way.

Consider the relationship between labor supply shocks and labor coercion. If a demographic collapse radically reduces the labor supply, then this has two immediate consequences. First, the shadow price of the coerced worker's labor skyrockets. The economic returns to work outside the manor to which the laborer is bound become much greater, so the attractiveness of risking punishment by seeking employment elsewhere increases significantly. For the elites, keeping what remains of the labor force in place requires either an increase in wages (and a lessening of customary obligations) or greater investment in the monitoring and punishment of laborers. Given economies of scale in policing labor, the per-laborer cost of dissuading exit through coercion will be exorbitantly high. Thus, unless labor productivity increases immensely as a consequence of the shock, movement towards an incipient free wage regime will be seen by elites as the least detrimental option.

The second consequence of a negative labor supply shock concerns the prospects for coordination among agrarian elites. Given the reality of a decimated labor force, the competition among elites for laborers will be quite intense: Success or failure in poaching the labor of neighboring manors may mean the difference between bringing a crop to harvest or having it rot in the fields. Consequently, to keep wages low and laborers on their manors, elites must expend significant effort in creating and policing an anti-poaching cartel among themselves. The larger the shock, however, the greater the returns to each member of the elite from defecting from the cartel. Thus, for a sufficiently large shock, maintaining the anti-poaching cartel will be next to impossible. An incipient free wage regime emerges by default.

If the shock to the labor supply is relatively minor, then these dynamics will be very different. With only a moderate reduction in the labor force, the returns to laborers from fleeing their manors will be smaller and for elites the per-laborer cost of dissuading exit through coercion will be much more manageable. Moreover, given the smaller returns to elites from poaching the laborers of their peers, it will be feasible to sustain a cartel. Consequently, whereas large labor supply shocks will prompt an early exit from labor coercion, smaller shocks will be associated with its persistence.

The abandonment or persistence of labor coercion, in turn, has implications for economic, social, and political organization. In settings where labor coercion has diminished, the freedom of movement for laborers contributes to greater urbanization as well as a restructuring of relationships in the countryside. With greater urbanization and higher living standards comes the development of new technologies that jumpstart new forms of manufacturing (such as textile production or the production of books based on moveable type). Overall, the weight of agriculture in the economy diminishes. Agricultural production itself shifts away from the classic manorial model where land and property rights are vested solely in elites to one in which land rights become more widely shared. The roots of a system of small farming are established, and formerly gaping inequalities in landownership become more modest.[4] The improvements in employment opportunities and diversification of property rights naturally lead to a more variegated social structure and a populace characterized by greater heterogeneity of preferences. The new social groups, in turn, demand

[4]See Alfani (2015) and Alfani and Ammannati (2017) for direct evidence on the reduction in wealth inequality in the Piedmont and Tuscany regions of Italy following the Black Death.

channels for the representation of their interests. At the local level, this leads to the development of institutions such as elected town councils, providing for a (limited form) of self-government. Although traditional elites frequently enjoy initial veto power over such institutions, their very existence encourages non-elite coordination and demand-making (cf. Giuliano and Nunn, 2013). The seeds for autonomous political participation are thus sown.

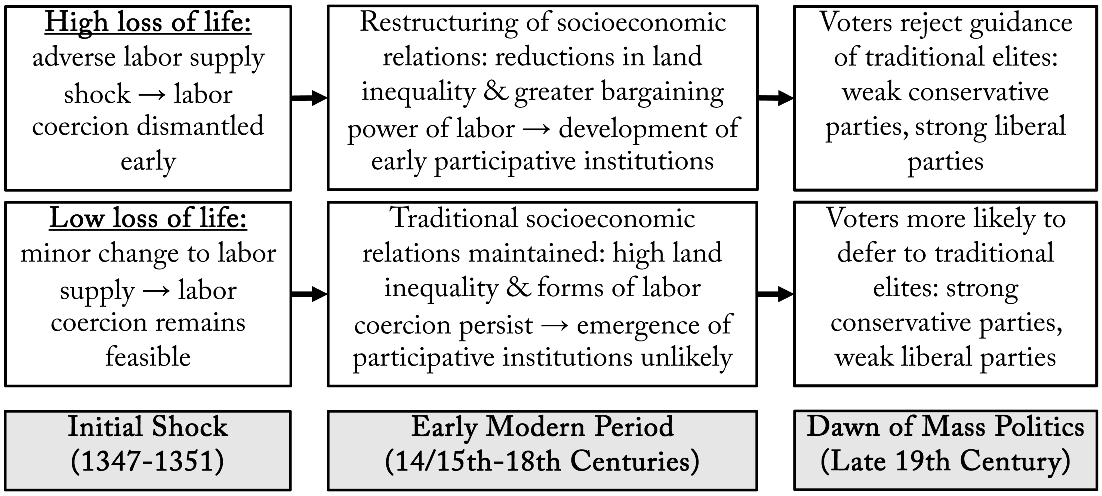
In settings where labor coercion persists unabated over a long period of time, the aforementioned occurrences do not come to pass. Rather, the basic tripartite Middle Ages class hierarchy—landed elite, clergy, peasants—remains fundamentally unaltered. Peasants remain tied to the land and urban areas are small and few and far between. The adoption of technological innovations, to the extent that these emerge from elsewhere, is actively discouraged by the traditional elites. Land tenure patterns evolve at a glacial pace, if they evolve at all. True political power remains vested in the landed aristocracy, which perpetuates its status through the use of enforcers deployed to police labor. The economy gravitates around agriculture, which in turn is dominated by a small number of large landholdings. Institutions designed to channel the demands of non-elite actors are unlikely to emerge, and if they do, they perish quickly. The great mass of the citizenry gains little or no experience in advocating for their own interests, and most certainly not in a way that might conflict with the desires of the agrarian elite. In this context, the prospects for autonomous political participation are dim.

The divergent paths of labor coercion that emerge in the wake of labor supply shocks create very different environments for the practice of electoral politics once the era of mass politics begins. Areas where labor coercion was dismantled early differ from those where it persisted over time in four crucial ways. First, early reforming areas have more differentiated economies, giving more voters viable employment opportunities outside of their current job.

As a consequence, they will not so easily be intimidated by employers who wish to sway their votes one way or another (cf. Frye, Reuter and Szakonyi, 2014; Mares, 2015). Second, the opportunities afforded to laborers in early reforming areas encourage greater human capital development, and in particular, higher levels of education. As a result, voters are more likely to be politically engaged, with greater awareness of what their political options are and a keener sense of how different contenders do or do not reflect their interests (Dee, 2004; Milligan, Moretti and Oreopoulos, 2004; Sondheimer and Green, 2010). Third, because of the legacies of labor coercion for urbanization, voters in early reforming areas are likely to be located in more densely populated communities than those in late reforming areas. Greater population density makes it more difficult for traditional elites to monitor and profit from clientelistic exchanges, thereby limiting the influence of material inducements on voting patterns (Brusco, Nazareno and Stokes, 2004; Gingerich and Medina, 2013). Finally, due to the erosion of traditional socioeconomic hierarchies in early reforming areas, voters in these areas are less likely to adhere to norms dictating deference to elites. Among such norms are norms of reciprocity, which have historically facilitated the ability of local elites to guide the electoral choices of voters (Finan and Schechter, 2012; Lawson and Greene, 2014).

To summarize, the societal context bequeathed by the early erosion of labor coercion is one where voters (1) have a clear sense of for whom they would prefer to vote; (2) enjoy the economic and cultural autonomy to vote as they wish. In contrast, the societal context bequeathed by the late or incomplete erosion of labor coercion is one where voters neither have strong preferences over contending political forces nor the wherewithal to resist the voting instructions of traditional elites. Figure 2 summarizes the theory.

Figure 2: Long-Term Consequences of the Black Death



4 Background on the Case of Germany

The subject of our empirical analysis is an area in Central Europe that, in the present-day, is mostly referred to as Germany. For nearly the entire time period under consideration (the 14th to 19th centuries), however, "Germany" remained politically fragmented. Because of the Holy Roman Empire's status as a confederation—as opposed to a centralized nation-state—Germany can also be understood as a *cultural entity*, united primarily by a common language and shared customs.

4.1 Rationale for Case Selection

There are two primary reasons for concentrating on this geographic area. The first is significant regional variation in the Black Death's intensity. Much of Germany's southwest and parts of the north were subject to devastating outbreaks while many towns and settlements in the easternmost parts were relatively unaffected.

The second reason is Germany's historically high level of political decentralization, allowing local traditions to persist over extensive time periods (Blanning, 2012; Frost, 2012; Wilson, 2002). In fact, Germany remained split into *hundreds* of principalities, city-states, kingdoms, and other administrative units. This combination makes Germany the ideal case for studying the pandemic's long-term effects. While in other countries, such as France and Britain, a central state was able to supplant local institutions, in Germany local political traditions had ample space to survive until the late 19th century.

4.2 Imperial Germany: Socioeconomic Conditions and Political Outcomes

In 1871, following the *Wars of German Unification* (1864-1871), Prussia united most of the German cultural region (excluding Austria) under a single political system known as "Imperial Germany" or the "German Empire." Based on our theory about the social transformation associated with the Black Death, we use this case to investigate variation in both (1) fundamental socioeconomic structures and (2) local political behavior. Since both outcomes reflect aspects of long-term political-economic equilibria affected by Black Death exposure, their combined analysis is of prime importance to our argument.

In terms of socioeconomic structures, we study an outcome that reflects economic power differentials in agrarian societies: the level of land inequality. This feature of society is often deeply rooted in historical events. Where land inequality is high, a small number of landholders have a disproportionate share of property in the agricultural sector, indicating that it is more elite-dominated. Such elite domination in the economic domain is often associated with elite domination in the political sphere (Ziblatt, 2008; Ziblatt, 2009).[5]

[5] Since the socioeconomic relevance of land inequality could be affected by the overall economic importance of agriculture, we account for this possibility in an extension to our empirical analysis (subsection A.6).

In terms of political outcomes, we consider voting behavior in elections of the Imperial Diet (*Reichstag*), the lower chamber of the Empire's legislature. Although it was less powerful than many present-day parliaments, its elections generated intense public interest. Importantly, the *formal* conditions of the elections (electoral rules, voting age, suffrage restrictions) were homogeneous across Germany, making it suitable for a cross-sectional analysis.[6] Two outcomes are of greatest interest to us: (1) the vote share that the Conservative party received in 1871 and (2) the number of electoral disputes between 1871 and 1912, with the latter indicating violations of electoral rules (typically by elites) and being studied extensively by Ziblatt (2009) and Mares and Zhu (2015).

We focus on the Conservative Party of the early 1870s because it was inherently elitist in both means and ends. Its stated goal was to defend traditional social structures, i.e. the privileged position of the landed elites. Moreover, in line with its historical roots, it turned against popular democracy, against socioeconomic changes caused by industrialization, and against national unification, as the latter was perceived to threaten the aristocracy (Anderson, 2000, Ch. 6; Berdahl, 1972, esp. 3-4, 18; Berdahl, 1988; Eley, 1986; Retallack, 1988, 13-14; Retallack, 2006).[7] Although the party ran in formally democratic elections, the landed elites used intimidation, clientelism, and the coerced mobilization of agricultural workers to improve their chances of victory (Anderson, 1993; Anderson, 2000, Ch. 6; Mares, 2015, Ch. 3-5; Nipperdey, 1961, Ch. 5).

As such tactics demonstrate, while formal electoral regulations were the same across Germany, local socioeconomic conditions and political norms varied significantly (Eley, 1986).

[6] Formally, all males aged 25 or above were allowed to give a direct and secret vote in a majoritarian single-member district electoral system.

[7] This was especially true for 1871, when the Conservative Party still endorsed an "estate society." Yet the incompatibility of the party's stance with capitalist development led it to give up on this goal, as expressed in the party's reorganization in 1876 as the "German Conservative Party" (Berdahl, 1972, 2-3).

This diversity also led to variation in the parties that ran across different districts (Sperber, 1997, 26, 114). In some districts, parties did not encounter the necessary socioeconomic conditions or political traditions to be viable competitors.[8] For the Conservative Party, the socioeconomic and political structures associated with high land inequality—reflecting an institutional equilibrium that facilitated abuses of power and undermined democratic elections—were a key factor determining its electoral viability (Anderson, 2000, Ch. 6; Ziblatt, 2008; Ziblatt, 2009). Yet where these conditions did not exist, the Conservative Party had little chance of succeeding in open electoral competition, leading to the absence of an appreciable party organization in such locations.

Considering the relationship of deeply-rooted political norms and socioeconomic circumstances with electoral outcomes is in line with previous scholarly work. Most importantly, Lepsius (1966) argues that parties in 19th-century Germany reflected "sociomoral milieus," which were themselves based in deeply rooted factors, such as culture, socioeconomic conditions, and political norms (Hübinger, 2008; Sperber, 1997, 3).[9] Importantly, this variation predated industrial society and the Empire's political system (Sperber, 1997, 4-5).

We focus on electoral outcomes in 1871 because politics in the following decades became more nationalized, impacting social attitudes and ultimately leading to the development of a national democratic culture (Anderson, 1993, 2000). This entailed a move away from the highly decentralized initial conditions. Additionally, after 1871, the second wave of industrialization took off in Germany and led to comprehensive social transformation (Hahn, 2011; Sperber, 1997, 5; Streb, Baten and Yin, 2006). The consequences included the rise of Social Democracy and a realignment of the party system (Berman, 2001, esp. 441-442,

[8]This is likely one of the reasons for why the Conservative Party chose to not nominate candidates in many more urban settings (cf. Nipperdey, 1961, 247).

[9]Variations in local culture and norms can persist over long time periods and shape political-economic outcomes (Acharya, Blackwell and Sen, 2018; Alesina and Giuliano, 2015; Vogler, 2019).

