Monopsony power and wages: Evidence from the introduction of serfdom in Denmark

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Abstract: We exploit a large historical shock to the Danish labour market to provide evidence of how restrictions on labour mobility increase monopsony power and thereby reduce wages. By severely limiting the possibility of the rural population to work beyond their place of birth, the reintroduction of serfdom in 1733 aimed to increase monopsony power and secure cheaper labour in the countryside. Using a unique data source based on the archives of estates from the eighteenth century, we test whether serfdom affected the wages of farmhands more strongly than other groups in the labour market, and results based on a difference-in-differences approach reveal evidence consistent with a strong negative effect following its introduction. This is confirmed when we use a different control group from the Swedish province of Scania. We also investigate whether one mechanism was that boys with rural backgrounds were prevented from taking up apprenticeships in towns and find suggestive evidence that this was indeed the case.

Keywords: Coercion, labour mobility, monopsony, serfdom.

JEL Classification: J42, J31, N33, P4

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1. Introduction

How do policies or institutions designed to reduce the mobility of workers affect monopsony power and thereby wages? This is a question of considerable importance both for the world today, as well as in the past, with authors such as Genicot (2002) and Acemoglu and Wolitzky (2011) noting that labour transactions throughout most of history, and a significant fraction of such transactions in developing countries today, are coercive and often attempt to limit mobility.[1] Moreover, monopsony power in product and labour markets has been argued to be on the rise even in advanced economies (Council of Economic Advisors, 2016). Search and mobility costs create imperfections in the labour markets where employees cannot exploit potential arbitrage opportunities offered by homogeneous job vacancies (Boeri and van Ours, 2021). In the twenty-first century these costs might even have increased, since new waves of globalization and of advanced technological change have facilitated the product market concentration of the most productive firms (Autor et al., 2017a and 2017b)) and thus their monopsonistic power. In addition, collusion among employers and the intensive use of legal instruments such as no-poaching agreements limits the flexibility of labour markets (Krueger and Ashenfelter, 2018). While models of imperfect competition have been used to analyse a wide variety of labour market phenomena following seminal work by Robinson (1933), Burdett and Mortensen (1998), and Manning (2003), credible estimates of the effect of monopsony power on wages are in general lacking (Naidu et al., 2016). The present paper on the other hand provides new evidence on this effect by considering a large negative shock to labour mobility in the form of the reintroduction of serfdom in eighteenth century Denmark, for which we have unique individual level labour market data.

Although historical studies are largely in line with the view that the restrictions on labour mobility associated with serfdom were negative for economic development (e.g., Acemoglu, Johnson and Robinson, 2005, p. 441),[2] an unequivocal long-run relationship between labour market imperfections and wage dynamics has not so far been identified. Thus, Dennison (2006, p. 74) highlights that "*a*

[1] Coerced labour has been a persistent feature in developing countries such as Brazil, India and Pakistan throughout the twentieth century, see the contributions in Andrees and Belser (2009). Yet another example is the Chinese Hukou system, which serves to restrict rural-urban migration, see Whalley and Zhang (2007).

[2] Recent examples include Ogilvie (2007) and Ogilvie and Carus (2014).

revisionist view has emerged, which portrays serfdom as having had little or no effect on peasants' social and economic behaviour." Ogilvie (2005, p. 93) similarly points out that revisionists have adduced *"[…] evidence that serfs sometimes migrated without apparent manorial hindrance,*" and a similar debate has played out in the Danish historiography.[3] This interpretation thus suggests that farmhands found ways of getting around the mobility restrictions associated with serfdom, and that its effects could very well be negligible.[4] Resolving this debate requires, however, individual micro level labour market data of the type we exploit here.

Thus, in this paper, we provide new quantitative evidence on the effects of reducing mobility in the labour market by exploiting the introduction of "*the Danish equivalent of serfdom*" (Østergaard 1995) known as adscription (in Danish: *stavnsbånd*) in Denmark in 1733. Although in principle adscription was applied to the entire rural population, we leverage that it was mostly targeted at tying male farmhands to the estate in the area in which they were born (Olsen 1933; Skrubbeltrang 1961). These agricultural workers were largely unskilled, and since they became bound to a given estate, the monopsony power of the estate would increase in relation to this group, with the consequence that the wages of agricultural workers were likely to decrease compared to other groups in the labour market as e.g. the mobility of craftsmen would be much less affected by serfdom.[5] Moreover, adscription could also serve to prevent young men from the countryside from moving out of the rural sector via an apprenticeship in a town.

Our dataset contains information on the wages, occupation and geographical location as well as other characteristics of individuals selling their labour to an estate. This allows us to divide the data into

[3] Mirroring the international literature, the traditional view has been that serfdom in Denmark was effective in restricting mobility and wage growth for farmhands. Thus, the Danish economic historian Hansen (1984, p. 43), for example, suggested that serfdom was effective in securing the estates a cheap, dependent labour force. In a similar vein, Andersen and Pedersen (2004, p. 46) argue that "*access to unfree labour supplied by the adscripted men on the estate must have guaranteed an upper ceiling for the wages of others."* By contrast, Løgstrup (1987, 1988) takes a view closer to that of revisionist historians, and emphasizes the existence of some geographical mobility.

[4] See, e.g., Hagen's (2002) study of Prussia, which emphasizes that serfdom and growth were compatible or the discussion in Clark (2007, pp. 220-223). For further examples, see Ogilvie (2005).

[5] Bobonis and Morrow (2014) show that when unskilled labour is coerced to work for e.g. landowners, then the relative wage of skilled workers increases. We return to potential mechanisms in Section 6.

different groups and evaluate whether there was a differential impact on farmhands as compared to other groups in the labour market in a difference-in-differences approach. This means that we exploit the re-introduction of serfdom as our source of variation across time combined with the fact that serfdom was targeted at farmhands, which provides us with variation across occupations. We therefore control for time and occupation in all our estimations. Given the micro-level nature of the data, we can also control for fixed effects for gender, geographical factors, seasonal work, and other characteristics of the individuals we observe. We also control for region by year fixed effects which vary by geographical area in more extended specifications. Further, we can control for agricultural prices, which according to the historical narrative may have driven the re-introduction of serfdom, see e.g. Christensen et al. (1934), Feldbæk (1982) and Dombernowsky (1988).

We find a sizeable, negative impact on the wages of farmhands using the difference-in-differences approach. In our baseline result, we find that farmhands experienced wages that were around thirty per cent lower than they otherwise would have been compared to those of other groups in the labour market after the introduction of serfdom.[6] Importantly, we can also provide evidence that there was no discernible differential trend between farmhand wages and those of other groups in the labour market prior to the reintroduction of serfdom. Yet, a concern with our identification strategy is that the wages of other occupations were also negatively affected by serfdom. To deal with this, we compare the evolution of farmhand wages with those of unskilled workers in Scania, a province in Sweden, which is geographically and institutionally close to Denmark, but was not subject to serfdom. Moreover, Scania had been under Danish rule until the mid-seventeenth century. We further complement this analysis with suggestive evidence on reduced opportunities for apprenticeships for young men from rural areas after serfdom was introduced using individual level data for apprentices in the Danish city of Odense.

This paper thus makes at least two contributions. First, we provide new evidence on the effects of monopsony power on wages by exploiting the introduction of serfdom as our quasi-experiment. The

[6] For example, Naidu (2010) reports that a 10 per cent increase in the enticement fine leads to a reduction in wages, which is between 0.11 and 0.17 per cent. He describes these as small magnitudes but suggests that they are lower bounds. While not directly comparable to our estimates, these appear modest.

study most closely resembling our paper in terms of method is Naidu et al. (2016), who consider a positive shock to labour mobility in the United Arab Emirates using monthly data over a four-year period, demonstrating that visa policies for immigrant workers can result in substantial monopsony power by restricting job-to-job transitions. In our case, we consider almost a century's worth of data over a period when similar restrictions were implemented on a national scale. We thus study the long run impact of this negative shock to mobility on wages of domestic farmhands, and an example of increased rather than decreased monopsony power. We also note that this is a case in which the historical narrative strongly suggests that estate owners had more monopsony power in the serfdom period.[7] Our case also clearly demonstrates the negative impact monopsony power can have on wages and the welfare of workers as caused by reduced mobility. This is in line with theory which suggests that monopsony power is negatively related to how mobile workers are across locations, see Mendez-Chacon and Van Patten (2019).[8]

Second, we contribute to the literature on the effects of serfdom and the broader literature on institutions and policies that restrict labour mobility. While there are many historiographical analyses of serfdom (Domar, 1970; North and Thomas, 1971; Brenner, 1976), there is relatively little quantitative evidence on the effects of serfdom on labour market outcomes for Western Europe, where serfdom usually ended in the early sixteenth century and where data are largely unavailable. Some evidence exists for Eastern Europe (as discussed below) where the emancipation of the peasantry came much later, see e.g. Persson and Sharp (2015, p. 90-97). That Denmark was an exception is also confirmed by Rudé (1972, p. 31), who refers to Denmark as the "*only major exception to the sharp east-west antithesis*". Thus, studying the re-introduction of serfdom in Denmark offers a unique possibility to investigate the impact of serfdom on a Western European country.[9] Another interesting feature of

[7] For example, Andersen and Pedersen (2004, p. 46) state that on one estate in our sample, Taasinge, the owner, Niels Juel, was said to dictate the level of wages in the 1740s and 1750s. Yet, they also argue that in general no estate was "an island" that could dictate wages, and stress that labour markets have a strong regional element as we discuss below.

[8] Mendez-Chacon and Van Patten (2020), who, using data from land concession in Costa Rica, show that a large firm, such as the *United Fruit Company*, may be beneficial for the welfare of workers once these are sufficiently mobile.

[9] Denmark is, of course, also a Scandinavian country. Among these countries, Denmark is also an exception as Norway and Sweden never experienced serfdom.