445-446; Sperber, 1997, esp. 7). In particular, the year 1890 is viewed as the turning point from more elite-centered politics to more popular politics (Sperber, 1997, 19).[10] For all these reasons, we focus on the 1871 election.[11]

5 Empirical Design

In this section, we describe the construction of our main independent variable, additional data we employ, and the specifications utilized in our empirical analysis.

5.1 Measuring the Intensity of the Plague: The *Black Death Exposure Intensity (BDEI) Score*

Since (1) the Black Death's impact varied widely across Central Europe and (2) its intensity represents our key explanatory variable, the construction of an appropriate measurement is of prime importance. To this end, we use data by Jedwab, Johnson and Koyama (2019*a*) on recorded outbreaks in European towns, which itself is primarily based on Christakos et al. (2005), to compute a measure of "Black Death Exposure Intensity" (the *BDEI score*).

While we have data on mortality rates for a number of individual medieval towns that we use to assess the Black Death's impact, our score is not simply a reflection of how intense the outbreak was in the nearest town only. Instead, it is a *composite* measurement, accounting for the extent to which the area *around* any specific location was affected. The key substantive reason for computing the score in this manner is as follows. Labor is a highly mobile factor of production. Accordingly, if the Black Death only has a minor impact or only hits a small number of locations in an area, labor supply can return to an old equilibrium more quickly

[10] For instance, as of 1890 all major parties began to become more active in rural areas that had previously been dominated by the landed elites (Eley, 1986).

[11] In terms of electoral disputes, however, we investigate a longer period as detailed in the next section.

due to regional market forces. But if many locations in an area are severely hit by an adverse shock at the same time, then returning to a previous equilibrium is much more difficult, even with a mobile production factor like labor.

Mathematically, the *BDEI score* represents the sum of recorded outbreak intensities inversely weighted by the distance to any specific location. The weighting is inverse (and exponentially decreasing) because outbreaks in the closest vicinity are most relevant.[12]

5.2 Imperial Germany: Outcome Variables

The analysis of outcomes in Imperial Germany is at the level of the electoral district. Based on our theory and the specific empirical case, we consider three main outcome variables that reflect distinct political-economic equilibria:

Socioeconomic Conditions

1. ***Land inequality (Gini coefficient):*** Data on land inequality are provided by Ziblatt (2009), who uses the Gini coefficient to compute a score between 0 (absolute equality) and 1 (absolute inequality).[13]

Political Outcomes

2. ***Conservative Party vote share (1871):*** Data on electoral outcomes are provided by Sperber (1997). These data reflect the Conservative Party's vote share in the 1871 elections.

[12] It is important to acknowledge that the underlying data we use to compute this score is imperfect as it simply does not cover every single European town. Nevertheless, the data's geographic distribution is in line with knowledge on the Plague's historical spread and they represent the best and most fine-grained measures currently available. Thus, our score provides a reasonable approximation to the Black Death's historical intensity at any given location. As a robustness check, we provide results based on two alternative datasets of outbreaks in the appendix (subsection A.11).

[13] Furthermore, in the appendix (subsection A.6), we provide results on the determinants of land inequality limited to contexts in which the agricultural workforce is large relative to the total workforce. See Mares (2015, 23-24, Ch. 4) for a directly related argument. Data on the agricultural workforce are by Reibel (2007), with Ziblatt (2009) offering a digitized version.

3. ***Net electoral disputes (1871-1912):*** Data on electoral disputes are by Arsenschek and Ziblatt (2008).[14] These data reflect the cumulative number of disputes that occurred in all peacetime elections.[15]

5.3 Imperial Germany: Control Variables

Controlling for factors that could affect both historical Black Death intensity and subsequent long-run political-economic outcomes is crucial. While the electoral districts of the Imperial Diet were a novel unit of organization (only going back to the census of 1864) for which there were no prior *unit-specific* historical data collected, several geographic features as well as historical levels of urban density are taken into account in our analysis. Our geographic controls in particular reflect the importance of trade in disease transmission: the Black Death spread through rats often transported by merchants and commercial ships.[16]

Specifically, our control variables are the following:

1. ***Urban density in 1300:*** Historical levels of urban density could influence both Black Death intensity and long-term political-economic outcomes. We use data by Wahl (2019) to compute a historical urban density score for each electoral district.[17]

2. ***Distance to the nearest major port:*** Not only did the Black Death spread through trade, closeness to major ports could also influence commerce and economic activity in the long run.

3. ***Distance to the nearest medieval trade city:*** For similar reasons as above, we also include distance to the nearest medieval trade city (Hribar, 2016).[18]

4. ***Distance to the ocean:*** While major ports were the primary centers of sea trade, there may have been a number of minor ports. Therefore, we include distance to the ocean as a proxy.

[14]Note that Ziblatt (2009) considers land inequality the key explanatory factor when it comes to electoral disputes. By contrast, we consider both outcomes to be part of long run political-economic equilibria that result from variation in Black Death intensity.

[15]The primary reason why we use the entire span of the existence of Imperial Germany is that this is a count variable. If we restrict the analysis to an individual year, we observe zero inflation.

[16]All geographic measures were computed in ArcGis or in R using data by GeoNames (2020).

[17]Similar to the *BDEI score's* construction, this measure reflects the sum of town *population sizes (log)* inversely weighted by their distance to the electoral district under consideration.

[18]See Wahl (2016*a*) for a detailed examination of the long-run influence of trade on economic development.

5. ***Distance to the nearest large river:*** Much trade took place on large, navigable rivers. Therefore, we include distance to the nearest large river (European Environment Agency, 2020).

6. ***Elevation***: Elevation could affect the accessibility of population centers to outsiders and animals carrying the plague (Bossak and Welford, 2016, 72), influencing both plague intensity and long-term political-economic outcomes.

5.4 Imperial Germany: Extensions

In the study's appendix, we present multiple extensions. In the first extension, we add covariates for *population size* and *Prussia*. In the second extension, we take into account a variable that reflects variation in the Reformation's long-term impact: an electoral district's *share of Catholics*. In the third extension, we calculate the *BDEI score* based on an alternative set of outbreak observations. In the fourth extension, we condition our analysis of *land inequality* on the relevance of agriculture in the district. In the fifth extension, we use the *timing of outbreaks* in a 2SLS setup to isolate quasi-random variation in mortality rates. This strategy is based on the observation that the Black Death was most severe in the spring and summer and that its intensity waned over time (Benedictow, 2004; Gottfried, 1983). In the sixth extension, we replace our absolute distance measures to geographic features with dummy variables. In the seventh extension, we control for variability in agricultural potential to account for historical information asymmetries (Ahmed and Stasavage, 2020). In the eighth extension, we include spatial fixed effects (Pepinsky, Goodman and Ziller, 2020). Finally, in the ninth extension, we use two alternative datasets of plague outbreaks to compute the *BDEI score* (Büntgen, Ginzler, Esper, Tegel and McMichael, 2012; Schmid, Büntgen, Easterday, Ginzler, Walløe, Bramanti and Stenseth, 2015).

5.5 Pre-Reformation Germany: Introduction of Participative Elections (1300-1500)

In addition to our primary analysis, we add a secondary set of empirical tests focused on changes in participative institutions at the town level between 1300 to 1500. These analyses are meant to evaluate empirical support for the transmission mechanisms outlined in our theory.

Here, we focus on a binary dependent variable based on data compiled by Wahl (2016*b*): *introduction of participative elections (1300-1500)*. This variable is equal to 1 for towns that newly adopted local participative elections during the 1300-1500 period; 0 otherwise.[19] Note that "participative elections" in medieval Germany did not refer to a participatory democracy with full voting rights for all citizens. Instead, such elections consisted of contests for the town council or other local offices, usually with limited public participation. That said, even these forms of "moderate" citizen participation are still indicative of important changes in political institutions and norms.[20]

Because our unit of analysis here is the town—an organizational unit that existed long before and after the time period that we investigate—additional control variables are available for different points in time for several units. Thus, we account for several socioeconomic and geographic factors that could have an impact on early democratic development.

Specifically, we include variables for (1) *elevation*, (2) *distance to the nearest river*, (3) *roman road in vicinity*, (4) *agricultural suitability*, (5) *population in 1300 (log)*, (6) *ruggedness*, (7) *urban potential (1300)*, (8) *trade city (1300)*, and (9) *proto-industrial city (1300)*.

[19]No towns with participative elections in 1300 discontinued these in 1500. Regardless of whether we include these towns, our substantive findings do not change.

[20]Further details on the underlying data and coding are provided by Wahl (2016*b*).

We draw these variables from Wahl (2019), which provides detail on coding procedures.

5.6 Empirical Specifications

We use a range of outcome variables with different properties and adjust our models accordingly. With respect to *land inequality* and *conservative vote share*, we primarily use OLS regression with clustered standard errors.[21] Since these two variables are truncated, i.e. they all have an upper and lower limit in their value, we also provide an alternative set of results using Tobit models in the appendix.

The format of our OLS regressions is the following:

$$y_i = \beta_0 + \beta_1 \; BDEI \; Score_i + \mathbf{x_i'} \, \boldsymbol{\beta} + \varepsilon_\mathbf{i} \tag{1}$$

where y_i is the respective outcome and $\mathbf{x_i}$ represents a vector of covariates at the electoral district level (i). β_1 represents the coefficient of the *BDEI score*.

The *BDEI score* is computed in the following way:

$$Raw \; BDEI \; Score_i = \sum_{j=1}^{n} LMR_j * (1 - DIST_{ji})^k \tag{2}$$

where $LMR_j \in (0, 1]$ is the local mortality rate at outbreak site j and $DIST_{ji} \in (0, 1]$ is the distance between i and j, which is used as the weight (with locations farther away from i being weighted down).[22] The parameter $k \in \{3, 6, 9, 12, 15\}$ for versions 1 through 5 of BDEI, respectively, represents the distance discount factor. We compute different versions of the *BDEI score* to demonstrate that results are not dependent on any single value of k. The further an outbreak site is from the location under consideration i, the more it is

[21] Errors are clustered at the level of the government district (*Regierungsbezirk*).

[22] The upper bound of 1 represents the maximum distance in the universe of cases under consideration.

exponentially discounted. In order to make the different versions of the *raw BDEI score* more comparable and our results easier to interpret, we standardize them to have a mean of $\mu = 0$ and a standard deviation of $\sigma = 1$.

Moreover, when considering net electoral disputes, which is a count variable, we use quasi-Poisson models. Quasi-Poisson models are based on regular Poisson regressions and begin with the following equation:

$$Pr(Y = y_i|\mu_i) = \frac{e^{-\mu_i}{\mu_i}^{y_i}}{y_i!}, \quad y_i = 0, 1, 2, \ldots \tag{3}$$

For each observation i, μ_i is determined by:

$$log(\mu_i) = \beta_0 + \beta_1 \ BDEI \ Score_i + \mathbf{x_i'} \ \boldsymbol{\beta} \tag{4}$$

The coefficient estimates (β) of the quasi-Poisson model are the same as in the standard Poisson model. Additionally, going beyond standard Poisson models, to account for variation in dispersion of the count variable, the quasi-Poisson adjusts the standard errors and p-values of the coefficients.

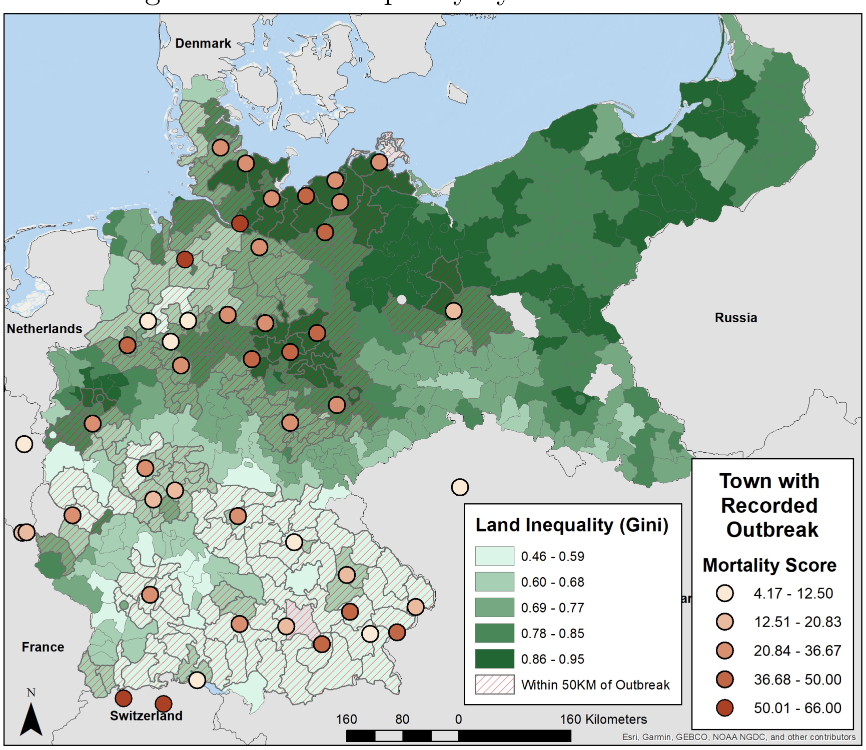
Finally, we use logistic regression when analyzing the binary variable *introduction of participative elections (1300-1500)*.

6 Results

6.1 Imperial Germany: Socioeconomic Conditions and Political Outcomes

The results of our empirical analysis reveal a strong relationship between the Black Death's historical intensity and long-term outcomes in Imperial Germany. We begin by considering a graphical overview of *land inequality* across Germany's electoral districts as provided in Figure 3.[23] The towns with recorded outbreaks are displayed as circles and the outbreaks' intensity is visible in the circles' color. The northeastern districts in particular exhibit high levels of land inequality. Additionally, almost all electoral districts in the easternmost parts, where the plague was least severe, have above-average levels of land inequality.