Danish serfdom was that the rules were gradually changed to pertain to larger age groups. In 1733, serfdom implied that a farmhand in the age group 14-36 years could not leave the estate to which he belonged from birth. The age group was extended to 9-40 years in 1742, and to 4-40 in 1764. The reform of 1788 meant that the age group was yet again 14-36 years. This allows us not only to investigate the immediate effect of serfdom under the 1733 rules, but we can also dig into whether tightening the rules was effective. It should be emphasized, however, that by investigating the impact of the 1733 serfdom in Denmark, what we look at is only one element of what is regarded as serfdom by many scholars, namely restrictions on mobility. This is not because other features often regarded as part of this institution did not exist, as we will discuss in more detail below. Yet, they were not a part of the reform we consider. Nevertheless, in the context of this study, we also note that it is an advantage in terms of identification that we can exploit variation in – and consider the consequences of – just one of the dimensions of an economy with serfdom, namely the mobility of farmhands. Moreover, mobility restrictions are included in most definitions of serfdom.

Thus, we contribute to the quantitative literature on the effects of monopsony, the effects of serfdom, as well as to the broader literature on institutions and policies that restrict labour mobility.[10] We discuss these studies in a literature review in the next section. Following this, section 3 gives a brief history of serfdom in Denmark. Section 4 describes the empirical strategy, section 5 describes the data, and section 6 presents the analysis. Section 7 offers interpretation and discusses mechanisms, and section 8 concludes.

2. Literature review

In this section, we discuss how serfdom has been defined in the historiographical literature and how the Danish form relates to this. We then review existing empirical studies of monopsony, serfdom and labour coercion.

[10] Slavery is an important example of a coercive labour market institution as in e.g. the American South. Slavery and serfdom share some similarities, but as pointed out by Rigby (2003), serfs have legal rights and are often able to provide for their own subsistence, which is not true of slaves (p. 463).

Definitions of serfdom and the Danish case

Much work on serfdom includes the observation that there is no uniform definition. For example, Blum (1961, p.6) argues that "Serfdom […] defies easy explanation", and that the word "*serf*" or its equivalent, was applied to a wide range of European peasants, from people whose condition could scarcely be distinguished from that of chattel slaves to men who were nearly free. Moreover, he argues that "*often the serf is thought to have been a person who was bound to the soil.*" Similarly, in the *Oxford Encyclopedia of Economic History*, Rigby (2003, p. 463) states that "*the precise form taken by serfdom cannot be specified in advance but must be established empirically for each particular time and pl*ace" but does however emphasize that legal restrictions on the right of movement are typically part of the whole serfdom package.[11] O'Rourke (2017, p. 423) similarly argues that "*there was no single system of serfdom.*"

It is perhaps, therefore, no surprise that definitions of serfdom also vary in the literature. In his typology of serfdom around the Baltic Sea, Schmidt (1997) concludes that the common denominator was a restriction on the mobility of the farming population in order to maintain them as labourers at the manorial demesnes or as payers of land rent. Persson (2014, p.228) also views restrictions on mobility as the essence of serfdom and states that "*serfdom was essentially a way for a landowner to deny or restrict labour mobility.*" Yet, others such as Blanning (1983, p. 20) view this as an attenuated form of serfdom. Ogilvie and Carus (2014, p. 474), drawing largely on the experience of Russia and Eastern European countries, describe serfdom as follows: "*a serf was legally tied to the landlord in a variety of ways, typically by being prohibited from migrating, marrying, practicing certain occupations, selling certain goods, participating in factor and product markets, or engaging in particular types of consumption without obtaining permission from his landlord.*" As is readily seen, this definition means that serfdom includes more than just restrictions on mobility. Field (1994, p.50) argues that the essence of Russian serfdom was the subjection of the serfs to the arbitrary power of their master or mistress. This included e.g. the selling of serfs. Yet, he also mentions that a second element of serfdom was "*ascription*", which meant that a serf could only travel from his home with his master's permission.

[11] The Encyclopedia bases its discussion on Blum (1957) and Bush (1996, 2000) among others.

Markevich and Zhuravskaya (2018) note that the Russian government limited the right of migration of Russian peasants from the fifteenth century and by 1649 migration had become a criminal offence.

The Danish policy of adscription clearly fits better with the definitions that emphasize mobility restrictions. Moreover, it is probably best thought of as a milder form of serfdom or attenuated serfdom as suggested by some authors. Still, all definitions above emphasize mobility restrictions, which underlines that this is an important element of any definition of serfdom. Yet, while it was not a part of adscription, tenant farmers had obligations in terms of in-kind rent and boon work for their landlord, and in this way, other elements of labour coercion were also present (Lampe and Sharp, 2018). Yet, there was no permanent changes to these elements in the period considered.[12] This shares similarity with the obligations of the serfs in Russia (Markevich and Zhuravskaya, 2018), but does not of course change the fact that serfdom in Denmark was different from Eastern European and Russian serfdom.

Existing empirical studies of monopsony, serfdom and labour coercion

Good overviews of existing studies of monopsony power can be found in Manning (2011, 2021), and we therefore refer the reader to his comprehensive reviews. Many of these studies exploit wage regulations to estimate the effect of wages on employment.[13] As mentioned in the introduction, our paper takes an approach similar to Naidu et al. (2016) in that we exploit a shock to mobility and perform difference-in-differences estimation. Yet, we focus on a reduction in mobility of domestic farmhands and can study the effects of reduced mobility over a much longer period. By studying the effect of a reform for a longer period, we gain the opportunity to look at both adaptation of workers to the reform as well as additional changes to a reform. Moreover, the responses to reduced mobility in terms of

[12] According to Hey (2010), boon work was *"A manorial duty to do such seasonal work as ploughing and harvesting.."* In the Danish case, there was a brief attempt in 1771 to 1772 to regulate boon work. This meant that estate owners had to issue receipts for work carried out and the amount of boon work was no longer based on the needs of the estate. This was abolished in 1773 when again, the amount of boon work was based on the needs of the estate. From 1791, the amount of boon work was based on voluntary agreements between estate and workers. In 1799, legislation meant that future changes to existing agreements had to be approved by the Ministry of Finance, an overview in Danish is given here: https://danmarkshistorien.dk/leksikon-og-kilder/vis/materiale/hoveri/

[13] For example, Falch (2010) considers wage regulations for teachers in Norway, while Steiger et al. (2010) study monopsonic power in the labour market for nurses in the US. Other work has considered minimum wage regulation, for example Dube et al. (2007) who consider the effects on restaurants in San Francisco. More recently, Dube et al. (2020) find evidence of monopsonistic power in online labour markets.

adaptation to increased mobility restrictions could be quite different from those from the abolition of mobility restrictions. It is also important to note that most of the literature on monopsony does not focus on agriculture. Yet, theoretical work by Raimondos (1993) assert that there is monopsony in agricultural markets in developing countries. Méndez-Chacón and Van Patten (2020) examines the case of the *United Fruit Company* as one large firm with monopsony power in an agricultural setting.

Regarding empirical studies on serfdom, Table A1 in the online appendix provides an overview of the most important studies. Domar and Machina (1984) provide a quantitative analysis of whether Russian serfdom was unprofitable by investigating whether the prices of serfs were similar across regions and different from zero in the 1850s. They observe some differences across regions, but in most places, prices were positive. This indicates that serfdom was not unprofitable. Klein and Ogilvie (2016) link the intensity of serfdom across Bohemian villages and show that there is a single peaked relationship to non-agricultural occupational activities. In relation to the empirical analysis, Klein and Ogilvie (2016, p. 509) use "*the presence of village holdings currently used or occupied by the landlord*" as a proxy for the intensity of serfdom. They argued that manorial presence on village holdings captures the intensity of landlords' interest, information, and control over villagers' economic choices.

Ashraf et al. (2018) build a model to explain the abolition of serfdom based on the idea that skilled labour became more important for fostering the return to physical capital. This led elites to abolish labour coercion in favour of employing free skilled workers. They show that Prussian regions with more water mills as their proxy for physical capital had fewer serfs. Their measurement of serfs is based on emancipation from servile duties. None of these works relate directly to restrictions on mobility.[14]

Nafziger (2012) and Buggle and Nafziger (2021) study whether serfdom had effects even after abolition in the Russian case. Nafziger (2012) finds relatively modest negative effects on mobility out of agriculture, whereas Buggle and Nafziger (2021) find evidence on long run persistent effects on well-being and economic development using Russian data. Our study speaks to the immediate effects of serfdom on labour mobility and not the long-run effects. Malinowski (2016) studies the case of Poland

[14] Ogilvie and Edwards (2000) analyse data for Bohemian villages, but do not consider the effects on wages.

and defines serfdom as being present in areas in which peasants did not enjoy legal protection from the state against their landlords. He finds that serfdom defined in this way is associated with higher urban growth in the face of market disintegration. Finally, Markevich and Zhuraskaya (2018) use a difference-in-differences strategy, which exploits the variation produced by abolishing serfdom and the intensity of serfdom across Russian regions.[15] They find increased agricultural productivity, industrial production and peasant nutrition after serfdom was abolished.

Unlike the studies reviewed above, we provide an evaluation of the effect of the introduction of serfdom on wages. In addition, we use micro-level data rather than more aggregate provincial or regional level data, and we focus on a case from Western Europe.

While our study is related to the literature on serfdom, we mentioned in the introduction that some contributions have considered other coercive labour market institutions. We have made an overview of the most relevant studies in Table A2 in the online appendix. The studies by Naidu (2010) and Naidu and Yuchtman (2013) are closest to the present work in the sense that they also apply difference-in-differences estimations and consider restrictions on mobility in different settings. Naidu (2010) estimates the effect of anti-enticement laws on wages by using US state-level data. He also employs individual census data to estimate the effect on the return to experience. He finds negative effects on wages of mobility restrictions. Naidu and Yuchtman (2013) employ county-level wages to estimate the effect of abolishing criminal sanctions on breach of contract. They find a positive effect of wages. The study by Bobonis and Morrow (2014) consider education as the outcome, and find that education did not respond to changing coffee prices during the coercive regime, whereas increased coffee prices in the non-coercive regime decreased education. Dippel et al. (2020) consider data for British colonies in the West Indies and find that higher sugar export shares are associated with lower wages of agricultural workers and higher sugar prices associated with higher wages. Gupta and Swamy (2017) consider migration to the Assam plantations in India and the association to the price of tea. Higher tea prices are

[15] As explained in the previous subsection, the studies of Russian serfdom examine a variety which has many elements such as serfs being the property of landlords as well as mobility restrictions.

associated with higher migration, but if more coercive contracts are used in a district, this response is weakened. The latter two papers consider workers who are mobile. We emphasize that none of these studies employ micro-level wage data to investigate the effect of the introduction of a coercive policy.