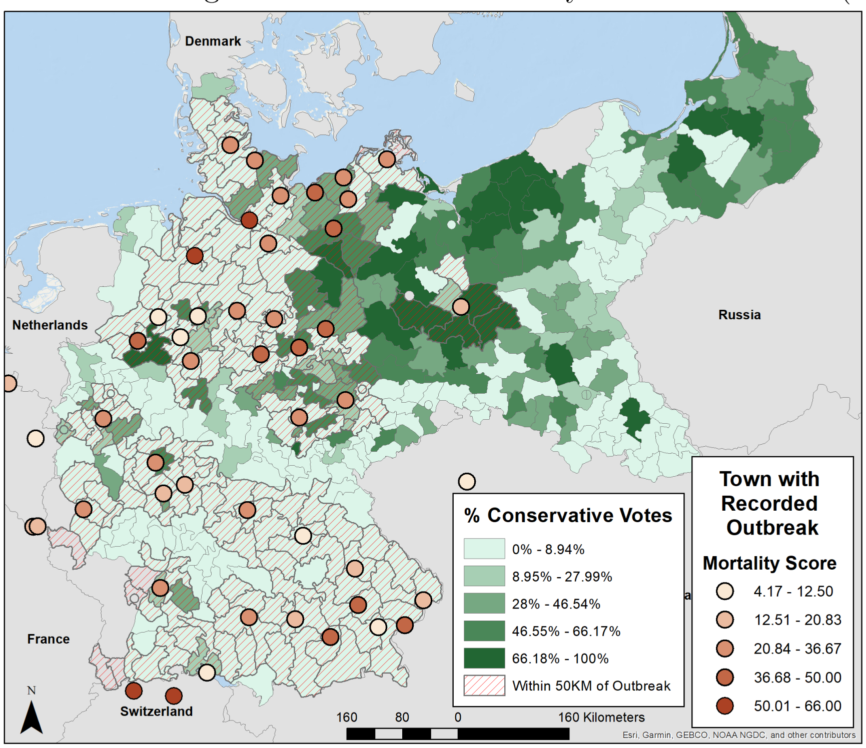
Figure 3: Land Inequality by Electoral District



[23]These maps are based on data by Nüssli and Nüssli (2008), Jedwab, Johnson and Koyama (2019*a*), Sperber (1997), and Ziblatt (2009).

As discussed earlier, we also expect a long-term impact of variation in Black Death intensity on *Conservative Party vote share*, with high vote shares indicating the political-economic equilibrium linked to high land inequality. This is clearly reflected in Figure 4. The party's vote share is systematically higher in areas with fewer and less intense recorded outbreaks. Importantly, as indicated earlier, in many places socioeconomic conditions (and associated political cultures) were such that the Conservative Party did not have a realistic chance to succeed in open electoral competition, as reflected by the absence of an appreciable local party organization and/or minimal vote shares.

Figure 4: Percentage of Conservative Votes by Electoral District (1871)



Next we turn to our regression analysis. Table 1 shows our findings with respect to *land inequality*. In addition to a first set of models (1-5) that are based on our key independent variable only, we provide a second set of models (6-10) that include the previously discussed controls. Across all specifications, the *BDEI score* has a significant negative impact on *land* *inequality*, indicating the Black Death's persistent influence on socioeconomic conditions. Specifically, a one standard deviation increase in the *BDEI score* results in a decrease in *land inequality (Gini)* that ranges from 0.047 to 0.061 (0.392 to 0.508 standard deviations). Figure 5 shows the marginal effects of *BDEI score v1*.

Table 2 shows the results with respect to *Conservative Party vote share*. As with the previous analysis, we also provide models without (1-5) and with (6-10) control variables. In line with our theory, the Conservative Party is weaker in areas that had more severe Black Death outbreaks, indicated by a high *BDEI score*. Specifically, a one standard deviation increase in the *BDEI score* leads to a reduction in the party's vote share ranging from 0.106 to 0.134 (0.426 to 0.538 standard deviations). The results are comparable to the above findings, highlighting the pandemic's long-term influence. Figure 6 shows the marginal effects of *BDEI score v1*.[24]

Finally, Table 3 shows the results of quasi-Poisson regressions on electoral disputes. Here we also find a result in line with our theoretical expectations: In places with more intense outbreaks, one encounters significantly fewer electoral disputes. Specifically, a one standard deviation increase in the *BDEI score* leads to a change in the logs of expected counts ranging from -0.172 to -0.254.

In short, we find comprehensive evidence that the Black Death shaped socioeconomic structures and local political behavior in the long run. Both in terms of landholding inequality and the Conservative Party's electoral viability, we find that regional variation in the intensity of plague outbreaks in the 14th century has strong predictive power for outcomes in the 19th century. These results indicate that this historical shock fundamentally reshaped

[24]With respect to both *land inequality* and *conservative vote shares*, additional Tobit models are in the appendix (subsection A.2).

society in areas where it hit hardest, while it did not alter (or even reinforced) socioeconomic and political hierarchies in other regions, leading to distinct institutional equilibria that persisted for generations. In the extensions located in the appendix, we find that our results are robust across a large set of alternative approaches to measurement and statistical analysis.

Figure 5: Marginal Effects Plot: BDEI Score v1 and Land Inequality (Gini)

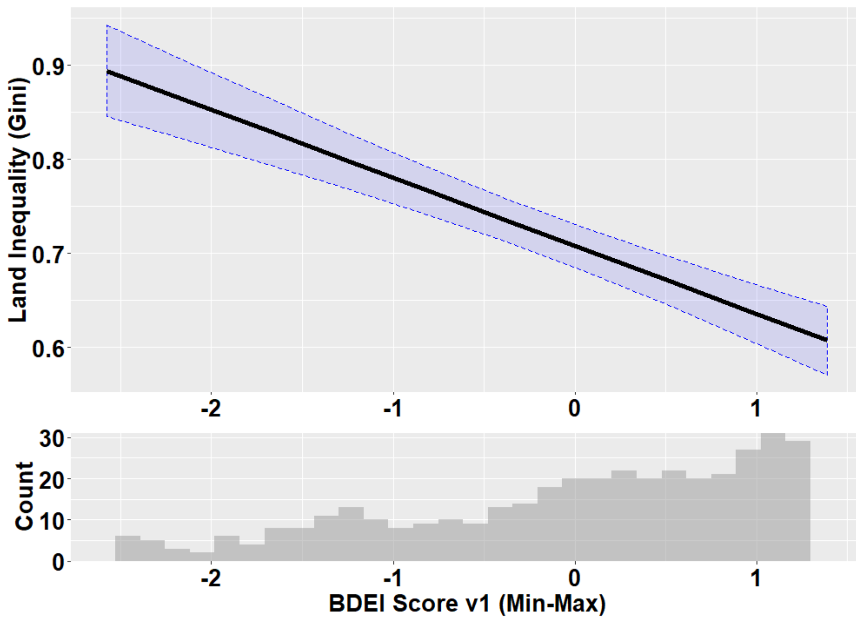


Figure 6: Marginal Effects Plot: BDEI Score v1 and Conservative Party Vote Share

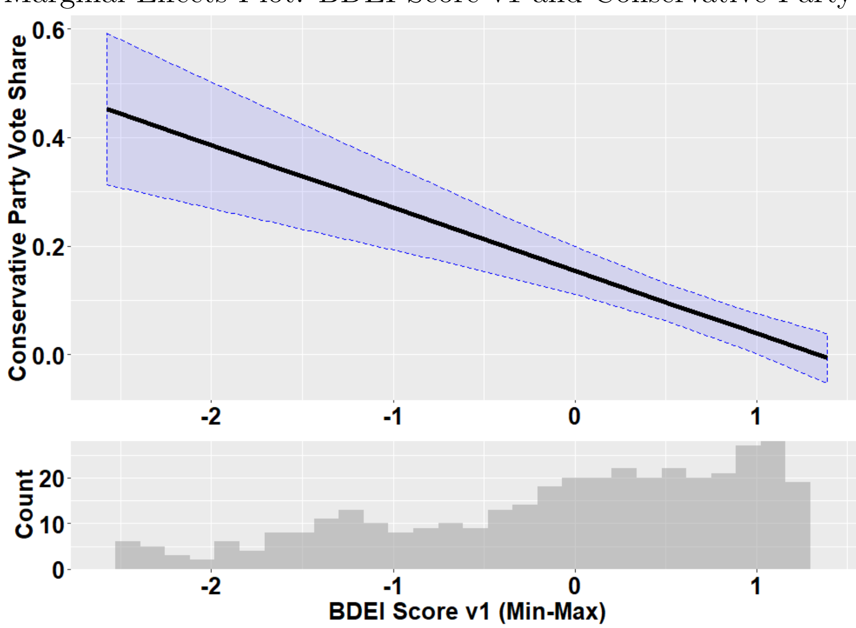


Table 1: Land Inequality (OLS)

	Dependent variable:									
	Land Inequality (Gini)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BDEI Score v1	−0.061***					−0.054***				
	(0.011)					(0.013)				
BDEI Score v2		−0.061***					−0.051***			
		(0.011)					(0.012)			
BDEI Score v3			−0.059***					−0.050***		
			(0.011)					(0.012)		
BDEI Score v4				−0.057***					−0.049***	
				(0.011)					(0.011)	
BDEI Score v5					−0.053***					−0.047***
					(0.011)					(0.011)
Urb. Dens. 1300						0.007	0.003	0.001	0.001	0.001
						(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
Dist. Maj. Port						−0.0004***	−0.0004***	−0.0004***	−0.0004***	−0.0005***
						(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Dist. Trade City						−0.00004	−0.00005	−0.00004	−0.00003	−0.00002
						(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Dist. Ocean						−0.00002	−0.00001	−0.00001	−0.00001	−0.00000
						(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.00005)
Dist. River						−0.0001	−0.0001	−0.0001	−0.0001	−0.0001
						(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Elevation						−0.0001***	−0.0001***	−0.0001***	−0.0001***	−0.0001***
						(0.00004)	(0.00004)	(0.00004)	(0.00005)	(0.00005)
Constant	0.726***	0.726***	0.726***	0.726***	0.726***	0.838***	0.838***	0.838***	0.839***	0.839***
	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Observations	397	397	397	397	397	397	397	397	397	397
R^2	0.260	0.255	0.243	0.223	0.193	0.632	0.633	0.631	0.625	0.616
Adjusted R^2	0.259	0.253	0.242	0.221	0.191	0.625	0.627	0.624	0.618	0.609

Note: Clust. SE — *p<0.1; **p<0.05; ***p<0.01

Table 2: Conservative Party Vote Share (OLS)

	Dependent variable:									
	Conservative Party Vote Share									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BDEI Score v1	−0.116***					−0.134***				
	(0.021)					(0.033)				
BDEI Score v2		−0.115***					−0.123***			
		(0.022)					(0.031)			
BDEI Score v3			−0.113***					−0.120***		
			(0.023)					(0.031)		
BDEI Score v4				−0.110***					−0.120***	
				(0.023)					(0.033)	
BDEI Score v5					−0.106***					−0.121***
					(0.024)					(0.036)
Urb. Dens. 1300						0.035	0.021	0.018	0.020	0.024
						(0.038)	(0.036)	(0.035)	(0.037)	(0.039)
Dist. Maj. Port						−0.0004**	−0.0004**	−0.0004**	−0.0004**	−0.0005**
						(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Dist. Trade City						0.0001	0.0001	0.0001	0.0002	0.0002
						(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Dist. Ocean						−0.0003*	−0.0003	−0.0003	−0.0003	−0.0003
						(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Dist. River						−0.0003	−0.0003	−0.0003	−0.0002	−0.0002
						(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Elevation						0.0001	0.0001	0.0001	0.0001	0.0001
						(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Constant	0.155***	0.154***	0.155***	0.155***	0.156***	0.245***	0.246***	0.247***	0.247***	0.247***
	(0.023)	(0.023)	(0.023)	(0.023)	(0.024)	(0.051)	(0.051)	(0.052)	(0.052)	(0.053)
Observations	382	382	382	382	382	382	382	382	382	382
R^2	0.212	0.208	0.202	0.193	0.180	0.299	0.297	0.294	0.292	0.288
Adjusted R^2	0.210	0.206	0.200	0.191	0.178	0.286	0.284	0.281	0.278	0.275

Note: Clust. SE *p<0.1; **p<0.05; ***p<0.01

Table 3: Net Electoral Disputes (Quasi-Poisson)

	Dependent variable:									
	Net Electoral Disputes									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BDEI Score v1	−0.200***					−0.254***				
	(0.051)					(0.092)				
BDEI Score v2		−0.200***					−0.235***			
		(0.055)					(0.086)			
BDEI Score v3			−0.196***					−0.231***		
			(0.056)					(0.084)		
BDEI Score v4				−0.186***					−0.231***	
				(0.057)					(0.084)	
BDEI Score v5					−0.172***					−0.226***
					(0.058)					(0.083)
Urb. Dens. 1300						0.047	0.020	0.015	0.020	0.024
						(0.074)	(0.067)	(0.065)	(0.066)	(0.069)
Dist. Maj. Port						−0.002***	−0.002***	−0.002***	−0.002***	−0.003***
						(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Dist. Trade City						0.0001	0.0001	0.0001	0.0002	0.0002
						(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Dist. Ocean						0.0003	0.0003	0.0003	0.0003	0.0003
						(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Dist. River						0.002	0.002	0.002	0.002	0.002
						(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Elevation						0.0002	0.0002	0.0002	0.0002	0.0002
						(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Constant	0.850***	0.850***	0.851***	0.853***	0.855***	1.019***	1.022***	1.025***	1.029***	1.035***
	(0.065)	(0.065)	(0.065)	(0.066)	(0.066)	(0.134)	(0.134)	(0.134)	(0.135)	(0.137)
Observations	397	397	397	397	397	397	397	397	397	397

Note: Quasi-Poisson, Clust. SE

*p<0.1; **p<0.05; ***p<0.01

6.2 Pre-Reformation Germany: Introduction of Participative Elections

Next we focus on a second set of analyses that examine pre-Reformation Germany. We study outcomes prior to the Protestant Reformation, which began in 1517, to rule out the possibility that it could be responsible for the outcomes observed in Imperial Germany. By showing that the Black Death is associated with key changes in proto-democratic institutions by 1500 (when compared to 1300), we demonstrate that some of the mechanisms discussed can be observed many years *before* the Reformation impacted Germany's political landscape.

Table 4 shows results for *introduction of participative elections (1300-1500)* for 325 towns. The results indicate that towns that were more strongly exposed to the Black Death were significantly more likely to adopt participative institutions by 1500.

Table 4: Introduction of Participative Elections (1300-1500)

	Dependent variable:				
	Introduction of Participative Elections (1300-1500)				
	(1)	(2)	(3)	(4)	(5)
BDEI Score v1	0.572***				
	(0.184)				
BDEI Score v2		0.527***			
		(0.174)			
BDEI Score v3			0.466***		
			(0.166)		
BDEI Score v4				0.397**	
				(0.161)	
BDEI Score v5					0.322**
					(0.159)
Constant	−1.836***	−1.821***	−1.802***	−1.782***	−1.764***
	(0.171)	(0.168)	(0.165)	(0.162)	(0.160)
Observations	325	325	325	325	325
Log Likelihood	−132.288	−132.751	−133.617	−134.660	−135.714
Akaike Inf. Crit.	268.575	269.501	271.235	273.319	275.428
Note: Logit			*p<0.1; **p<0.05; ***p<0.01		

In Table 5, we add a variety of control variables, including geographic factors. While the results are at or below the threshold of statistical significance in two specifications, the direction of the effect remains the same. Indeed, the lower level of significance is likely due to the much smaller number of cases for which covariate data is available. Overall, the evidence suggests that demographic collapse from the Black Death set in motion institutional changes that are consistent with the patterns of political behavior observed in the 19th century.

Table 5: Introduction of Participative Elections (1300-1500)

	Dependent variable:				
	Introduction of Participative Elections (1300-1500)				
	(1)	(2)	(3)	(4)	(5)
BDEI Score v1	2.203**				
	(1.030)				
BDEI Score v2		2.022**			
		(0.965)			
BDEI Score v3			1.751**		
			(0.890)		
BDEI Score v4				1.326*	
				(0.777)	
BDEI Score v5					0.861
					(0.646)
Elevation	−0.002	−0.001	−0.001	−0.001	0.0002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Distance to River	0.007	0.008	0.008	0.008	0.008
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Roman Road	−0.137	−0.060	0.098	0.310	0.466
	(1.045)	(1.030)	(1.014)	(1.003)	(1.000)
Coast	−18.130	−18.129	−18.050	−16.895	−16.721
	(2,355.184)	(2,381.540)	(2,407.436)	(1,472.045)	(1,479.105)
Agricult. Suit.	0.020	0.020	0.020	0.019	0.017
	(0.020)	(0.020)	(0.020)	(0.020)	(0.019)
Population (Log.)	−0.423	−0.403	−0.377	−0.355	−0.335
	(0.411)	(0.409)	(0.407)	(0.405)	(0.405)
Ruggedness	−0.064**	−0.064**	−0.063**	−0.061**	−0.058**
	(0.031)	(0.031)	(0.030)	(0.029)	(0.028)
Urban Potential 1300	−0.001	−0.001	−0.001	−0.0005	−0.0002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.0004)
Trade City 1300	−4.744	−4.618	−4.397	−4.068	−3.753
	(6.797)	(6.396)	(5.698)	(4.971)	(4.573)
Proto-Indust. City 1300	4.888	4.878	4.726	4.407	4.046
	(6.750)	(6.355)	(5.663)	(4.941)	(4.543)
Constant	9.264	8.204	6.583	4.502	2.593
	(5.827)	(5.522)	(5.118)	(4.661)	(4.279)
Observations	86	86	86	86	86
Log Likelihood	−28.884	−29.010	−29.306	−29.854	−30.468
Akaike Inf. Crit.	81.769	82.021	82.612	83.707	84.937

Note: Logit *p<0.1; **p<0.05; ***p<0.01

7 Conclusion

Contemporary social science emphasizes the importance of actions taken during critical junctures in explanations of differences in the nature, scope, and quality of government across societies (cf. Collier and Collier, 1991; Mahoney, 2001). As moments in time, critical junctures are defined by significant upheaval and fluidity (Capoccia, 2015): Institutional structures and social arrangements long taken for granted are suddenly amenable to changes that would have been inconceivable in normal circumstances. Such windows for change do not open easily. The antecedent to a critical juncture may be a shock that profoundly reorders economic circumstances and/or the balance of de facto power in a society (Roberts, 2002; Tarrow, 2017). Among the various types of shocks that may produce such an alteration in circumstances, demographic collapses due to pandemics surely number among the most consequential.

Our paper examined the long-term legacy of one of the most profound demographic shocks in European history: the loss of life due to the Black Death in the mid-14th century. Concentrating on the historical experience of the German-speaking areas of Europe from the arrival of the Black Death until the onset of the German Empire in 1871, the study explicitly laid out all four stages of analysis necessary for establishing the importance of a critical juncture (Collier and Munck, 2017): (1) characterization of the shock (i.e., the intensity of exposure to the Black Death); (2) the critical juncture itself (i.e., the decision to roll back or augment labor coercion); (3) the mechanisms of production of the legacy (i.e., changes in economic arrangements and political institutions resulting from changes in labor coercion); (4) the legacy (i.e., electoral behavior in the late 19th century).

Empirically, our paper shows that areas more intensely affected by the Black Death developed more inclusive political institutions at the local-level and more equitable ownership of land, both reflecting a fundamentally changed political-economic equilibrium. Contrariwise, those areas less affected by the Black Death maintained political institutions and land ownership patterns that concentrated political and economic power. In the first set of areas, voters in the late 19th century would come to reject the Conservative Party in electoral competition, an outcome indicative of autonomy of voters from the directives of the landed elite. In the second set of areas, voters would overwhelmingly cast their votes in favor of the Conservative Party, indicative of the ability of the landed elite to guide voters' decisions at the ballot box. By restructuring political institutions and social organization at the local level, the Black Death had significant consequences for how citizens would come to engage in mass politics.

What lessons does the Black Death offer about the potentially transformative role of pandemics more generally? One important lesson is that the depth of the shock matters. As the Black Death made its way through Europe, it imposed physical and emotional suffering of an incalculable magnitude, profoundly darkening the tenor of literature, music, and the visual arts. Yet in spite of the death and suffering associated with the disease outbreak, the world inherited by survivors and their descendants in areas ravaged by the Black Death was in many ways favorable to the world in which their ancestors had long toiled. Massive demographic collapse had improved the bargaining power of labor, leading to major changes in social organization and political institutions. These developments would improve living standards and provide opportunities for meaningful political engagement. In a dark twist of irony, the experience of the Black Death demonstrates that the long-term political independence of labor may have blossomed from the graves of workers.

As a general matter, however, one should not expect that pandemics will usually have these types of consequences. In order to radically restructure labor relations—the catalyst for the subsequent social and political changes wrought by the Black Death—a disease shock has to be very large, affect individuals in their prime working age, and not be easily reversible. Pandemics that infect great numbers of individuals but which have relatively low mortality rates—such as the Spanish Flu of 1918 or today's Covid-19 outbreak—do not change the labor supply sufficiently to fundamentally alter factor prices. The same is true for pandemics that have a high mortality rate but limited contagiousness, as was the case for HIV/AIDS prior to the widespread use of antiretroviral drugs. Diseases that primarily afflict children, such as measles and polio, also do not reconfigure relative factor prices—at least not in the long run—as fertility strategies may compensate for heightened mortality in children (cf. Hossain, Phillips and LeGrand, 2007).

To produce a labor market shock that generates dynamics comparable to that initiated by the Black Death, a pandemic would have to combine high contagiousness with high mortality for working age adults. The Ebola virus seemingly had this potential, but the recent development of a vaccine has thankfully reduced the threat to life posed by this disease. Although no obvious alternative threat lies on the horizon, the present combination of high population density and unprecedented global interconnectedness will surely make the next great pandemic all the more destructive when (not if) it does emerge. What the Black Death offers us, at the end, is an important reminder: When the next wave of destruction emerges, the particular set of labor repressive institutions of our contemporary era may be washed away in its wake.

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A Appendix

This appendix includes additional empirical evidence and further discussions of claims that were made in the main body of the paper. In subsection A.1, we provide summary statistics for electoral districts in Imperial Germany. In subsection A.2, we show the results for two outcome variables when using Tobit models instead of OLS regression. In subsection A.3, we include additional control variables that were not in the main analysis because they may be subject to post-treatment bias. In subsection A.4, we provide a second empirical response to the argument that the Reformation could be responsible for the observed patterns in 19th-century Germany. In subsection A.5, we exclude a number of observations when calculating the *BDEI score*. In subsection A.6, we provide results for a measure of land inequality conditional on the relevance of agriculture versus other sectors of the economy. In subsection A.7, we use the timing of Black Death outbreaks as an instrument in a two-stage least squares regression to more effectively isolate the quasi-random component of local Black Death intensities. In subsection A.8, we show results when using dummy variables instead of absolute distances to geographic features. In subsection A.9, we account for historical information asymmetries in agricultural production potential. In subsection A.10, we introduce spatial fixed effects to address the possibility of unobserved regional heterogeneity. In subsection A.11, we use two alternative datasets of Black Death outbreaks to check if our results hold when using a different set of underlying observations. In subsection A.12, we discuss some possible substantive issues related to the empirical design. Finally, in subsection A.13, we provide summary statistics for towns in pre-Reformation Germany.

A.1 Imperial Germany: Descriptive Summary Statistics

Table A1 shows descriptive summary statistics for electoral districts in Imperial Germany.

Table A1: Descriptive Statistics: Imperial Germany

Variable	**n**	**Min**	$\mathbf{q_1}$	$\bar{\mathbf{x}}$	$\widetilde{\mathbf{x}}$	$\mathbf{q_3}$	**Max**	**IQR**
BDEI Score v1	397	-2.57	-0.67	0.00	0.21	0.86	1.39	1.54
BDEI Score v2	397	-2.24	-0.74	0.00	0.16	0.86	1.45	1.61
BDEI Score v3	397	-2.11	-0.76	0.00	0.17	0.85	1.49	1.61
BDEI Score v4	397	-2.07	-0.78	0.00	0.28	0.82	1.45	1.59
BDEI Score v5	397	-2.05	-0.76	0.00	0.37	0.82	1.34	1.58
Landholding Inequality (Gini)	397	0.46	0.63	0.73	0.73	0.83	0.95	0.20
Conservative Party Vote Share (1871)	382	0.00	0.00	0.16	0.00	0.29	1.00	0.29
Net Electoral Disputes (1871-1912)	397	0.00	1.00	2.39	2.00	3.00	10.00	2.00
Urban Density (Standardized) (1300)	397	-3.06	-0.38	0.00	0.26	0.75	1.20	1.13
Distance to the Nearest Major Port (km)	397	0.00	59.32	164.59	141.50	255.86	475.98	196.54
Distance to the Nearest Medieval Trade City (km)	397	0.00	34.54	91.10	62.96	115.53	470.02	81.00
Distance to the Ocean (km)	397	0.00	90.90	221.87	214.97	348.16	582.91	257.25
Distance to the Nearest Large River (km)	397	0.00	0.00	34.37	20.45	55.72	157.30	55.72
Elevation	397	-15.00	65.00	221.51	158.00	330.00	979.00	265.00
Population Size (in 1000s)	391	32.06	91.67	103.30	104.40	114.34	208.00	22.67
Prussia	397	0.00	0.00	0.59	1.00	1.00	1.00	1.00
Proportion Catholic (1871)	397	0.00	0.02	0.37	0.23	0.73	1.00	0.71
Dummy Major Port ($\leq$ 10km)	397	0.00	0.00	0.08	0.00	0.00	1.00	0.00
Dummy Trade City ($\leq$ 10km)	397	0.00	0.00	0.11	0.00	0.00	1.00	0.00
Dummy Ocean ($\leq$ 10km)	397	0.00	0.00	0.13	0.00	0.00	1.00	0.00
Dummy River ($\leq$ 10km)	397	0.00	0.00	0.39	0.00	1.00	1.00	1.00
Caloric Variability	397	6.21	46.85	154.39	124.86	219.77	1449.68	172.93
BDEI Score v1 (Alternative Version)	397	-2.66	-0.72	0.00	0.17	0.78	1.60	1.50
BDEI Score v2 (Alternative Version)	397	-2.42	-0.75	0.00	0.22	0.77	1.66	1.52
BDEI Score v3 (Alternative Version)	397	-2.31	-0.68	0.00	0.28	0.77	1.63	1.45
BDEI Score v4 (Alternative Version)	397	-2.24	-0.67	0.00	0.36	0.73	1.54	1.40
BDEI Score v5 (Alternative Version)	397	-2.17	-0.70	0.00	0.44	0.76	1.41	1.46
BDEI Score v1 (2SLS)	397	-2.62	-0.69	0.00	0.22	0.86	1.34	1.56
BDEI Score v2 (2SLS)	397	-2.28	-0.77	0.00	0.19	0.88	1.38	1.65
BDEI Score v3 (2SLS)	397	-2.13	-0.75	0.00	0.20	0.88	1.36	1.64
BDEI Score v4 (2SLS)	397	-2.06	-0.78	0.00	0.28	0.88	1.32	1.66
BDEI Score v5 (2SLS)	397	-2.00	-0.82	0.00	0.33	0.83	1.24	1.65
BDEI Score v1 (Alt. Data 1) (Büntgen et al.)	397	-2.71	-0.73	0.00	0.16	0.82	1.53	1.55
BDEI Score v2 (Alt. Data 1) (Büntgen et al.)	397	-2.33	-0.81	0.00	0.11	0.82	1.72	1.62
BDEI Score v3 (Alt. Data 1) (Büntgen et al.)	397	-2.13	-0.84	0.00	0.08	0.81	1.88	1.65
BDEI Score v4 (Alt. Data 1) (Büntgen et al.)	397	-2.01	-0.87	0.00	0.06	0.79	2.02	1.66
BDEI Score v5 (Alt. Data 1) (Büntgen et al.)	397	-1.96	-0.88	0.00	0.11	0.76	2.16	1.64
BDEI Score v1 (Alt. Data 1) (Schmid et al.)	397	-2.86	-0.67	0.00	0.22	0.81	1.37	1.48
BDEI Score v2 (Alt. Data 1) (Schmid et al.)	397	-2.45	-0.76	0.00	0.13	0.84	1.53	1.60
BDEI Score v3 (Alt. Data 1) (Schmid et al.)	397	-2.19	-0.77	0.00	0.09	0.83	1.66	1.60
BDEI Score v4 (Alt. Data 1) (Schmid et al.)	397	-2.03	-0.83	0.00	0.09	0.84	1.76	1.67
BDEI Score v5 (Alt. Data 1) (Schmid et al.)	397	-1.92	-0.90	0.00	0.06	0.84	1.86	1.74

A.2 Imperial Germany: Tobit Models as an Alternative Specification

In our main empirical analysis we use OLS regression to estimate the impact of the *BDEI score* on *land inequality* and *Conservative Party vote share*. Because these two outcome variables are truncated, i.e. both have an upper and lower bound, we also use Tobit models as an alternative empirical specification.