3. Historical background

This section briefly details the history of serfdom in Denmark to inform our investigation of its labour market implications. We first discuss the early serfdom which applied to only part of the country (the eastern islands of Zealand, Lolland and Falster). Next, we discuss the introduction of serfdom over the whole country in 1733. Finally, we discuss the changes made to serfdom during the eighteenth century.

Early serfdom – vornedskab

At the end of the fifteenth century, a form of serfdom termed "*vornedskab*" was established on the eastern islands of Denmark.[16] As is also true for the 1733 serfdom, it was directed at male farmhands. Christensen et al. (1934, p. 40) note that the sons of farmhands were tied to the same estates as their fathers, and were thus not at liberty to move, although they were not the property of the landlord. Thus, if the farmhand was able to raise enough money to pay a fee, he could be allowed to work elsewhere. Nevertheless, while farmhands under *vornedskab* were not slaves, Christensen et al. (1934) mention that the buying and selling of farmhands by landlords did in fact take place. While there were earlier attempts at abolishing this form of serfdom, it was not until 1702 that this was finally enacted,[17] and then only for children born after August 25th, 1699. This leads Munch (1974, p. 308) to conclude that the abolition did not have any effect until 1717, when the first free workers were able to enter the labour market.[18]

[16] Henriksen (1995) discusses some of the factors behind the adoption of *vornedskab* on the eastern Islands. She emphasizes that the estates were larger on Zealand and the other eastern islands. Yeoman did not exist on the eastern islands. Grain production required more labour input and was more common on Zealand compared to Jutland, which focused more on cattle feeding.

[17] This was done by a royal decree of February 21st, 1702, see Holmgaard (1999, p. 130), who also provides a discussion of the process that led to abolition.

[18] In a similar vein, Løgstrup (2015) concludes that *vornedskab* was gradually abolished as those subject to it passed away.

Reintroduction of serfdom

Serfdom was reintroduced in 1733 for all males in the age group 14 to 36 years old, and this time for the entire country.[19] More precisely, serfdom was reintroduced on February 4th, 1733, when a royal decree declared that any peasant of age 36 or younger was not allowed to leave the estate on which he was born if the landlord had work for him.[20] From 1735, it was made clear that the intended group was from 14 to 36 years of age, although the original decree might have signified that originally by using the word "*bondekarl*" (peasant), which indicates someone who is clearly not a young child (Holmgaard, 2003, p. 28).[21] The reintroduction was mainly aimed at ensuring farmhands for the estates, but in principle every male peasant in the age group 14 to 36 years was now tied to an estate.[22]

According to Olsen (1933, p. 63), the main motivation was to ensure low wages in the agricultural sector as run by the estates. Prior to 1733, farmhands had become freer at least *de jure* (Olsen, 1933, p. 64) due to the abolition of the early serfdom, *vornedskab*, on the eastern islands. Yet, in the western part of the country, farmhands had not been limited in their mobility prior to 1733 to the same extent. The introduction of serfdom has also been associated with scarcity of labour as compared to land (Holmgaard, 2003), in line with earlier work by Domar (1970). Yet, serfdom was introduced by the political system, which Domar himself acknowledged is not in his model. Moreover, as pointed out by Simonsen (1988), Danish historians have in general been rather dismissive of the complaints by the

[19] The reader may wonder whether other labour market legislation was in place. While this was not the case, Skrubbeltrang (1940) describes a royal decree of 1701 which states recommended nominal wages for men and women at the regional level in the countryside. From his discussion of various other wages, these recommendations were not followed across all regions. This shows that no minimum wage was in place.

[20] The original decree states that "No male peasant can move from the estate where he is born, as long as his master can provide him with work, unless he is enlisted in that year or… his time in the rural militia has been served, but when he has completed his service, which he will not receive payment for, he must legally terminate *his service and can subsequently take work anywhere in the country he so desires, unless he is from the old serfs* [Vornede], or has neglected his farm, but in other situations this absolutely must not be denied, on the payment of 50 rigsdaler mulct [which was a type of payment], half of which to the peasant and the other half to the regiment." The Danish reads "*Bonde-Karl*", which is somewhat ambiguous, but was interpreted to mean any male of a certain age living in the countryside.

[21] In 1735, it was made clear that those who had moved to another estate before 1733 on the basis of a valid passport were bound to the estate at which they had residence, see Løgstrup (1987a, p. 36).

[22] This is different from serfdom in e.g. Russia in which roughly 43 per cent of peasants were privately owned serfs (Markevich and and Zhuraskaya,2018) and the remaining 57 were formally free (relative to serfs) in 1858. All peasants in the age group 14 to 36 years became adscripted in Denmark.

landlords regarding labour scarcity and have rather interpreted this as an attempt to repress wages. Nevertheless, if the agricultural crisis or the end of *vornedskab* were creating conditions of labour scarcity, this should have shown up in wages, for which reason we investigate below whether there were early impacts on the wages of farmhands prior to serfdom. Another theory put forward by Holmgaard (1990) suggests that the introduction of serfdom is related to the (temporary) abolition of the country militia in 1730. As young peasants were no longer required to serve, some seem to have decided to be drafted into the army rather than work on an estate (Holmgaard, 1990: p. 248-250). On the other hand, the country militia is also believed to have caused young peasants to flee the country, so in principle its abolition could have increased the number of available farmhands.

Whatever the case, a process during which it was argued that the freedom of the peasants had to be constrained was set in motion, culminating in the introduction of serfdom in 1733, which was formally enacted by re-establishing the country militia.[23] This came with the requirement to keep a register of all males of the ages 14 to 36, which could be used to keep track of the farmhands in the relevant age group (Skrubbeltrang, 1978, p. 185).[24] Since the landlord could choose to enrol anybody who was not a tenant farmer in the militia (which by all accounts was not an experience most people desired), it has been argued that this threat was an important means of control for the landlords, allowing them to pressure young farmers into taking over otherwise undesirable or poorly maintained vacant farms (Skrubbeltrang, 1978). In fact, even stronger forms of serfdom in which the peasants would become the actual property of the estate owners were also proposed to the king (Holmgaard, 1990: p. 259), giving some support to the idea that the introduction of serfdom was mostly driven by a desire on the side of the estate owners to reduce the mobility of its labour force and increase monopsony power, rather than actual labour scarcity.[25] Figure 1 shows the data for farmhands and other occupations for

[23] The discussion in Skrubbeltrang (1978, p. 184-189) suggests that military needs may have had some influence for the reintroduction of serfdom as it also included a reintroduction of a rural militia. Løgstrup (1987a) views the re-introduction of the militia as driven by a combination of the interest of the minister of war and estate owners. Yet she notes that the rural militia only affected about 6,000 men, whereas all men from 14 to 36 years were bound to the land.

[24] We do not have access to these registers, but to estate level data as described below.

[25] Our analysis pertains to geographical mobility, but one may also wonder about the mobility between professions. Løgstrup (1987a) discusses the limited evidence on this. There is no systematic evidence on farmhands moving to other rural professions, but Løgstrup indicates that there are cases of farmhands becoming farm owners, but nohing is said about moving to other rural professions. In terms of social mobility, Løgstrup (1987a) presents evidence from the city of

the period from 1705 to 1799. We plot the (average) log of wages for both groups and the difference in log wages between the two groups. Figure 1 suggests that although farmhands experienced average wage increases prior to serfdom, and after the abolition of the country militia, so too did those of other groups.[26] However, with the re-introduction of serfdom in 1733 until the end of the 1740s, farmhands appear to have experienced substantial decreases in wages compared to other groups.[27] While the raw data fail to control for composition effects within the groups, the visual impression suggests nevertheless that average wages among farmhands did fall. We test more formally for an effect below.

<Figures 1 and 2 about here>

The reintroduction of serfdom has also been associated with falling agricultural prices following the Great Northern War of 1709-1720 (Christensen et al., 1934; Feldbæk, 1982; Dombernowsky, 1988) which turned into an agricultural crisis leading farmhands to leave the estates.[28] When we plot our data for the (natural logarithm of) barley price along with (the natural logarithm of) farmhand wages in Figure 2, we find that in the aggregate, grain prices were falling in the years prior to the reintroduction of serfdom.[29] Yet, we note that the barley price was starting to increase from the 1730s. Moreover, barley prices were increasing in the 1730s at the same time as farmhand wages were decreasing. We also note that barley prices seem to be slowly increasing in the 1740s. Olsen (1950) argues that the 1740s was a period in which demand for grain was increasing, and this may arguably also be the reason

Aalborg on the family background of merchants. Out of 108 only 2 had farmers as fathers. She also discusses a reform that meant that it became easier to get title as a lawyer from 1736. Only 17 out of 853 lawyers in the period 1736-63 had fathers who were farmers. This suggests a society with limited mobility.

[26] Figures 1 and 2 reveal that farmhands had higher wages than other groups at times prior to the reintroduction of serfdom. We note that empirically the evidence is in line with parallel trends. Also note that the data used for constructing average wages are discussed in detail in Section 4, but it should be noted that we are comparing nominal wages of the two groups. Given that we are comparing wages within the same country, the two groups would face similar prices.

[27] We note that the effect is only visible after some time. This is also true when we look at estimates that control for the composition of groups and year fixed effects among other control variables. This could be due to the fact that it took time before estate owners understood the new rules, which initially were unclear as mentioned in the text and in footnote 18.

[28] The Great Northern War was a war between Sweden and most other northern European countries and is either dated from 1700-1721 or 1709-1720. Denmark was allied with Poland, Prussia, Russia and Saxony. Major battles include a failed attempt by the Danish army to recapture Scania in southern Sweden in 1709-1710 and the defeat of the Swedish army in Tønning, Schleswig in 1713. Following a series of battles with no clear winner and the Swedish king Karl XII's death, peace was made on February 1, 1720 (Jespersen 1989: pp. 306-315).

[29] Feldbæk (1982) and Dombernowsky (1988) plot more aggregate barley prices at the regional level. Dombernowsky stresses that it was mainly producers of grains that were hit by the agricultural crisis and to a lesser extent those specialized in animal products.

that farmhand wages experience a rebound in the 1740s, yet he also mentions that the cattle plague in the mid-1740s and late 1750s may have exacerbated this increase. This suggests that it is unlikely that agricultural prices were driving the reintroduction of serfdom, yet they could have driven wages.[30] Therefore, we will control for the barley price in some estimations.

In Figure 1, we compare to other occupations than farmhands. Given that serfdom affected everyone in the labour market, this may raise the question as to whether this is a good control group. To address this, we plot the wages of farmhands and compare them to unskilled labourers from the province Scania in Southern Sweden in Figure 3. Scania was under Danish rule until 1658 and shared a long institutional history. Scania and Denmark also have similar climate and soil conditions; and language and culture were similar.[31] Both countries were subject to absolutism in the eighteenth century, while serfdom obviously represents an institutional difference. Moreover, Scania's agrarian ownership and labour relations were quite like those in Denmark, with significant landed estates and demesne production (Olsson and Morell 2010: p.324). There were no major changes in the way the Scanian labour market functioned after the change of nationality. On the contrary, the similarities with Denmark to a certain extent were reinforced; some Scanian landlords even tried to practice Zealand-style serfdom at their estates during the 1690s, long after Swedish law had been implemented (Fabricius 1958: pp. 112-113; Olsson 2003). Sweden has been characterized as having a "mercantilist labour market regime" during the period of study. The basic patterns prevailed during the whole eighteenth century, people without property or occupation were subject to *laga försvar*, which meant compulsory year-long service, at the same time as the right to quit for a new employer was emphasized. Moreover, the wages of day labourers were not regulated, whereas this was the case for servants' wages (Lundh 2002: 48–59; Uppenberg 2018: 113). Using the Scanian comparison group, we see that farmhands in Denmark suffered a reduction in

[30] The data shown are averages of the barley prices observed at the level of the estate. In Figure A1 in the online appendix, we show that a similar development is true for rye prices.

[31] *Encyclopedia Denmark* describes the soils of Scania as clay soils, which are also prevalent in Denmark. Moreover, the climate is said to be similar to the one on Zealand with winters beings slightly colder, see http://denstoredanske.dk/Geografi_og_historie/Norge_og_Sverige/Egne_og_byer_i_Sverige/Skåne. Concerning languages, Kirby (1990) notes that the linguistic difference between Danish and Swedish is small. Regarding institutions, both Denmark and Sweden transitioned to absolutism in the seventeenth century, see e.g. Lockhart (2004) and Johansen (2006).

the (log) wages from the 1730s, whereas this does not seem to happen for unskilled labourers in Scania. We note that wages seem to increase in the serfdom period in Scania, and this could suggest that the differences in wages are driven by other factors. Yet, when we make a real wage comparison, the real wage in Scania seems fairly constant, whereas the one for Denmark decreases, see Figure 4. Thus, while our main analysis will focus on data from Denmark, we show below that the results also hold when we use this comparison.

<Figures 3 and 4 about here>

As noted above, *vornedskab* was gradually abolished on Zealand, and so by February 1733, all farmhands of age 33 years or below were no longer subject to it (Skrubbeltrang, 1978). Moreover, farmhands whose parents had migrated from Jutland and Funen had been free of *vornedskab* (Skrubbeltrang, 1978, p. 178). This means that the effect of serfdom might plausibly have been smaller on Zealand as it would not affect all those aged between 33 and 36. We investigate this in our difference-in-differences analysis below and find that the effect is negative for both subsamples.[32]

Tightening and abolition of serfdom

From 1742, serfdom was tightened to the age group 9 to 40 years old. Further edicts from the 1740s link the reduced mobility to the presence of cattle plague (Christensen et al., 1934: p. 75). From 1764, serfdom was further tightened to the age group 4 to 40 years. As part of wider agrarian reforms, serfdom reverted to the 1733 version in 1788 and was finally abolished in 1800.[33] Tightening the rules could have been an endogenous response to problems with for example runaways leaving the country. Yet, if we consider the 1742 changes, Holmgaard (2003, p.31) has noted that a main reason given by the estate owners for changing the rules was not that they were being broken, but rather that males younger than 14 years of age were leaving the estate in order to avoid serfdom. He also finds, however,

[32] An interesting question is the degree to which product and labor markets in Jutland and Funen are integrated with those on Zealand. Figures A2 and A3 suggest that barley markets are as prices move closely together. Moreover, the coefficient of variation (the variance divided by the mean) is very low for barley prices. For average wages, we observe that these move less closely and there is more variation across the country in wages.

[33] The agrarian reforms also included e.g. the enclosure movement, see e.g. Løgstrup (2015) or Lampe and Sharp (2018).

based on a review of primary evidence, that this was only happening to a very limited extent.[34] Returning to Figure 1, we do not see a clear effect of the 1742 tightening, but notice that the wage differential between farmhands and other occupations is at its lowest for the period 1742-1764. Yet, we also note that the tightening was a smaller shock compared to the initial one. Similarly, the data for the period 1705-1799 fails to reveal any visible effects of the 1764 tightening.[35]

4. Data

Here, we describe how we obtain our measure of individual wages as well as an indication of which individuals are unskilled farmhands. Fortunately, it turns out that for the eighteenth century, a vast amount of data was collected by the Danish Price History Project, which was started at the University of Copenhagen in 1939 and terminated in 2004. The wage data are referenced and briefly summarized in volume II of the *History of Prices and Wages in Denmark 1660-1800* (Friis and Glamann, 1958; Andersen and Pedersen 2004). The data come from manorial accounts (Andersen and Pedersen, 2004: p.16) and the period covered overlaps that when serfdom was reintroduced.[36] They represent a unique dataset on labour and product markets during that time. Jensen et al. (2021) details how the wage data are harmonized at the individual level, and it should be noted that they have been corrected for in-kind payments (also documented in the data).[37] The Scanian data are documented by Gary (2018).

Given the extensive use of patronymic surnames, we are unfortunately not able to track individuals across time, but we have data for individuals working for a total of sixteen estates[38] across the regions

[34] Løgstrup (1987a) is more partial to the explanation provided by the estate owners for wanting to tighten the rules. She provides a few cases in which a family tried to move a child from the estate before the age of four.
[35] We also note that there is no visible upward trend in wages of either groups in Figure 1.
[36] The wage data are daily wages. We note that Humphries and Weisdorf (2019) have pointed out that daily wage underestimate full year income and may therefore not be appropriate when e.g. dating the great divergence across countries. It could also be that serfdom changes contracts and wage rates. We have investigated whether our main result is driven by a change in seasonal pay and find that it is not.
[37] We control for in-kind payments in some estimations. Skrubbeltrang (1978, p. 247) discusses money wages in different part of the country and notes that they were likely to be tied to grain prices given that payment in kind was common. Yet, he gives several examples in which money wages were paid to day labourers.
[38] Andersen and Pedersen (2004, pp.44-96) provide a brief discussion of each manorial account. While the accounts are of varying quality, they detail wage payments and the type of work being performed. According to Jørgen Mikkelsen of the Danish National Archive (*Rigsarkivet*), the accounts only include the transactions of the estate and not the transactions of tenant farmers (personal correspondence).

of Funen, Jutland and Zealand in the full dataset covering 1705-1799: see Figure 5 for a map of Denmark with the estates marked and Table A3 in the appendix for a list of the estates. We restrict ourselves to these years since the data are very thin before and after. Thus, the dataset consists of repeated cross-sections available at an annual level. For measuring whether a person is a farmhand, we use the fact that we have information on occupation, which we have coded according to the HISCO system (van Leeuwen, Maas and Miles 2002). We code as farmhands those who are designated as "farm labourers", "day labourers" and "labourers"[39] as our baseline, but also consider specifications in which we only use "farm labourers", which arguably captures most closely those who worked the field.

Table A4 summarizes the available data for the three regions of Jutland, Funen and Zealand. While we have observations for all three regions in the four periods, eight estates do not have any observations in the period which contains the reintroduction of serfdom (1705-1741) and we therefore only have 731 observations for this period. The data become thicker over time and are thickest for the period 1764-1787. The Danish price history does not provide detailed descriptions of who these labourers were, but it indicates that the day labourers working in the field were usually cotters renting a house, often with a small field. They had to work as farmhands with a contract for 6 or 12 months. The day labourers would receive money wages and if they worked on medium sized farms, they would typically receive payment in kind (Andersen and Pedersen, pp. 19-20). Skrubbeltrang (1978, p. 143) mentions that threshers would not be subject to the contract requirement and could be hired at any time. The data may also include farmhands who were allowed to work on other estates (Olsen 1950) for wages on a temporary basis. However, we stress that given that landlords had the right to make the local peasants stay to farm the land on their estates, the wages of those who could move even temporarily were also likely to have been affected. In Table A5, we provide a list of the 30 occupations of which we have at least 100 observations in the dataset, to provide an impression of other occupational

[39] Farm labourers perform a variety of tasks in growing crops and breeding and raising livestock according to the HISCO classification scheme. Labourers perform lifting, carrying, stacking, shoveling, digging, cleaning and similar tasks by hand, using simple labouring tools such as pick, shovel, wheelbarrow and street broom where necessary. Day labourers perform the same range of tasks as labourers using the same types of tools as a labourer (with codes 9-99.10), but are specifically hired and paid by the day.

categories,[40] which include craftsmen such as bricklayers and carpenters, as well as teachers for example.

<Table 1 about here>

Besides occupations and wages, the data also provide information on gender, the season of the year that the work was carried out, job title (master or ordinary craftsman), whether the individual was a child, and the location of the individual as given by region and estate.[41]

<Figure 5 about here>

5. Empirical strategy

We employ a difference-in-differences estimation on a dataset covering the period 1705-1799. We estimate a standard difference-in-differences model, which treats the reintroduction of serfdom as a single period (1733 to 1799). We proceed by estimating the following equation for the period 1705-1799 for the natural logarithm of wages as denoted by $ln\ w_{it}$:

$$ln\ w_{it} = Farmhand_{it} Serfdom_{1733}\beta + \alpha_t + X_{it}'\gamma + \varepsilon_{it}, \quad (1)$$

where *i* indicates individual and t indicates year. $Farmhand_{it}$ is a dummy variable that indicates whether the individual observed is a farmhand, and finally $Serfdom_{1733}$ is a dummy which is equal to 1 from 1733-1799 and which is equal to 0 otherwise. The parameter of interest is β, which measures the impact of serfdom on the farmhands and can be interpreted as a measure of monopsonistic power of the manor.[42] α_t indicates year fixed effects. X_{it} is a vector of control variables which include fixed effects for occupations (as described above), region fixed effects, gender fixed effects, seasonal fixed effects, and other control variables. ε_{it} is the error term. We include region fixed effects since the literature argues that the group of estate owners must have comprised a kind of employer organization

[40] There are 75 occupational codes in the dataset in total.