Table A2 shows the results with respect to *land inequality* when using Tobit models. Furthermore, Table A3 shows the results with respect to *Conservative Party vote share* when using Tobit models. In both cases, the direction, magnitude, and significance of the coefficients do not change in a way that would alter our previous interpretation.

Table A2: Land Inequality (Tobit)

	Dependent variable:									
	Land Inequality (Gini)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BDEI Score v1	−0.061***					−0.054***				
	(0.005)					(0.006)				
BDEI Score v2		−0.061***					−0.051***			
		(0.005)					(0.006)			
BDEI Score v3			−0.059***					−0.050***		
			(0.005)					(0.006)		
BDEI Score v4				−0.057***					−0.049***	
				(0.005)					(0.006)	
BDEI Score v5					−0.053***					−0.047***
					(0.005)					(0.006)
Urb. Dens. 1300						0.007	0.003	0.001	0.001	0.001
						(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Dist. Maj. Port						−0.0004***	−0.0004***	−0.0004***	−0.0004***	−0.0005***
						(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Dist. Trade City						−0.00004	−0.00005	−0.00004	−0.00003	−0.00002
						(0.00005)	(0.00005)	(0.00005)	(0.00005)	(0.00005)
Dist. Ocean						−0.00002	−0.00001	−0.00001	−0.00001	−0.00000
						(0.00004)	(0.00004)	(0.00004)	(0.00004)	(0.00004)
Dist. River						−0.0001	−0.0001	−0.0001	−0.0001	−0.0001
						(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Elevation						−0.0001***	−0.0001***	−0.0001***	−0.0001***	−0.0001***
						(0.00003)	(0.00003)	(0.00003)	(0.00003)	(0.00003)
Constant	0.726***	0.726***	0.726***	0.726***	0.726***	0.838***	0.838***	0.838***	0.839***	0.839***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Observations	397	397	397	397	397	397	397	397	397	397
Log Likelihood	338.781	337.299	334.267	328.939	321.554	477.125	478.119	476.724	473.552	468.661

Note: Tobit — *p<0.1; **p<0.05; ***p<0.01

Table A3: Conservative Party Vote Share (Tobit)

	Dependent variable:									
	Conservative Party Vote Share									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BDEI Score v1	−0.229***					−0.284***				
	(0.026)					(0.043)				
BDEI Score v2		−0.230***					−0.261***			
		(0.026)					(0.040)			
BDEI Score v3			−0.227***					−0.255***		
			(0.026)					(0.039)		
BDEI Score v4				−0.220***					−0.258***	
				(0.026)					(0.040)	
BDEI Score v5					−0.210***					−0.263***
					(0.026)					(0.041)
Urb. Dens. 1300						0.092**	0.062*	0.055	0.063*	0.075*
						(0.040)	(0.037)	(0.037)	(0.038)	(0.039)
Dist. Maj. Port						−0.001***	−0.001***	−0.001***	−0.001***	−0.001***
						(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)
Dist. Trade City						−0.00001	−0.00002	0.00000	0.00005	0.0001
						(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Dist. Ocean						−0.001**	−0.001**	−0.001**	−0.001**	−0.001**
						(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Dist. River						−0.0003	−0.0002	−0.0002	−0.0001	−0.0001
						(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Elevation						0.0001	0.0001	0.0001	0.0001	0.0001
						(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Constant	−0.080***	−0.081***	−0.081***	−0.080***	−0.079**	0.189***	0.192***	0.195***	0.199***	0.206***
	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.061)	(0.061)	(0.061)	(0.061)	(0.061)
Observations	382	382	382	382	382	382	382	382	382	382
Log Likelihood	−213.050	−213.422	−214.693	−216.896	−219.913	−183.872	−184.398	−184.810	−185.072	−185.499

Note: Tobit *p<0.1; **p<0.05; ***p<0.01

A.3 Imperial Germany: Extension 1 — Including Additional Covariates

In the main body of the paper we did not include any political or social control variables specific to 19th-century Germany due to the possibility of introducing *post-treatment bias*. Nevertheless, in a limited number of cases, the inclusion of further controls from this time period may be justified due to their substantive or technical relevance. We elaborate on two specific instances below. Importantly, these results can only be seen as complementary to our main results, not as a substitute.

First, while most electoral districts were similar in population size (as they were based on the 1864 census), some were above or below the average, for example in cases in which migratory movements after 1864 had changed district sizes. Therefore, we control for the *population size* of electoral districts.

Second, historians often differentiate between Prussian and "non-Prussian" Imperial Germany, especially when it comes to electoral outcomes (Sperber, 1997, 29). Doing so would also be important for a substantive political reason: the Conservative Party originated in Prussia and did not have a sufficient party organization in many other parts of the country. In fact, in many areas, no comparable (conservatively-oriented) party was a viable competitor in elections. Of course, this is clearly *linked to* differing socioeconomic conditions and political norms/traditions that also were a long-term outcome of variations in Black Death intensities. Nevertheless, including a control variable for *Prussia* may be considered a more "conservative" empirical strategy.

The results we obtain can be found in Table A4. For the most part, they confirm previous findings and are in line with our theory. It is noticeable that Prussian districts experienced a significantly higher number of *electoral disputes* between 1871 and 1912.

Table A4: Extension 1: Including Additional Covariates

	Dependent variable:								
	Land Inequality (Gini)			Conservative Party Vote Share			Net Electoral Disputes		
	OLS			*OLS*			*glm: quasipoisson* *link = log*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BDEI Score v1	−0.052***			−0.144***			−0.270***		
	(0.013)			(0.029)			(0.091)		
BDEI Score v3		−0.047***			−0.128***			−0.243***	
		(0.011)			(0.028)			(0.083)	
BDEI Score v5			−0.044***			−0.128***			−0.231***
			(0.011)			(0.032)			(0.082)
Population	0.0004*	0.0004*	0.0003*	−0.001**	−0.001**	−0.001**	0.005**	0.005**	0.005**
	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Prussia	0.008	0.008	0.006	0.098**	0.095**	0.086*	0.283**	0.278**	0.267**
	(0.017)	(0.017)	(0.018)	(0.047)	(0.047)	(0.047)	(0.110)	(0.110)	(0.111)
Urb. Dens. 1300	0.006	−0.00003	−0.001	0.064*	0.044	0.048	0.117	0.081	0.084
	(0.012)	(0.011)	(0.011)	(0.037)	(0.036)	(0.038)	(0.073)	(0.064)	(0.068)
Dist. Maj. Port	−0.0004***	−0.0004***	−0.0005***	−0.0002	−0.0003	−0.0003*	−0.002***	−0.002***	−0.002***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)
Dist. Trade City	−0.0001	−0.0001	−0.00003	0.0003	0.0003	0.0003*	0.0004	0.0003	0.0004
	(0.00005)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)
Dist. Ocean	−0.00001	−0.00000	−0.00000	−0.0002	−0.0002	−0.0002	0.0005	0.001	0.001
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0004)	(0.0004)	(0.0004)
Dist. River	−0.0001	−0.0001	−0.0001	−0.0003	−0.0003	−0.0002	0.002*	0.002*	0.002*
	(0.0001)	(0.0001)	(0.0001)	(0.0004)	(0.0004)	(0.0004)	(0.001)	(0.001)	(0.001)
Elevation	−0.0001***	−0.0001***	−0.0001***	0.0001	0.0001	0.0001	0.0003	0.0003	0.0002
	(0.00004)	(0.00004)	(0.00005)	(0.0001)	(0.0001)	(0.0001)	(0.0004)	(0.0004)	(0.0004)
Constant	0.791***	0.793***	0.797***	0.281***	0.288***	0.302***	0.221	0.234	0.267
	(0.028)	(0.028)	(0.031)	(0.084)	(0.085)	(0.087)	(0.263)	(0.264)	(0.270)
Observations	391	391	391	376	376	376	391	391	391
R^2	0.634	0.633	0.617	0.334	0.328	0.319			
Adjusted R^2	0.625	0.624	0.608	0.318	0.311	0.302			

Note: Clust. SE *p<0.1; **p<0.05; ***p<0.01

A.4 Imperial Germany: Extension 2 — Accounting for a Potential Effect of the Reformation

In this extension, we provide a second empirical response to the argument that the Reformation—and not the Black Death—could be responsible for some of the variation we observe in Imperial Germany. During the Reformation, which began in 1517, many rulers of principalities across Germany turned away from the Catholic Church and towards Protestantism. Of course, the geographic pattern of the Reformation itself could partially be an outcome of variations in the intensity of the Black Death. While we have already demonstrated that key changes in political institutions at the town level *predate* the Reformation period, we include additional models that account for the *proportion of an electoral district's population that is Catholic* (based on data by Sperber (1997)). This control variable picks up differences between areas of Germany where Catholicism is strong and those where Protestantism is strong, which largely is a long-term outcome of the Reformation.

Table A5 shows the results of our extended analysis. The findings are again mostly in line with our theory and confirm previously obtained results. Only the effect of the *BDEI score* on *net electoral disputes* is no longer significant. However, as with extension 1, we caution the reader to carefully interpret these results due to the high likelihood of post-treatment bias.

Table A5: Extension 2: Accounting for a Potential Effect of the Reformation

	Dependent variable:								
	Land Inequality (Gini)			Conservative Party Vote Share			Net Electoral Disputes		
	OLS			*OLS*			*glm: quasipoisson link = log*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BDEI Score v1	−0.047***			−0.094***			−0.014		
	(0.015)			(0.034)			(0.103)		
BDEI Score v3		−0.042***			−0.081**			−0.016	
		(0.013)			(0.031)			(0.094)	
BDEI Score v5			−0.036***			−0.079**			−0.011
			(0.012)			(0.037)			(0.091)
Proportion Catholic	−0.018	−0.020	−0.031	−0.183***	−0.189***	−0.199***	−0.946***	−0.943***	−0.950***
	(0.022)	(0.021)	(0.021)	(0.057)	(0.058)	(0.058)	(0.191)	(0.188)	(0.184)
Population	0.0003*	0.0003*	0.0003*	−0.002***	−0.002***	−0.002***	0.004**	0.004**	0.004**
	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Prussia	0.011	0.010	0.010	0.123**	0.121**	0.118**	0.387***	0.386***	0.387***
	(0.018)	(0.018)	(0.018)	(0.048)	(0.049)	(0.050)	(0.114)	(0.114)	(0.114)
Urb. Dens. 1300	0.001	−0.004	−0.008	0.019	0.004	0.005	−0.103	−0.103	−0.106
	(0.013)	(0.012)	(0.012)	(0.034)	(0.031)	(0.032)	(0.082)	(0.070)	(0.071)
Dist. Maj. Port	−0.0004***	−0.0004***	−0.0004***	−0.00002	−0.0001	−0.0001	−0.001*	−0.001*	−0.001
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)
Dist. Trade City	−0.0001	−0.0001	−0.00005	0.0002	0.0002	0.0002	−0.00001	−0.00001	−0.00001
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)
Dist. Ocean	−0.00001	−0.00000	−0.00000	−0.0002	−0.0002	−0.0002	0.0005	0.001	0.001
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0004)	(0.0004)	(0.0004)
Dist. River	−0.0001	−0.0001	−0.0001	−0.0005	−0.0004	−0.0004	0.001	0.001	0.001
	(0.0001)	(0.0001)	(0.0001)	(0.0004)	(0.0004)	(0.0004)	(0.001)	(0.001)	(0.001)
Elevation	−0.0001***	−0.0001***	−0.0001***	0.0001	0.0001	0.0001	0.0005	0.0005	0.0005
	(0.00004)	(0.00004)	(0.00005)	(0.0001)	(0.0001)	(0.0001)	(0.0003)	(0.0003)	(0.0003)
Constant	0.794***	0.796***	0.801***	0.318***	0.323***	0.334***	0.374	0.374	0.377
	(0.028)	(0.028)	(0.030)	(0.079)	(0.079)	(0.081)	(0.259)	(0.260)	(0.266)
Observations	391	391	391	376	376	376	391	391	391
R^2	0.636	0.635	0.622	0.371	0.368	0.365			
Adjusted R^2	0.626	0.625	0.612	0.354	0.350	0.348			

Note: Clust. SE — *p<0.1; **p<0.05; ***p<0.01

A.5 Imperial Germany: Extension 3 — Using an Alternative Version of the BDEI Score

The formula on which the *BDEI score* is based automatically and exponentially discounts the weight of observations that are farther away from a location under consideration. Therefore, the observations in the immediate vicinity of Germany have by far the largest impact on the score, while the weight of observations that are farther away approaches zero.