[41] For descriptive statistics see Table 1.

[42] The coefficient β can be interpreted as a negative measure of the monopsonistic power, being the degree of this measure usually computed as a ratio of the wedge between the value of the marginal product of labour and the wage effectively paid and the latter measure (see, e.g., Boeri and van Ours, 2021).

at the regional level, and they may have had some agreements on wages (Andersen and Pedersen, 2004: p.46). Similarly, Skrubbeltrang (1940, p.239) writes that differences in wages were *regional* and that differences between estates in a region were minor. Andersen and Pedersen (2004) further argue that there must have been competition between estates to secure the most necessary and, especially, the best labourers even in the serfdom period. They note that "the son of a wealthy copyholder might secure a passport to leave if he had received a better offer from a neighbouring manor" (p. 47).[43] Andersen and Pedersen (p. 90) further argue that "*[…] a kind of bargaining did take place, where wages were determined in a process of negotiation between non-equals: a process which, all the same, even the most assertive landowner ignored at his peril, despite being backed by an absolutist government. Adscription or not, no manor was an island, and neither was it a forced labour camp where manorial authority could dictate wage levels. At the most basic level, labourers had to survive physically, particularly in periods of rising prices.*" Thus, we believe there are good reasons to assume that labour markets would have strong regional components. Yet, we do explore specifications with estate fixed effects. As mentioned above, we control for fixed effects for years and occupation. This means that we exploit differences across *time* combined with differences across *occupations*. By controlling for year fixed effects, we capture any overall impact of introducing serfdom, and by controlling for occupation, we control for any time-invariant impact of being a farmhand. We then ultimately exploit that there is a differential impact of serfdom on farmhands. The crucial identifying assumption is that log wages would be on parallel trends for farmhands and other occupations in the absence of serfdom. We therefore present event studies to evaluate the plausibility of the common trend assumption. The event studies show the estimated coefficients on the interaction between a dummy for being a farmhand and time dummies. We also assume that the introduction of serfdom is the only relevant shock for understanding the changes in relative wages. Clearly, apart the aforementioned shock to grain prices, which we control for below, other shocks to institutions might therefore be a threat to identification,

[43] Unfortunately, according to Løgstrup (1987a) little systematic evidence on passports exists, as passports would allow farmhands to leave for better paid jobs at other estates. Yet, the available circumstantial examples of prices suggest that they could be quite high. Løgstrup (1987a) gives an example of a price of passport of 300 *rigsdaler* for someone who were acquiring a farm which cost 400 *rigsdaler*. If the presence of a passport would mean that more productive workers could mov to a better paid job in the region, this would tend to lower the effect of the serfdom.

but we are not aware of any which would have differentially affected farmhands. Yet we note that year fixed effects capture common shocks.[44]

We estimate our main equation on the data for Denmark only using variation across occupations and time. Yet, as an alternative difference-in-differences strategy, we compare the wages of farmhands to those of unskilled workers in Scania, i.e. we estimate the above equation with Danish farmhands as the treated group and unskilled, Scanian workers as the control group.

6. Results

This section presents the results from the estimation of our equation of interest using our main dataset on Danish estates. All standard errors are clustered at the level of the estate or alternatively at the occupational level.

We begin by showing an event study in which we have divided the period 1705 to 1799 into 19 periods. The excluded period is 1705-1709, and all periods contain five years except the periods 1725-1732 and 1733-1734. We notice that the coefficients become systematically negative from the period 1735-1739, and significantly so in periods 1740-1744, 1745-1749, 1780-1784 and 1785-1789. Importantly, for the design there is no discernible trend prior to period six. When we implement the same design using Scanian unskilled workers as the control group, we obtain more significant results, arguably because the Scanian control group was not negatively affected. For this reason, we interpret the results using Danish data only as conservative.

In addition to the event study in Figure 6, we have also estimated the flexible model for males only (see Figure A4) a fully flexible model in which the farmhand dummy is interacted with year dummies (see Figure A5). Both give similar results to those we find in Figure 6. While the specification in Figure A5 is more extended, the results indicate that there are no negative and statistically significant coefficients prior to the re-introduction of serfdom in 1733, and the pattern of coefficients is such that most coefficients after 1732 are negative and significantly so in some of the years, see Figure A5. As we do not have many observations per year, the lack of significance is perhaps not surprising. As another way

[44] E.g. confirmation in 1739 (which made learning to read compulsory, Lampe and Sharp, 2018: p.44) and the cattle plague of the 1740s.

of estimating a flexible model, we have divided the data into regular five-year periods. We find a similar result to that with the yearly periods. Again, it is only for the periods that included the years of serfdom that we find negative and statistically significant coefficients, see Table A6 in the appendix.[45]

<Figure 6 about here>

We next discuss the main results as reported in Table 2. Column 1 shows a negative and statistically significant estimate of the interaction $Farmhand_{it}Serfdom_{1733}$, which is consistent with the view that serfdom did indeed affect farmhands more strongly compared to other groups in the labour market. The estimate in column 1 implies that farmhands experienced wages that were 29.6 per cent lower than they otherwise would have been compared to those of other groups in the labour market after the introduction of serfdom. We have also calculated that a farmhand could have bought 5.5 pounds of rye bread before serfdom, but that this declined to 3.42 under serfdom, although this was in part due to increased prices. In columns 2 and 3, we show the effect of splitting the sample between those regions that did not have serfdom prior to 1733 (Funen and Jutland) and those that had the early serfdom, *vornedskab* (Zealand and surrounding islands). We find negative coefficients for both subsamples, though the effect is more precisely estimated for those regions that did not have serfdom prior to 1733. The effect is still negative for the Zealand sample, though precision is smaller.[46] In column 4, we add region by year effects separately. The estimate remains negative, but significance is reduced to the ten per cent level. The sign of the coefficient is nonetheless still negative, and we cannot reject that it is of similar magnitude to that reported in column 1. Since the mobility restrictions only applied to male farmhands, we check that the results are not driven by including women and children in the control group, and we find that this is not the case: see column 5 in Table 2. We have also tried to control for whether the worker received in-kind payments and find that this does not matter either: see column 6 of Table 2.

[45] We find some indication that farmhand wages were increasing in 1710-1719, but the effects are statistically insignificant. From 1720-1729, the estimated effect is very small. Thus, we do not find evidence of any pre-existing negative trend in farm hand wages using regular five-year periods.

[46] When carrying out the estimation on the data for Funen and Jutland only, we arguably better capture the effect of serfdom at the extensive margin. We note that all males born after August 25th, 1699 would be affected on Zealand. Yet, there was mobility restrictions on older cohorts, and for this reason, we can regard this as a shock on the intensive margin.

<Table 2 about here>

We next investigate the degree to which our results depend on the control group used, see more below. In Table 3, we first divide the control group into unskilled and low skilled on the one hand and medium and high skilled on the other hand. In columns 1 and 2, we find negative and significant effects for farmhands with both control groups. In columns 3 and 4, we use men only and obtain similar results. We also investigate whether including certain occupations in the control group drives the results. This would be the case if other groups were also affected by serfdom. We also consider removing occupations with a substantial number of observations in the data set. This is true for teachers who worked on the estates. As these may have been more mobile than other groups during serfdom, including them in the control group could affect the results. The result in column 5 suggests that this is only so to a limited extent, since the effect remains negative and significant, as well as numerically similar. Excluding carpenters, farm servants or masons also has little effect, and the same is true for farm servants, see columns 6-8. We further investigate effects on these groups below.

<Table 3 about here>

We have also re-estimated the baseline model on samples running from 1705-1741, 1705-1763 and 1705-1787, see Table A7. We note that the coefficient of interest is negative for the initial period before the first tightening, but it is statistically insignificant, see column 1. Once we extend the sample to 1763 as in column 2, the coefficient is very like the baseline estimate (see column 4) and is also significant at the five per cent level. The same is true when we extend the sample further to 1787, see column 3. The fact that we do not observe any effects using only the period before the rules were initially tightened could be related to the fact that the sample is reduced to fewer than 800 observations. Once we increase the sample, we get much stronger results. Below, we report results suggesting that the effects on farm labourers were stronger than for the other unskilled labourers we use in our measures. If we use the farm labourer category as the treated category and men only, we find that the coefficient is larger than for our main result and statistically significant for all sub periods, see Table A7. This suggests that the result for the initial period is driven by low power, since when we use those labourers treated more intensely, the effect seems to get stronger. This is also corroborated by the fact that we find a

statistically significant effect for the first period when we estimate our model on the full sample, as shown in Table 2.

We also investigate the effect of using the six estates which have observations for the period before and after serfdom, see columns 5 to 8 in Table A7. While the effects are similar, the size and precision of estimates change. Given that the sample size is reduced, we note the importance of including all 16 estates to obtain sufficient precision of estimates. Again, when we use men only and farm labourers, the results are stronger, see Table A8.

We next include additional control variables in Table 4. First, we investigate whether the price of grain (specifically barley, the most widely grown variety) could be the underlying factor driving wages and serfdom, see the discussion above. In column 2, we have included the average annual barley price observed at the level of the estate. If lower grain prices were driving serfdom and lower wages for farmhands, we would expect the coefficient to be positive. We find that the coefficient on (the natural logarithm of) the price of barley is positive, but not statistically significant. Moreover, although the coefficient on the interaction between the farmhand and the serfdom dummy is reduced compared to the baseline, it remains significant.[47] Additionally, when we use the sample for which we have barley prices, we see that the lower coefficient is similar, see column 1. When we replace our measure of grain prices by the price of rye, the coefficient on the price of rye is negative and marginally significant at the 10 per cent level, see column 3. This result runs counter to expectation but is possibly driven by the fact that we have less complete data for this variable. Yet, these results suggest that our main result is not driven by differential developments in grain prices across estates.

We also investigated the extent to which our results are driven by other ways in which serfdom affected wage development. Our identification relies on serfdom being mainly targeted at male farmhands. Other males could be restricted from moving as well, and so we control for other aspects of the

[47] We have also used the lag of the barley price and find similar results. Moreover, the correlation coefficient between the (natural logarithm of) price of barley and the (natural logarithm of) price of rye is 0.82. Our preferred measure of grain prices is the one for barley as there are more observations available for this grain. This result seems counterintuitive as this would imply that it was higher prices that drove wages down. The coefficient on the serfdom interaction remains similar, although precision is reduced.

environment. First, being close to the border might have implied that it was easier to run away as suggested by Holmgaard (2003). Being closer to a market town or the coast were associated with higher wages after serfdom, see columns 4 and 5. These results suggest that in areas in which it was easier to escape, wages did increase after serfdom. Yet, the effect on farmhands after serfdom remains negative and significant in all three columns. When we control for distance to Schleswig-Holstein (the region across the border in the south of the Jutland peninsula) interacted with the serfdom dummy in column 6, and contrary to what Holmgaard suggests, the coefficient is positive. Yet, this could be related to the fact that part of the region south of the border had a stronger version of serfdom than the kingdom of Denmark. In column 7, we control for all three distance variables, and find that our conclusions are largely unchanged, although the point estimate on the farmhand interaction is now smaller. We note also that the 95 per cent confidence intervals between the estimates from the different columns overlap.[48]

<Table 4 about here>

None of our distance variables, however, relate directly to competition between estates. Data from Boberg-Fazlic et al. (2020) on the location of 782 estates allow us to compute the number of estates within 10, 20, 30 and 40 kilometres from the estates in our data set. We use these data to compute a simple Herfindahl index with each estate getting equal weight.[49] The results are reported in Table 5. We note that the estimate of the main effect remains negative and mostly significant except when we consider the local market to be in a circle within 20 kilometres from the estate. Yet, the effect is still negative and significantly so when we consider 10, 30 and 40 kilometres. The main effect of competition is as expected as a lower value of the Herfindahl index indicates more competition. The interaction effects are positive. The interactions with the serfdom dummy suggest that less concentrated markets experienced larger reductions in farmhand wages. One interpretation of this result is that estates in

[48] We have also tried triple interactions with the distance variables and find that they are not significant.

[49] The formula is $\sum \left(\frac{1}{n}\right)^2$ where n is the number of estates within the different radiuses used. We take this as an approximation of the regular Herfindahl index, which uses market shares.

more concentrated markets gained more monopsony power after serfdom by cooperating with other estates on keeping track of workers and thereby reducing their bargaining power.

<Table 5 about here>

Next, we investigate the extent to which local innovation affects the results and the extent to which they affect the impact of serfdom. We consider five different variables here. The first variables relate to the adoption of a new field system known as koppelwirtschaft and a new crop, namely clover. Koppelwirtschaft was introduced into Denmark in the eighteenth century (Boberg-Fazlic et al., 2020). This system required that the cultivated area was divided into at least seven fields. With, for example, 11 fields, the fields would be used in the following way: (1) fallow, (2) wheat or rye, (3). barley, (4) rye, (5) barley, (6) oats with clover, (7) clover for hay, (8) clover for hay and grazing, and (9–11) grazing, see e.g. Bjørn (1988: pp. 35–37). We have collected data on the estates that introduced this field system. One measure is simply whether the system was introduced by 1782, whereas another measure used available sources to date the year in which it was introduced. Clover is often argued to have been introduced along with koppelwirtschaft though this was not always the case. As discussed in Dall Schmidt et al. (2018), clover increased both crop yields and dairy production and it could have affected wages. We have data on which estates adopted clover in 1775. As these innovations may have affected farmhand wages as well, we include interactions with these variables with the farmhand dummy. We have also coded dummies for the years in which the owner of the estate changed when new buildings were finished and when other improvements were made on the estate.

The results are shown in Table 6. In column 1, we show the result for controlling for adoption in 1782 of koppelwirtschaft and the interaction with the farmhand dummy. We find that the interaction has a positive coefficient, which is also significant at the 10 per cent level. When we use the time-varying measure in column 2, we obtain a similar result. This is in line with one view in the historical narrative that the introduction of koppelwirtschaft increased the wages of farmhands (Porskrog Rasmussen 2010). Others have argued that koppelwirtschaft relied on serfdom as establishing and maintaining an

11-field system was labour-intensive and therefore may have required more forced labour.[50] In column 3, we control for similar variables for clover adoption, but here we do not find any significant effects. In column 4, we add the dummy variables for owner change, building construction and other improvements. Owner changes are associated with lower wages in the year of the new owner. In column 5, we estimate a full model using the time-varying measure for koppelwirtschaft adoption. Results are similar to those in the other columns, and we note that our main variable is always significant at least at the 10 per cent level in these estimations. Still, it should be kept in mind that these innovations could be endogenous, and this should be kept in mind when interpreting the results in Table 6.

<Table 6 about here>

An important question that we only answer tentatively is whether the intensity of serfdom as measured by the share of farmhands affected koppelwirtschaft adoption at the regional level. To evaluate this, we use a pseudo panel approach explained below, and find that the share of farmhands after serfdom either negatively affected the regional share of estates that adopted serfdom or had no discernible effect.

The fact that serfdom was tightened and then relaxed subsequently might influence the result, and we therefore also estimate a model in which we take advantage of this:

$$ln\, w_{it} = Farmhand_{it} Period_1 \beta_1 + Farmhand_{it} Period_2 \beta_2 + Farmhand_{it} Period_3 \beta_3 + Farmhand_{it} Period_4 \beta_4 + \alpha_t + X_{it}{}' \gamma + \varepsilon_{it} \quad (2)$$

where $Period_1$is a dummy equal to one in the years from 1733 to 1741, $Period_2$ is a dummy equal to from one in the period 1742-1763, $Period_3$ is a dummy equal to one from 1764 to 1787, and $Period_4$ is a dummy equal to one for the period 1788 to 1799.

[50] Olsen (1933), for example, links serfdom with the adoption of *Koppelwirtschaft*, which in the Danish context was associated with the establishment of modern dairying (Lampe and Sharp 2018; Boberg-Fazlic, Jensen, Lampe, Sharp and Skovsgaard 2018). Dennison (2006) also highlights that serfdom may have led to innovation among Russian landholding magnates.

In column 1 of Table A9, we look at whether there were any differential effects across the periods in which the restrictions were either tightened or loosened. Overall, the effects for the four sub-periods seem similar and it cannot be rejected that they are the same.[51] This result may seem surprising, but one reason that we do not see changes to the effect (e.g. that it becomes larger when rules are tightened) is that farmhands are adapting to the rules. Moreover, we noted above that there might have been a positive shock to demand for farmhands caused by increased demand for grains in the second period. Yet, we note that when we use estate fixed effects, the data are consistent with a stronger adaptation effect, although as noted below using estate fixed effects likely absorbs part of the effect of serfdom. In column 2 of Table A9, we investigate whether there are pre-existing trends in the form of the abolition of the early serfdom. The event studies suggested there were none, but as this would start to matter from 1717, when the first farmhands who were free of early serfdom turned 18 (see above), we interact a dummy for 1717-1732 with the farmhand dummy and obtain a positive yet statistically insignificant estimate on this variable.[52]

In column 3 of Table A9, we show results when we let the sample begin in the seventeenth century and end in 1799 (i.e. including years for 1661-1665). Again, the result for the period of serfdom is similar to our baseline estimate. In column (4), we ask whether it matters that children are in the sample and find that it does not. In columns (5) and (6), we cluster at the level of the occupation in column (5) and use two-way clustering (for estate and occupation) in column (6). In column (7), we investigate whether differential effects on the three groups included in the serfdom measure are present.[53] It might be the case that using just those we know for sure worked on the field produces stronger results. We investigate this by allowing for different effects of the three types of labourer. When we do so, we find

[51] The F-statistic for a test of equality of coefficients equals 0.15. The lack of increase in the coefficients could be caused by the fact that we have limited observations for all subperiods. Alternatively, this could suggest that to obtain the same effect, serfdom needed to be tightened as farmhands found ways of avoiding serfdom as stressed by revisionist historians.
[52] As an alternative way of getting at whether the changes had separate effects, we have interacted the four different periods with the dummy for being a child to capture the changes to the groups being affected. None of the coefficients are significant at conventional levels.
[53] There are 75 occupations included in the baseline estimation. Using clustering corrections at this level alleviates the concern that the number of clusters is relatively low in our baseline estimations to some degree. We have also applied the wild cluster bootstrap technique by Cameron, Gelbach, and Miller (2008) to the two-way cluster model to take into account that we only have 16 estates and obtain a p-value of 0.086 for the serfdom variable.

that the coefficient on the farm labourer interaction is -0.41 and significant at the five per cent level. For day labourers it is -0.34, but only significant at the ten per cent level. Finally, for labourers the coefficient is -0.257 and significant at the five per cent level. These results are suggestive of some differences, though statistically speaking we cannot tell whether this is in fact the case, see column (7) of Table A9.