In spite of the score's technical features and despite the fact that sea travel was often much more efficient than land travel (which justifies the general inclusion of observations from the British Isles in our calculations), we also present results based on an alternative *BDEI score* that systematically excludes all recorded outbreaks on the British Isles.

The results can be found in Table A6 and are substantively almost identical to previously obtained results, even when including control variables. The fact that the results remain largely unchanged in substantive terms indicates that the formula that is the basis of the *BDEI score* already sufficiently discounts observations at a greater distance, rendering their impact marginal.

Table A6: Extension 3: Alternative Version of the BDEI Score

	Dependent variable:								
	Land Inequality (Gini)			Conservative Party Vote Share			Net Electoral Disputes		
	OLS			*OLS*			*glm: quasipoisson* *link = log*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BDEI Score v1 (Alt.)	−0.066***			−0.148***			−0.351***		
	(0.017)			(0.043)			(0.126)		
BDEI Score v3 (Alt.)		−0.060***			−0.127***			−0.295***	
		(0.015)			(0.044)			(0.107)	
BDEI Score v5 (Alt.)			−0.048***			−0.117***			−0.224**
			(0.012)			(0.044)			(0.090)
Urb. Dens. 1300	0.017	0.014	0.006	0.047	0.034	0.031	0.117	0.081	0.038
	(0.015)	(0.013)	(0.012)	(0.045)	(0.045)	(0.046)	(0.095)	(0.084)	(0.077)
Dist. Maj. Port	−0.0003***	−0.0004***	−0.0004***	−0.0001	−0.0002	−0.0003*	−0.002***	−0.002***	−0.002***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)
Dist. Trade City	−0.00004	−0.00000	0.00001	0.0002	0.0002	0.0003	0.0001	0.0003	0.0003
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)
Dist. Ocean	−0.00000	0.00000	−0.00000	−0.0003	−0.0002	−0.0003	0.0003	0.0003	0.0003
	(0.00005)	(0.00005)	(0.00005)	(0.0002)	(0.0002)	(0.0002)	(0.0004)	(0.0004)	(0.0004)
Dist. River	−0.0002	−0.0001	−0.00004	−0.0004	−0.0003	−0.0001	0.001	0.002	0.002
	(0.0001)	(0.0001)	(0.0001)	(0.0005)	(0.0005)	(0.0004)	(0.001)	(0.001)	(0.001)
Elevation	−0.0001***	−0.0001***	−0.0001***	0.0002*	0.0002	0.0001	0.0004	0.0003	0.0002
	(0.00005)	(0.00005)	(0.00005)	(0.0001)	(0.0001)	(0.0001)	(0.0004)	(0.0004)	(0.0004)
Constant	0.814***	0.817***	0.828***	0.192***	0.201***	0.219***	0.897***	0.933***	0.990***
	(0.017)	(0.015)	(0.015)	(0.049)	(0.054)	(0.056)	(0.147)	(0.146)	(0.148)
Observations	397	397	397	382	382	382	397	397	397
R^2	0.627	0.623	0.602	0.273	0.262	0.260			
Adjusted R^2	0.621	0.616	0.595	0.259	0.248	0.246			

Note: Clust. SE — *p<0.1; **p<0.05; ***p<0.01

A.6 Imperial Germany: Extension 4 — Conditioning Land Inequality on the Size of the Agricultural Workforce

In this extension, we respond to the argument that *land inequality* is of the greatest socioeconomic relevance when agriculture is a key sector of the economy. The homogeneity/heterogeneity of an economy may influence the extent to which elites can use their dominant position in influencing voting patterns (Mares, 2015, 23-24, Ch. 4). Thus, we limit our analysis of land inequality patterns to districts that have a share of at least one third of workers engaged in the agricultural sector (Ziblatt, 2009). In other districts, where industry and services account for a greater share of labor force utilization, our measure of *land inequality* is less substantively meaningful.

We replicate all previous analyses with this new constraint and find that all our results still hold, with small changes to coefficient magnitudes. Table A7 shows these results for models without (1-5) and with (6-10) control variables.

Moreover, when applying Tobit models, as shown in Table A8, we also find results similar to previous Tobit regressions.

Table A7: Extension 4: Conditioning Land Inequality on the Size of the Agricultural Workforce

	Dependent variable:									
	Land Inequality (Gini)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BDEI Score v1	−0.072***					−0.066***				
	(0.009)					(0.013)				
BDEI Score v2		−0.073***					−0.063***			
		(0.009)					(0.012)			
BDEI Score v3			−0.072***					−0.061***		
			(0.009)					(0.012)		
BDEI Score v4				−0.069***					−0.060***	
				(0.009)					(0.011)	
BDEI Score v5					−0.065***					−0.057***
					(0.009)					(0.011)
Urb. Dens. 1300						0.012	0.007	0.005	0.005	0.005
						(0.012)	(0.010)	(0.010)	(0.010)	(0.011)
Dist. Maj. Port						−0.0004***	−0.0004***	−0.0004***	−0.0004***	−0.0004***
						(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Dist. Trade City						−0.00005	−0.00005	−0.00004	−0.00004	−0.00002
						(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Dist. Ocean						−0.00003	−0.00002	−0.00002	−0.00001	−0.00001
						(0.00004)	(0.00004)	(0.00004)	(0.00004)	(0.00004)
Dist. River						−0.0001	−0.0001	−0.0001	−0.0001	−0.0001
						(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Elevation						−0.0001***	−0.0001***	−0.0001***	−0.0001***	−0.0001***
						(0.00004)	(0.00004)	(0.00004)	(0.00004)	(0.00004)
Constant	0.708***	0.708***	0.708***	0.708***	0.708***	0.816***	0.817***	0.817***	0.818***	0.819***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)
Observations	307	307	307	307	307	307	307	307	307	307
R^2	0.389	0.389	0.377	0.351	0.312	0.712	0.715	0.712	0.703	0.690
Adjusted R^2	0.387	0.387	0.375	0.349	0.309	0.706	0.708	0.705	0.696	0.683

Note: Clust. SE | *p<0.1; **p<0.05; ***p<0.01

Table A8: Extension 4: Conditioning Land Inequality on the Size of the Agricultural Workforce (Tobit)

	Dependent variable:									
	Land Inequality (Gini)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BDEI Score v1	−0.072***					−0.066***				
	(0.005)					(0.007)				
BDEI Score v2		−0.073***					−0.063***			
		(0.005)					(0.006)			
BDEI Score v3			−0.072***					−0.061***		
			(0.005)					(0.006)		
BDEI Score v4				−0.069***					−0.060***	
				(0.005)					(0.006)	
BDEI Score v5					−0.065***					−0.057***
					(0.006)					(0.007)
Urb. Dens. 1300						0.012*	0.007	0.005	0.005	0.005
						(0.007)	(0.007)	(0.006)	(0.007)	(0.007)
Dist. Maj. Port						−0.0004***	−0.0004***	−0.0004***	−0.0004***	−0.0004***
						(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Dist. Trade City						−0.00005	−0.00005	−0.00004	−0.00004	−0.00002
						(0.00005)	(0.00005)	(0.00005)	(0.00005)	(0.00005)
Dist. Ocean						−0.00003	−0.00002	−0.00002	−0.00001	−0.00001
						(0.00004)	(0.00004)	(0.00004)	(0.00005)	(0.00005)
Dist. River						−0.0001	−0.0001	−0.0001	−0.0001	−0.0001
						(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Elevation						−0.0001***	−0.0001***	−0.0001***	−0.0001***	−0.0001***
						(0.00003)	(0.00003)	(0.00003)	(0.00003)	(0.00003)
Constant	0.708***	0.708***	0.708***	0.708***	0.708***	0.816***	0.817***	0.817***	0.818***	0.819***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)
Observations	307	307	307	307	307	307	307	307	307	307
Log Likelihood	289.820	289.667	286.787	280.515	271.454	405.362	406.750	404.992	400.659	393.875

Note: Tobit — *p<0.1; **p<0.05; ***p<0.01

A.7 Imperial Germany: Extension 5 — Two-Stage Least Squares Models

In our main analysis, we include a large number of geographic variables and *urban density in 1300* to account for factors that could influence both local Black Death intensities and long-term political-economic outcomes. Despite our comprehensive attempts to control for these geographic variables, it would be desirable to more rigorously isolate the quasi-random component of Black Death intensities. In this respect, we follow a similar strategy as Jedwab, Johnson and Koyama (2019*b*), who use the timing of the onset of the Plague to predict mortality rates in an instrumental-variable framework.

Similarly, as shown in Table A9 we use a combination of quarterly and yearly dummy variables to predict *local mortality rates* (LMR). The first-stage regressions show two interesting patterns. First, outbreaks that began in the second quarter (April, May, June) led to the highest mortality rates. Second, places that were hit in later years had significantly lower mortality rates. These findings are fully consistent with the observations of historians that (1) the Black Death was most severe when it was able to spread in the late spring and summer months and (2) the overall intensity of the Plague decreased over time (Benedictow, 2004; Gottfried, 1983).

In a second step, we compute a new *BDEI score* based on the predicted rather than the actual values of local mortality rates. The results of the analysis for this second-stage *BDEI score* are in Table A10. The estimated effects of Black Death intensity are statistically significant and similar in magnitude to those reported in the main text. To the degree there is any change, the estimated impacts of the Black Death based on the 2SLS procedure is slightly larger for *Conservative Party vote shares* and *net electoral disputes* than the original OLS estimates.

Table A9: Predicting Outbreak Intensity Based on Timing

	Dependent variable:
	Local Mortality Rate (LMR)
First Quarter	0.035
	(0.038)
Second Quarter	0.087^{**}
	(0.035)
Third Quarter	-0.024
	(0.037)
1348	-0.157^{**}
	(0.061)
1349	-0.215^{***}
	(0.063)
1350	-0.301^{***}
	(0.069)
Constant	0.584^{***}
	(0.053)
Observations	178
R^2	0.188
Adjusted R^2	0.160
Note: OLS	*p<0.1; **p<0.05; ***p<0.01

Table A10: Extension 5: Using a Two-Stage Regression Approach

	Dependent variable:								
	Land Inequality (Gini)			Conservative Party Vote Share			Net Electoral Disputes		
	OLS			*OLS*			*glm: quasipoisson* *link = log*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BDEI Score v1 (2SLS)	−0.057***			−0.140***			−0.271***		
	(0.014)			(0.035)			(0.098)		
BDEI Score v3 (2SLS)		−0.054***			−0.126***			−0.240***	
		(0.012)			(0.033)			(0.090)	
BDEI Score v5 (2SLS)			−0.054***			−0.129***			−0.224**
			(0.011)			(0.037)			(0.089)
Urb. Dens. 1300	0.010	0.006	0.007	0.041	0.025	0.031	0.062	0.026	0.022
	(0.013)	(0.011)	(0.011)	(0.039)	(0.038)	(0.041)	(0.079)	(0.069)	(0.071)
Dist. Maj. Port	−0.0004***	−0.0004***	−0.0005***	−0.0003*	−0.0004**	−0.0005***	−0.002***	−0.002***	−0.003***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)
Dist. Trade City	−0.00005	−0.00005	−0.00003	0.0001	0.0001	0.0002	0.0001	0.0001	0.0002
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)
Dist. Ocean	−0.00001	−0.00000	−0.00000	−0.0003*	−0.0003	−0.0003	0.0003	0.0003	0.0003
	(0.0001)	(0.00005)	(0.00005)	(0.0002)	(0.0002)	(0.0002)	(0.0004)	(0.0004)	(0.0004)
Dist. River	−0.0001	−0.0001	−0.00003	−0.0003	−0.0002	−0.0001	0.002	0.002	0.002
	(0.0001)	(0.0001)	(0.0001)	(0.0004)	(0.0004)	(0.0004)	(0.001)	(0.001)	(0.001)
Elevation	−0.0001***	−0.0001***	−0.0001***	0.0001	0.0001	0.0001	0.0003	0.0003	0.0002
	(0.00004)	(0.00004)	(0.00004)	(0.0001)	(0.0001)	(0.0001)	(0.0004)	(0.0004)	(0.0004)
Constant	0.834***	0.836***	0.839***	0.236***	0.242***	0.248***	1.002***	1.015***	1.031***
	(0.015)	(0.015)	(0.014)	(0.050)	(0.051)	(0.051)	(0.135)	(0.135)	(0.138)
Observations	397	397	397	382	382	382	397	397	397
R^2	0.633	0.636	0.629	0.297	0.294	0.293			
Adjusted R^2	0.626	0.629	0.623	0.284	0.281	0.280			

Note: Clust. SE

*p<0.1; **p<0.05; ***p<0.01

A.8 Imperial Germany: Extension 6 — Using Dummy Variables Instead of Absolute Distances to Geographic Features

In our main regression analysis, we use absolute distances to several geographic features (such as the ocean or large rivers) to account for variation in proximity to trade routes. An alternative approach is to use dummy variables that indicate if a feature is within a certain distance. This approach is motivated by the possibility that areas in close vicinity to the ocean or a large river could be disproportionately affected by trade levels. Accordingly, in this extension, we replace all absolute distance measures with dummy variables indicating if any of our original geographic features are located at a distance of 10 km or less from the electoral district. The results can be found in Table A11 and are fully in line with previous findings (with small changes to the magnitude of coefficients).