In Table 7, we investigate the extent to which unobservable estate characteristics drive our results. In the main analysis, we use region fixed effects, which capture larger regions. This is warranted for several reasons. First, the historical narrative suggests that labour markets were to some extent regional, see above. Second, in terms of institutional legacy, the larger regions capture the divide between regions which had the early serfdom and those which did not. Finally, we can observe data for all the regions before and after serfdom, which is where we get our time variation for our difference-in-differences estimation. This is not true for estates, as we observe only six estates before and after as discussed above. This means that some of the before and after variation will be captured by the estate fixed effects. Bearing this in mind, we nevertheless run the estimations with estate level fixed effects. In column 1, we allow the effect to vary by periods. The coefficients are negative for all periods, but only significant for the first period. This could suggest that farmhands adapted to serfdom or that other effects are distorting the result. In column 1, we show our baseline model and we note the effect is still negative, though imprecisely estimated. Yet, we note that the (adjusted) R squared hardly increases compared to the baseline model. In column 3, We allow the effect of serfdom to vary by type of farmhand and see that that there are negative effects for farm labourers and day labourers, which are both significant at the 10 % level. The effect is positive, but non-significant for labourers. In column (4), we investigate what happens when we compare farm labourers to all non-farmhand occupations. The coefficient on the serfdom variable is negative and significant at the 5 level. In column (5), we replace farm labourers by labourers and find a positive non-significant coefficient on the serfdom variable as in column (3). In column (6), we find a negative coefficient on the serfdom variable for day labourers that is significant at the 10 % level. In columns (7) to (9), we run regressions similar to the ones in columns (4) to (6), but now we let the coefficient vary by the four periods outlined earlier. We notice that the coefficients are negative for all periods for farm labourers and day labourers, and significant in periods

1 to 3 for farm labourers, and significant for periods 1 and 2 for day labourers. For labourers, the coefficients are mostly small and significant, but in period 2, the coefficient is positive and significant. As we argued, the fixed effects for estates likely capture some part of the negative serfdom effect, which would go some way in explaining insignificance. Yet, the positive effects in columns (5) and (8) could be explained by increased demand for grains in the period 1740s. If we control for log barley prices, the coefficient on the serfdom variable for labourers become negative though not significant, and the positive coefficient for the second period becomes insignificant. Moreover, controlling for log barley prices in the specification used in column (2), the coefficient becomes -0.124 and significant at the 10 % level.

Overall, our reading of the evidence is that estate level effects do not explain the results, but that controlling for them adds substantial noise and the results become more dependent on which samples and treatment group is used. This is hardly surprising as estate fixed effects will absorb a lot of the variation in our data since we only have six estates for which fixed effects for estates can be estimated based on before and after data. In sum, we conclude that the estimated effect of serfdom is not driven by unobserved estate level characteristics. Yet, we also note that with estate fixed effects, the estimated effect becomes smaller with time, which is consistent with farmhands adapting to serfdom.

<Table 7 about here>

Additional results

We report additional robustness checks in Table A9 in the online appendix. We investigate what happens when we replace farmhands by either teachers, bricklayers, or carpenters in the difference-in-differences variable in columns (1)-(3) of Table A9. We find that teachers had increasing wages after 1733, and to investigate whether any of the other occupations explain our main results, we run models which augment our baseline model by the three variables added in columns 1 to 3. As is clear, effects on other occupations do not explain those found for farmhands, see columns 4 to 6 in Table A10. We have also tried to include linear trends for these three occupations and find that results are

unchanged (see column 7) and controlled for them individually by multiplying the occupation by year fixed effects, see Figures A6-A8. This is further evidence that results are not driven by other groups being affected by serfdom.

The nature of our dataset does not allow us to follow the single individuals' dynamics of wage and personal characteristics over time and, thus, traditional panel techniques cannot be implemented.[54] We deal with this by following the pseudo-panel technique introduced by Browning et al (1985) and Deaton (1985). We create a synthetic data panel aggregating different cross-sections into cohorts having the same characteristics. Given the information available in our dataset, we create six different cohorts, c, based on the common fixed individual characteristics, i.e., the three regions[55] and gender, obtaining an average of 67 observations per group. Then, we estimate the following linear model:

$$\ln \bar{w}_{ct} = \overline{Farmhand}_{ct} Serfdom_{1733}\beta + \bar{\alpha}_c + \bar{\alpha}_t + \bar{X}_{ct}\gamma + \bar{u}_{ct} \,, \qquad (3)$$

where $\ln \bar{w}_{ct}$ and $\bar{X}_{ct}$ are the average in cohort c in year t of log wages and the control variables. $\overline{Farmhand}_{ct}$ is the share of farmhands, $\bar{\alpha}_c$ represents cohort fixed effects and $\bar{\alpha}_t$ represents year fixed effects.

Table A11 displays the results of the estimation based on the pseudo panel allowing for different fixed effects. Also, in this case, the β coefficient is negative and significant. Moreover, Table A12 displays the results for a model in which the share of estates adopting koppelwirtschaft at the regional level is used as the outcome. The results show that the effect of serfdom was either negative or close to zero.

Table A13 corrects the standard errors for spatial correlation, where log wages at an estate are allowed to correlate with the log wages of other estates placed within 40 kilometres. As can be seen from the table, this correction makes the results more significant.

[54] On the other hand, a positive aspect is that our repeated cross sections are not affected by the attrition problem typical of individual panel data.

[55] In this case, we assume that individuals do not move into other regions.

Alternative control groups: Urban unskilled workers from Copenhagen and unskilled workers from Scania

It is possible that using other occupations than farmhands bias the results as other groups in the labour market may have experienced a negative (or positive) effect on their wage because of serfdom. If the effect on the wages of non-farmhands was also negative, we would underestimate the effect on farmhands.

To investigate this possibility, we first compare the evolution of the wages of farmhands to the wages of unskilled workers from the Danish capital Copenhagen. The capital was not subject to serfdom, and so this group of workers would only be indirectly affected by serfdom in the sense that flows of rural workers would be smaller. The comparison suggests that the wages of farmhands fell relative to those of unskilled workers in Copenhagen as seen in Figure 7, which shows the comparison from 1731 to 1799. It should be noted that we see both a decrease in farmhand wages and an increase in wages of unskilled urban workers. The latter increase could be explained by a lower flow of workers from rural areas.

<Figure 7 about here>

To further investigate this, we have also estimated a version of our model in which we compare the wages of farmhands to the wages of unskilled labour in Scania in Southern Sweden. As we have seen above, Scania shared some important traits with Denmark, with the crucial difference being that it never experienced serfdom. Most of the Swedish data come from the city of Malmö, but they also include some rural areas. Figure 5 includes the Scanian locations used, and we have already given a preview of these data in Section 3. For a detailed description, see Gary (2018).

We begin the analysis by showing the event study for log wages in Figure 8. This figure is like Figure 6, but we now compare to unskilled Scanian labourers defined by the same HISCO codes as those we use for farmhands. We note that a negative effect is visible in the period 1735-1739 and significantly so from the period 1740-1744. Moreover, the common pre-trend assumption appears plausibly satisfied.

There are no significant differences prior to serfdom, and if anything Danish wages were increasing in the period just prior to serfdom. This would tend to work against us.

<Figure 8 about here>

In Table 8, we show the simple difference-in-differences estimates using three different ways to compare log wages. In column 1, we base the comparison on Danish to Swedish skilling. In column 2, we have used an exchange rate correction using annual data from Abildgren (2018). Since the Swedish currency depreciated against the Danish, the effect is smaller, but still negative and significant. In column 3, we have deflated the wages by consumer price indices from Abildgren (2018) for Danish wages and Edvinsson and Söderberg (2010) for the Scanian wages. We find the effect to be a little smaller, but still negative and significant. As shown in Figure 4, the Scanian real wage data exhibits fluctuations but no increases in contrast to the nominal wages. In columns 4 to 6, we estimate the same three models, but for a sample which included the areas which did not experience serfdom prior to 1733. The estimated coefficients are negative and significant at least at the ten per cent level. The same is true if we consider the regions which experienced *vornedskab*, see columns 7 to 8. The estimated coefficients imply reductions of wages from around 40 per cent to 63 per cent after serfdom in comparison to the control group.[56] These results are all larger than our baseline results, which is in line with the idea that most occupations were affected by serfdom suggesting again that the estimates based on using data for Denmark alone are conservative.

<Table 8 about here>

Discussion

All the results presented above are consistent with the view that farmhands were negatively affected by the introduction of serfdom and, more generally, that monopsony power can reduce wages. In this section, we discuss interpretations as well as mechanisms. We noted that serfdom has in part been viewed as a response to an agricultural crisis with falling prices beginning after 1720. Yet, we note that

[56] We have also tried to control for whether the area is urban and find that this has no impact on our results.

wages of farmhands were not significantly statistically different from other groups in society in the period 1717-1732, which marks the time that the abolition of *vornedskab* would have come into effect. We also note using annual variation that there were no discernible pre-trends.

Our results necessarily allow for several mechanisms as to why the wages of farmhands are relatively low. One mechanism is that farmhands could to a lesser extent exert their outside option by getting jobs at other estates, as compared to e.g. craftsmen. The relatively stronger mobility of craftsmen may be attributed to the fact that they could more easily find jobs outside the village due to their education (Løgstrup, 1987b), or they might have had better options if they were to run away and migrate to another country (Olsen, 1933). Olsen (1933, p. 75) argued that the young craftsmen tied to an estate could easily find jobs abroad and believed that they were relatively numerous among those that ran away. Another related mechanism is that the supply of apprentices coming to the cities from the rural areas would contract as also suggested by Olsen (1933). If serfdom prevented young men from moving to other occupations, this would tend to weaken their outside options. This could also lead to a shortage of craftsmen in both cities and the rural sector, which would mean that the relative wages of farmhands would decrease. Finally, this also speaks to whether serfdom encouraged more able workers in the rural sector to select out of unskilled farm work, which could possibly explain the result on wages presented above.

To obtain some suggestive evidence on this latter mechanism, we employ micro-level data for the city of Odense for which information on the birthplaces of apprentices has been coded. These data include information on what type of guild the apprentice joined (e.g., for shoemakers and tailors), covering the years 1700 to 1790 and yielding a total of 516 observations.[57] While we cannot employ a difference-in-differences approach in this setting, it is possible to test whether the probability the apprentice is recruited from the countryside declined from 1733. We do this by estimating the following linear probability model:

[57] We have data for 17 guilds. We do not have data points for each year meaning that for some years we have at least one observation, whereas for others we have none. Moreover, the data only contain information about the guild the apprentice joined and whether the apprentice had his origin in a town or in the countryside.

$$P(apprentice\ from\ country\ side = 1|x) = \alpha_{guild} + serfdom_{1733}\gamma + \varepsilon_{it}, \quad (4)$$

where the variables in x are α_{guild} and $serfdom_{1733}$. α_{guild} indicates guild fixed effects and γ indicates the effect of serfdom on the probability of recruiting from the country. $serfdom_{1733}$ is defined as above. Now γ<0 would be consistent with this mechanism.