Table A11: Extension 6: Using Dummy Variables Instead of Absolute Distances

	Dependent variable:								
	Land Inequality (Gini)			Conservative Party Vote Share			Net Electoral Disputes		
	OLS			*OLS*			*glm: quasipoisson* *link = log*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BDEI Score v1	−0.056***			−0.120***			−0.258***		
	(0.013)			(0.032)			(0.092)		
BDEI Score v3		−0.049***			−0.103***			−0.218**	
		(0.012)			(0.030)			(0.086)	
BDEI Score v5			−0.045***			−0.095***			−0.195**
			(0.012)			(0.033)			(0.089)
Urb. Dens. 1300	0.022**	0.015	0.015	0.026	0.010	0.009	0.110	0.069	0.064
	(0.010)	(0.009)	(0.010)	(0.036)	(0.034)	(0.035)	(0.072)	(0.066)	(0.073)
Dummy Maj. Port	0.060***	0.062***	0.070***	−0.062	−0.058	−0.043	0.189	0.195	0.225*
	(0.022)	(0.023)	(0.024)	(0.041)	(0.041)	(0.040)	(0.121)	(0.119)	(0.120)
Dummy Trade City	0.002	0.003	0.003	−0.066**	−0.063**	−0.064**	0.047	0.053	0.052
	(0.015)	(0.015)	(0.015)	(0.025)	(0.025)	(0.026)	(0.121)	(0.121)	(0.122)
Dummy Ocean	0.022	0.022	0.027	0.005	0.004	0.013	0.009	0.008	0.023
	(0.016)	(0.016)	(0.016)	(0.047)	(0.049)	(0.049)	(0.163)	(0.163)	(0.165)
Dummy River	−0.013	−0.015	−0.017	−0.030	−0.034	−0.038	−0.191*	−0.201*	−0.211*
	(0.011)	(0.012)	(0.012)	(0.025)	(0.025)	(0.024)	(0.110)	(0.109)	(0.109)
Elevation	−0.0003***	−0.0003***	−0.0003***	−0.0003***	−0.0003***	−0.0003***	−0.001*	−0.001*	−0.001*
	(0.00004)	(0.00004)	(0.00003)	(0.0001)	(0.0001)	(0.0001)	(0.0004)	(0.0004)	(0.0004)
Constant	0.792***	0.794***	0.795***	0.234***	0.239***	0.243***	1.025***	1.037***	1.051***
	(0.015)	(0.015)	(0.015)	(0.042)	(0.043)	(0.045)	(0.130)	(0.129)	(0.128)
Observations	397	397	397	382	382	382	397	397	397
R^2	0.559	0.550	0.533	0.254	0.245	0.230			
Adjusted R^2	0.551	0.542	0.525	0.240	0.231	0.216			

Note: Clust. SE — *p<0.1; **p<0.05; ***p<0.01

A.9 Imperial Germany: Extension 7 — Accounting for Historical Information Asymmetries

In their study on historical political development, Ahmed and Stasavage (2020) suggest that information asymmetries between rulers and the ruled contributed to the emergence of government by council. Councils, as an early form of political participation, helped mitigate information asymmetries that posed challenges to the setting of tax rates.

Considering their historical focus, Ahmed and Stasavage (2020) construct and rely on a measure of *caloric variability* in agricultural production potential to quantify the aforementioned information asymmetries. Their indicator is based on the extent of local variations in the maximum caloric potential of crops grown in a given area. Accordingly, the variable is related to the most fundamental economic activity in pre-modern societies, namely, agriculture. Given the arguments by Ahmed and Stasavage (2020), *caloric variability* may be an important determinant of early democratic institutions, and thus an important variable to control for when analyzing the long-term influences on democratic practices. Therefore, we present an extended analysis below.

Following Ahmed and Stasavage (2020), we use data by Galor and Özak (2016) on maximum caloric potential (pre-1500 CE) to calculate local variation based on the standard deviation of surrounding raster cells (*caloric variability*). We then include this measure as an additional control variable in our regression analyses. We find that adding *caloric variability* does not affect the results in a way that would compromise our earlier interpretation. All details can be found in Table A12.

Table A12: Extension 7: Accounting for Historical Information Asymmetries

	Dependent variable:								
	Land Inequality (Gini)			Conservative Party Vote Share			Net Electoral Disputes		
	OLS			*OLS*			*glm: quasipoisson* *link = log*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BDEI Score v1	−0.054***			−0.135***			−0.254***		
	(0.013)			(0.034)			(0.092)		
BDEI Score v3		−0.049***			−0.121***			−0.230***	
		(0.011)			(0.031)			(0.085)	
BDEI Score v5			−0.046***			−0.122***			−0.226***
			(0.011)			(0.036)			(0.084)
Caloric Variability	−0.00002	−0.00002	−0.00003	0.00005	0.00004	0.00004	−0.00001	−0.00002	−0.00003
	(0.00003)	(0.00003)	(0.00003)	(0.0001)	(0.0001)	(0.0001)	(0.0003)	(0.0003)	(0.0003)
Urb. Dens. 1300	0.007	0.001	0.001	0.035	0.018	0.025	0.047	0.015	0.024
	(0.012)	(0.011)	(0.011)	(0.038)	(0.036)	(0.039)	(0.074)	(0.065)	(0.069)
Dist. Maj. Port	−0.0004***	−0.0004***	−0.0005***	−0.0004**	−0.0004**	−0.0005**	−0.002***	−0.002***	−0.003***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)
Dist. Trade City	−0.00004	−0.00004	−0.00002	0.0001	0.0001	0.0002	0.0001	0.0001	0.0002
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)
Dist. Ocean	−0.00002	−0.00001	−0.00000	−0.0003*	−0.0003	−0.0003	0.0003	0.0003	0.0003
	(0.0001)	(0.00005)	(0.00005)	(0.0002)	(0.0002)	(0.0002)	(0.0004)	(0.0004)	(0.0004)
Dist. River	−0.0001	−0.0001	−0.0001	−0.0003	−0.0003	−0.0002	0.002	0.002	0.002
	(0.0001)	(0.0001)	(0.0001)	(0.0004)	(0.0004)	(0.0004)	(0.001)	(0.001)	(0.001)
Elevation	−0.0001***	−0.0001***	−0.0001***	0.0001	0.0001	0.0001	0.0002	0.0002	0.0002
	(0.00005)	(0.00005)	(0.00005)	(0.0001)	(0.0001)	(0.0001)	(0.0004)	(0.0004)	(0.0004)
Constant	0.839***	0.840***	0.841***	0.242***	0.244***	0.244***	1.019***	1.026***	1.037***
	(0.015)	(0.015)	(0.014)	(0.050)	(0.051)	(0.052)	(0.135)	(0.135)	(0.138)
Observations	397	397	397	382	382	382	397	397	397
R^2	0.632	0.632	0.616	0.300	0.295	0.288			
Adjusted R^2	0.625	0.624	0.609	0.285	0.280	0.273			
Note: Clust. SE							*p<0.1; **p<0.05; ***p<0.01		

A.10 Imperial Germany: Extension 8 — Introducing Spatial Fixed Effects

In a response to Homola, Pereira and Tavits (2020), Pepinsky, Goodman and Ziller (2020) suggest that historical measures based on distance to locations can lead to incorrect inferences if researchers do not account for unobserved regional heterogeneity in their empirical specifications. Among other suggestions, they advocate for the use of spatial fixed effects to address this issue.

In our case, the introduction of spatial fixed effects may be merited as well. However, there are two crucial differences between our paper and Homola, Pereira and Tavits (2020): First, while Homola, Pereira and Tavits (2020) have precise data on all concentration camp locations (a central object of inquiry in their study) and distances to them, our *BDEI score* is an imperfect extrapolation based on the best available data. As such, it likely includes a random noise component. Due to the fact that our measure is an extrapolation that may include random noise (meaning that there likely is an unobserved component of Black Death intensities), it is possible that spatial fixed effects will absorb variation that may actually be due to differences in the historical intensity of plague outbreaks. Second, our approach does not rely on the distance to the nearest outbreak location only. Instead, we take into account the entire set of outbreak locations weighted by their distance to the location under consideration. Therefore, our measure includes a spatial dependence component to begin with. These two factors make our analysis quite different from Homola, Pereira and Tavits (2020). Although we present results with spatial fixed effects below, models that are limited to the spatial clustering of errors (as we use throughout the paper) instead of spatial fixed effects are our preferred option.

To model unobserved spatial heterogeneity without introducing post-treatment bias (since the formal groupings of districts are non-random and instead constructed based on socioeconomic and political characteristics), we create a quasi-random[25] global spatial grid consisting of 75x75 rectangular cells that—in the geographic area where Germany is located—are approximately 300x300 km.[26] We observe that, without further modifications, the centroids of Imperial Germany's electoral districts are distributed across 16 rectangular cells. Cells with five or fewer observations are merged with the adjacent cell, which results in a total of 11 spatial groupings (fixed effect categories), with an average of 36.1 units per group.

Subsequently, we rerun our analysis with these spatial fixed effects as shown in Table A13. We find that the majority of our results still hold: With respect to *land inequality*, all versions of the *BDEI score* except for v5 are significant at $p < 0.05$ (v2 and v4 are omitted from the table for space reasons). Furthermore, with respect to *Conservative Party vote share*, we can also confirm all previously obtained results (although there are smaller differences in magnitude). At the same time, introducing spatial fixed effects appears to weaken the results with respect to *net electoral disputes*: We no longer find results that are significant at $p < 0.05$. While there is a strong reduction in the significance of the *BDEI score's* effect on electoral disputes, we caution the reader again to consider the possibility that the spatial fixed effects absorb some of the unobserved (i.e., imperfectly extrapolated) impact of the Black Death.

[25]The grid is only quasi-random because it is constructed based on the global latitude/longitude system and the international prime meridian.

[26]Due to the curvature of the earth, this is only a rough approximation. Actual size may vary by up to 30-40 km in east-west/north-south length depending on exact location.

Table A13: Extension 8: Introducing Spatial Fixed Effects

	Dependent variable:								
	Land Inequality (Gini)			Conservative Party Vote Share			Net Electoral Disputes		
	OLS			*OLS*			*glm: quasipoisson* *link = log*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BDEI Score v1	−0.069***			−0.145***			−0.282*		
	(0.021)			(0.045)			(0.167)		
BDEI Score v3		−0.061***			−0.133***			−0.236	
		(0.020)			(0.043)			(0.166)	
BDEI Score v5			−0.032			−0.126**			−0.169
			(0.024)			(0.052)			(0.188)
Urb. Dens. 1300	0.008	0.003	−0.001	0.003	−0.008	0.002	−0.135	−0.161	−0.162
	(0.019)	(0.018)	(0.020)	(0.046)	(0.045)	(0.047)	(0.144)	(0.140)	(0.148)
Dist. Maj. Port	−0.001***	−0.001***	−0.001***	−0.0002	−0.0003	−0.0003	−0.003***	−0.003***	−0.003***
	(0.0001)	(0.0001)	(0.0001)	(0.0003)	(0.0003)	(0.0003)	(0.001)	(0.001)	(0.001)
Dist. Trade City	−0.00001	−0.00001	−0.00000	0.0001	0.0001	0.0001	0.00004	0.00004	0.0001
	(0.00004)	(0.00005)	(0.00005)	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)
Dist. Ocean	0.00002	0.00003	0.00003	−0.0003**	−0.0003**	−0.0003**	0.0003	0.0003	0.0003
	(0.00004)	(0.00004)	(0.00005)	(0.0002)	(0.0002)	(0.0002)	(0.0004)	(0.0004)	(0.0004)
Dist. River	0.0001	0.0001	0.0001	−0.00002	0.00000	0.00003	0.002	0.002	0.002
	(0.0001)	(0.0001)	(0.0001)	(0.0005)	(0.0005)	(0.0005)	(0.001)	(0.001)	(0.001)
Elevation	−0.0001***	−0.0001***	−0.0001**	0.0001	0.0001	0.0001	0.0002	0.0002	0.0002
	(0.00005)	(0.00005)	(0.00005)	(0.0001)	(0.0001)	(0.0001)	(0.0004)	(0.0004)	(0.0004)
Spatial Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	397	397	397	382	382	382	397	397	397
R^2	0.991	0.991	0.991	0.575	0.573	0.569			
Adjusted R^2	0.991	0.991	0.990	0.554	0.552	0.547			

Note: Clust. SE *p<0.1; **p<0.05; ***p<0.01

A.11 Imperial Germany: Extension 9 — Using Alternative Datasets of Plague Outbreaks

In the main empirical analysis, we use data by Jedwab, Johnson and Koyama (2019*a*) to construct different versions of the *BDEI score*. We use these data because, to the best of our knowledge, they are the only data on the Black Death that do not simply record the occurrence of an outbreak but also its *intensity*. Accounting for the intensity of outbreaks is of crucial importance to our paper for two reasons.

First, our theory is centered on explaining how *variation in intensity* accounts for different legacies of the Black Death. Therefore, measuring levels of intensity is necessary to properly test the theory.

Second, there were vast differences in local mortality rates across space and time. As we have shown in subsection A.7, places where the plague started at a later time experienced much milder outbreaks. This could help explain why the eastern parts of German-speaking central Europe historically were less affected than other areas: For the most part, the Black Death only arrived there in 1351, the last year of the plague's initial outbreak.

While the data by Jedwab, Johnson and Koyama (2019*a*) on plague outbreaks have the crucial advantage of also including local mortality rates, readers of our paper may be concerned about the lack of observations that are in the easternmost parts of German-speaking central Europe. Even though the lack of concrete data on mortality rates in these parts is likely directly related to the much lower severity of the outbreak there, it would be desirable to identify alternative datasets that contain outbreaks in this part of Europe (even if such datasets omit crucial information on outbreak intensity) and check if our results hold when using them.

In this respect, we have identified two alternative datasets by Büntgen et al. (2012) and

Schmid et al. (2015). These two datasets are closely related to each other. Specifically, Schmid et al. (2015) merges the original Büntgen et al. (2012) data with another dataset to create a comprehensive record of plague outbreaks for the entire medieval period (this dataset also makes some corrections to previous data entries).

For reasons of transparency, we provide results using both of these alternative datasets as the underlying data to construct the *BDEI score*. Since both cover a longer time period of plague outbreaks, we restrict the analysis to outbreaks in 1347-1352. Furthermore, since these data do not include information on mortality rates, but on the number of years during which a location was affected by the Black Death, when constructing the *BDEI score*, we have to assign a mortality rate of "1" and subsequently account for every year in which there was an outbreak (so that observations that had outbreaks in two years are weighted twice as much as observations that only had an outbreak in one year). This means that these scores are based on recurrence of the plague rather than its severity, though the two concepts are likely correlated.

The results are in Table A14, which based on data by Büntgen et al. (2012), and Table A15, which is based on data by Schmid et al. (2015). While there are minor differences to the main results, they are broadly in line with what we have found previously. In some cases, the magnitude of the effect is slightly larger, in others, it is slightly smaller. Most importantly, the coefficients of the *BDEI score* are consistently at the highest level of statistical significance ($p < 0.01$).

Table A14: Extension 9: Using an Alternative Dataset of Plague Outbreaks (Büntgen et al., 2012)

	Dependent variable:								
	Land Inequality (Gini)			Conservative Party Vote Share			Net Electoral Disputes		
	OLS			*OLS*			*glm: quasipoisson* *link = log*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BDEI Score v1 (Alt. Data 1)	−0.065***			−0.148***			−0.344***		
	(0.016)			(0.041)			(0.122)		
BDEI Score v3 (Alt. Data 1)		−0.057***			−0.109***			−0.279***	
		(0.013)			(0.036)			(0.102)	
BDEI Score v5 (Alt. Data 1)			−0.048***			−0.092***			−0.232***
			(0.011)			(0.034)			(0.086)
Urb. Dens. 1300	0.015	0.004	−0.004	0.045	0.006	−0.010	0.107	0.028	−0.014
	(0.014)	(0.011)	(0.009)	(0.044)	(0.037)	(0.035)	(0.092)	(0.068)	(0.058)
Dist. Maj. Port	−0.0003***	−0.0003***	−0.0004***	−0.0001	−0.0002	−0.0003	−0.002***	−0.002***	−0.002***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)
Dist. Trade City	−0.00005	−0.00003	−0.00002	0.0001	0.0002	0.0002	0.0001	0.0001	0.0002
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)
Dist. Ocean	−0.00000	0.00000	0.00000	−0.0003	−0.0002	−0.0003	0.0003	0.0004	0.0004
	(0.0001)	(0.00005)	(0.00005)	(0.0002)	(0.0002)	(0.0002)	(0.0004)	(0.0004)	(0.0004)
Dist. River	−0.0002	−0.0001	−0.0001	−0.0004	−0.0003	−0.0002	0.001	0.001	0.002
	(0.0001)	(0.0001)	(0.0001)	(0.0005)	(0.0005)	(0.0005)	(0.001)	(0.001)	(0.001)
Elevation	−0.0001***	−0.0001**	−0.0001**	0.0002*	0.0002	0.0001	0.0004	0.0004	0.0003
	(0.00005)	(0.00005)	(0.00005)	(0.0001)	(0.0001)	(0.0001)	(0.0004)	(0.0004)	(0.0004)
Constant	0.816***	0.815***	0.818***	0.196***	0.201***	0.207***	0.906***	0.914***	0.936***
	(0.017)	(0.016)	(0.015)	(0.048)	(0.052)	(0.055)	(0.146)	(0.147)	(0.148)
Observations	397	397	397	382	382	382	397	397	397
R^2	0.629	0.632	0.624	0.278	0.259	0.252			
Adjusted R^2	0.623	0.626	0.618	0.264	0.245	0.238			

Note: Clust. SE *p<0.1; **p<0.05; ***p<0.01

Table A15: Extension 9: Using an Alternative Dataset of Plague Outbreaks (Schmid et al., 2015)

	Dependent variable:								
	Land Inequality (Gini)			Conservative Party Vote Share			Net Electoral Disputes		
	OLS			*OLS*			*glm: quasipoisson* *link = log*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BDEI Score v1 (Alt. Data 2)	−0.065***			−0.151***			−0.340***		
	(0.016)			(0.041)			(0.118)		
BDEI Score v3 (Alt. Data 2)		−0.054***			−0.108***			−0.260***	
		(0.012)			(0.034)			(0.094)	
BDEI Score v5 (Alt. Data 2)			−0.046***			−0.092***			−0.218***
			(0.010)			(0.030)			(0.079)
Urb. Dens. 1300	0.017	0.003	−0.006	0.052	0.007	−0.012	0.117	0.021	−0.027
	(0.014)	(0.010)	(0.008)	(0.045)	(0.037)	(0.033)	(0.094)	(0.065)	(0.053)
Dist. Maj. Port	−0.0003***	−0.0004***	−0.0004***	−0.0002	−0.0003	−0.0003*	−0.002***	−0.002***	−0.002***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)
Dist. Trade City	−0.00004	−0.00003	−0.00002	0.0001	0.0002	0.0002	0.0001	0.0002	0.0002
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.001)
Dist. Ocean	−0.00001	0.00000	0.00000	−0.0003	−0.0003	−0.0003	0.0003	0.0003	0.0004
	(0.0001)	(0.00005)	(0.00005)	(0.0002)	(0.0002)	(0.0002)	(0.0004)	(0.0004)	(0.0004)
Dist. River	−0.0002	−0.0001	−0.0001	−0.0004	−0.0003	−0.0002	0.001	0.002	0.002
	(0.0001)	(0.0001)	(0.0001)	(0.0005)	(0.0005)	(0.0005)	(0.001)	(0.001)	(0.001)
Elevation	−0.0001***	−0.0001***	−0.0001***	0.0002	0.0002	0.0002	0.0003	0.0004	0.0003
	(0.00005)	(0.00005)	(0.00005)	(0.0001)	(0.0001)	(0.0001)	(0.0004)	(0.0004)	(0.0004)
Constant	0.821***	0.823***	0.826***	0.207***	0.215***	0.221***	0.936***	0.953***	0.973***
	(0.016)	(0.015)	(0.014)	(0.049)	(0.052)	(0.053)	(0.142)	(0.141)	(0.142)
Observations	397	397	397	382	382	382	397	397	397
R^2	0.629	0.636	0.632	0.281	0.267	0.264			
Adjusted R^2	0.623	0.629	0.625	0.268	0.253	0.250			

Note: Clust. SE *p<0.1; **p<0.05; ***p<0.01

A.12 Further Discussion of the Empirical Design

In this section, we further discuss aspects of the empirical design, namely (1) our choice to focus on the plague outbreak in 1347-1351, (2) the possibility that preexisting differences in socioeconomic conditions inflate our *BDEI score* estimates, and (3) reasons for not including the Free Conservative Party when analyzing political outcomes.

A.12.1 The Difference Between the Initial and Subsequent Plague Outbreaks

For several centuries after the initial outbreak of the Black Death—the event that is at the center of our study—Europeans repeatedly suffered from further plague outbreaks. Why did we limit our study and empirical design to the major outbreak that occurred in 1347-1351?

The reason for this choice is primarily a substantive one. Only the shock of 1347-1351 was of such depth, severity, and geographic extent that it led to the "tectonic" movements in political-economic equilibria that many have historians, economists, and epidemiologists have observed before us.

All subsequent outbreaks were limited in their geographic extent and/or killed a substantially smaller number of people (in many of the affected locations). Not only do less severe outbreaks make it more likely that external market forces restore an old political-economic equilibrium more quickly, but it is also improbable that they would lead to fundamental reconfigurations of social and political relationships as did the first wave of the Black Death. Equally important, subsequent outbreaks of the plague were anticipated, whereas the initial shock was not. The experience with the Black Death led to changes in inheritance patterns and other adaptations that cushioned the economic blow of subsequent plague recurrences. For these reasons, our focus is on the 1347-1351 episode.

A.12.2 The Possible Effect of Preexisting Differences in Political Institutions

Some may argue that preexisting differences in landholding inequality and labor coercion could potentially bias the results of our study. One such argument might be that the eastern parts of German-speaking central Europe historically (i.e., pre-1347) already had significantly higher levels of labor coercion and landholding inequality, rendering the impact of the Black Death less substantial than we argue it is.

As a first response to this objection, note that at least part of such variation in initial socioeconomic structures would likely be picked up by one of the covariates that we employ in our analysis: the level of *urban density in 1300*. As it turns out, the coefficient on this variable is insignificant in the vast majority of specifications, indicating that regional differences in urban density in the early 14th century were small and cannot account for the substantial variation in socieconomic structures observed in 19th-century Imperial Germany.

More importantly, the objection relies on the assumption that prior to the arrival of the Black Death labor coercion and land inequality were stronger in the eastern parts of German-speaking central Europe than in the west. This is a dubious historical claim. Not only were the regional differences likely small, existing historiography suggests that the eastern parts of "Germany" probably had more progressive labor regimes than the western parts.

Describing conditions in the east prior to the Black Death, Carsten (1954, 88) writes:

> "The peasants' position was far better than it was in the west, and this included the native population. Class distinctions in the east were less sharp, noblemen moved into the towns and became burghers, while burghers acquired estates and village mayors held fiefs. The whole structure of society, as might be expected of a colonial area, was much freer and looser than it was in western Europe."

This more favorable context for laborers was tied to the relatively recent colonization of the east by German speakers. As explained by Carsten (1954, 38):

> "The fact that the German villages [in the east] as a rule were founded 'from wild root' explained, in the opinion of a legal commentator of the early fourteenth century, that the peasants had better rights in Brandenburg than they had in Saxony, that they could freely sell and leave their farms, that they had a 'heritage' which was better than leasehold, as they had improved their holdings with their own work."

In short, the high level of labor coercion and landholding inequality that existed in Prussia in the centuries preceding the German Empire was *not* a feature of these regions in the period prior to the Black Death. Rather, the differential impact of the Black Death led to what was, in effect, a long-run reversal of fortune for laborers: the abandonment of labor coercion in previously highly coercive areas (the west) and the growth of labor coercion in previously less coercive areas (the east). The crucial point here for our analysis is that unmeasured differences in pre-Black Death socioeconomic structures likely bias *against* our findings, since these structures were historically more coercive towards labor in the west than in the east.

A.12.3 The Free Conservative Party / German Empire Party

In addition to the Conservative Party, a second party in Imperial Germany represented conservative interests: the Free Conservative Party or German Empire Party (*Freikonservative Partei* or *Deutsche Reichspartei*). We did not include an analysis of this party's electoral outcomes for two reasons: First, different from the Conservative Party, the Free Conservative Party was not exclusively a party representing the interests of traditional landed elites. Instead, industrialists, who embraced capitalism and industrial production, were members, too. Second, the party's program was more moderate than that of the Conservative Party. While Free Conservatives also defended existing social hierarchies, they were less extreme in their political goals. In contrast, the Conservative Party of the early 1870s went so far as to demand the construction of an "estate society" (Berdahl, 1972, 2-3). For these reasons, vote shares for the Free Conservative Party are not as good an empirical match with the expectations derived from our theory about the Black Death's long-term consequences as vote shares for the Conservative Party.

A.13 Pre-Reformation Germany: Descriptive Summary Statistics

Table A16 shows descriptive summary statistics for towns in pre-Reformation Germany.

Table A16: Descriptive Statistics: Pre-Reformation Germany

Variable	**n**	**Min**	$\mathbf{q_1}$	$\bar{x}$	$\widetilde{x}$	$\mathbf{q_3}$	**Max**	**IQR**
BDEI Score v1	325	-2.78	-0.71	0.00	0.18	0.86	1.92	1.57
BDEI Score v2	325	-2.52	-0.72	0.00	0.12	0.79	2.45	1.52
BDEI Score v3	325	-2.46	-0.69	0.00	0.10	0.73	3.01	1.42
BDEI Score v4	325	-2.52	-0.63	0.00	0.15	0.62	3.51	1.24
BDEI Score v5	325	-2.65	-0.52	0.00	0.21	0.52	3.87	1.05
Introduction of Participative Elections (1300-1500)	325	0.00	0.00	0.15	0.00	0.00	1.00	0.00
Elevation	86	2.72	79.33	227.90	187.22	357.24	852.91	277.91
Dist. to River	86	0.13	12.69	51.90	50.53	78.28	143.05	65.59
Roman Road	86	0.00	0.00	0.26	0.00	0.75	1.00	0.75
Coast	86	0.00	0.00	0.08	0.00	0.00	1.00	0.00
Agricultural Suitability	86	0.00	19.25	35.91	37.33	50.42	69.27	31.17
Population (Log)	86	6.91	7.70	8.57	8.70	9.28	10.90	1.58
Ruggedness	86	2.21	11.14	34.75	25.91	46.03	342.94	34.89
Urban Potential 1300	86	2252.19	4437.69	5147.74	4998.34	5852.00	8224.76	1414.31
Trade City 1300	86	0.00	0.00	0.06	0.00	0.00	1.00	0.00
Proto-Industrial City 1300	86	0.00	0.00	0.05	0.00	0.00	1.00	0.00