We can also test whether the changes to serfdom mattered by allowing for separate coefficients for the four periods by estimating the following model:

$$P(apprentice\ from\ country\ side = 1|x) = \alpha_{guild} + Period_1\gamma_1 + Period_2\gamma_2 + Period_3\gamma_3 + Period_4\gamma_4 + \varepsilon_{it}, \quad (5)$$

where $Period_1$,….., $Period_4$ are defined as above.

The result of estimating these two equations is shown in Table 9. When we estimate the first model, we find that $\hat{\gamma} = -0.267$, which is significant at the one % level, see column 1 of Table 9. Although this could possibly indicate a general, negative trend for potential apprentices from the country unrelated to serfdom, we find that this is implausible given our evidence on rural wages.

For the second model, the coefficients are all negative and statistically significant, see column 2 of Table 9. Moreover, they are statistically different from each other. This suggests that changes to the affected age groups did matter for those who wanted to become apprentices. In particular, the tightening in 1742 seems to have made a difference, consistent with Holmgaard's (2003) argument that the reason for changing the age to 9 years old was that the younger residents of the estate left before serfdom would apply to them. As a final test of this, we investigated whether we observe an effect when we only estimate on data from 1733, and then code the dummy as 1 from 1742. We find that there is a negative coefficient, which is statistically significant at the 10 per cent level, suggesting that the observed pattern is associated with serfdom and not simply general trends. The tightening in 1764 does not seem to have changed much, however, and we find no detectable difference between the periods 1742-1763 and 1764-1790. This may (very plausibly) suggest that reducing the lower age from 9 to 4 did not matter much for the supply of apprentices. This is in line with the available information from the 1787 census which we used to investigate the age distribution of apprentices. The average age of

apprentices is 18.1 years in 1787. There is one apprentice of age 7 and about 13 per cent below 14. The census data, however, does not tell us the age at which the apprenticeship commenced. For the 1780s, we were able to combine the census data with the apprentice data from Odense and found that the average age when the apprenticeship started was 15. 46 per cent began before the age of 14, but again there was only one observation in which the apprentice started at age 7. These data are in line with the idea that the supply of apprentices was not strongly affected by reducing the lower age from 9 to 4.

<Figure 9 about here>

In contrast to what we typically find for the wage data, there are differential effects of tightening the rules. One reason we see effects with the apprentices could be that they are a small group so any shock could be easier to discern.

In sum, these results suggest that opportunities for becoming apprentices for young men from the countryside diminished after serfdom as well as the tightening of the rules. As mentioned, there are other plausible mechanisms, and while we do not know whether the effect on recruitment of apprentices from serfdom is more important than migration out of the country, its presence suggests that the mobility of farm labourers was, in fact, affected by serfdom.

7. Conclusion

This paper presents evidence that increased monopsony power can depress wages substantially through a case in which elites increased monopsony power by limiting the mobility of workers. We further address the tendency of revisionist historians to argue that serfdom had limited effects, due to the presence of runaways, as well as other ways of leaving estates. We thus offer new quantitative evidence on the impact of serfdom on labour markets by exploiting unique individual level data for eighteenth century Denmark. We find a considerable negative effect on the wages of farmhands, consistent with the view that serfdom did matter for the mobility of workers and the labour market in general, and this is robust across a variety of specifications. This result is confirmed when we compare farmhand wages to the wages of unskilled Scanian workers. We also present evidence of a potential mechanism using data on apprenticeships, which suggests a decline in the number of apprentices coming from the countryside during periods of serfdom. This implies that restrictions on mobility can

have considerable negative impacts on the labour market, might stymy human capital formation, and thus ultimately have a deleterious effect on general development.

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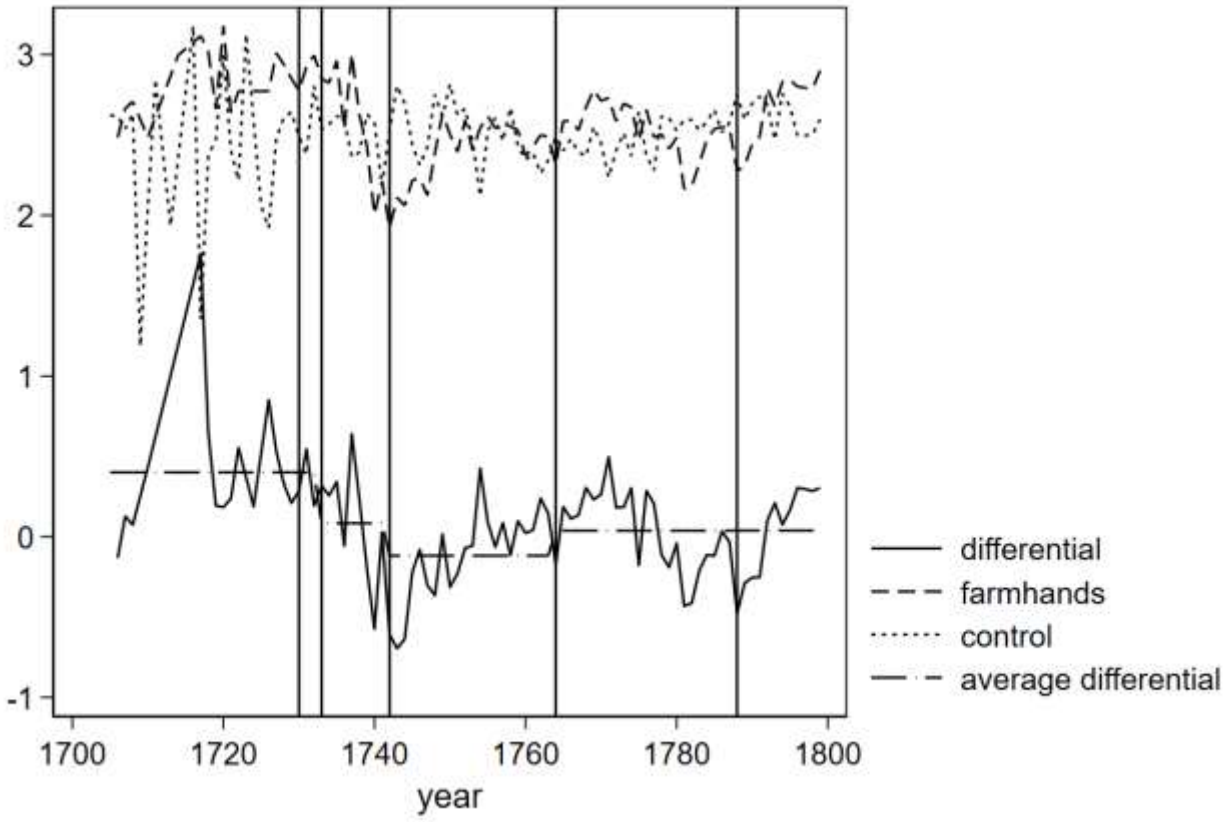
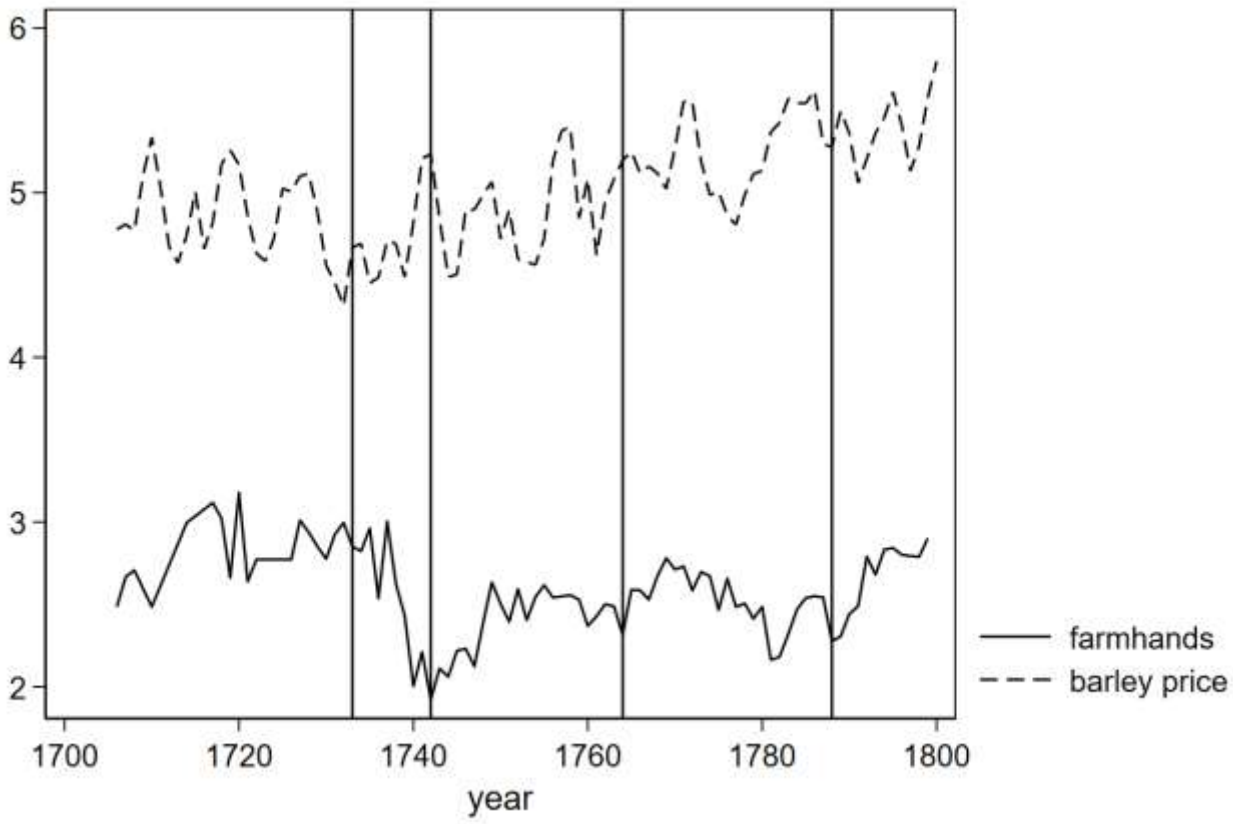
Tables and Figures

Figure 1: Average (log) wages for farmhands and other occupations, 1705-1799



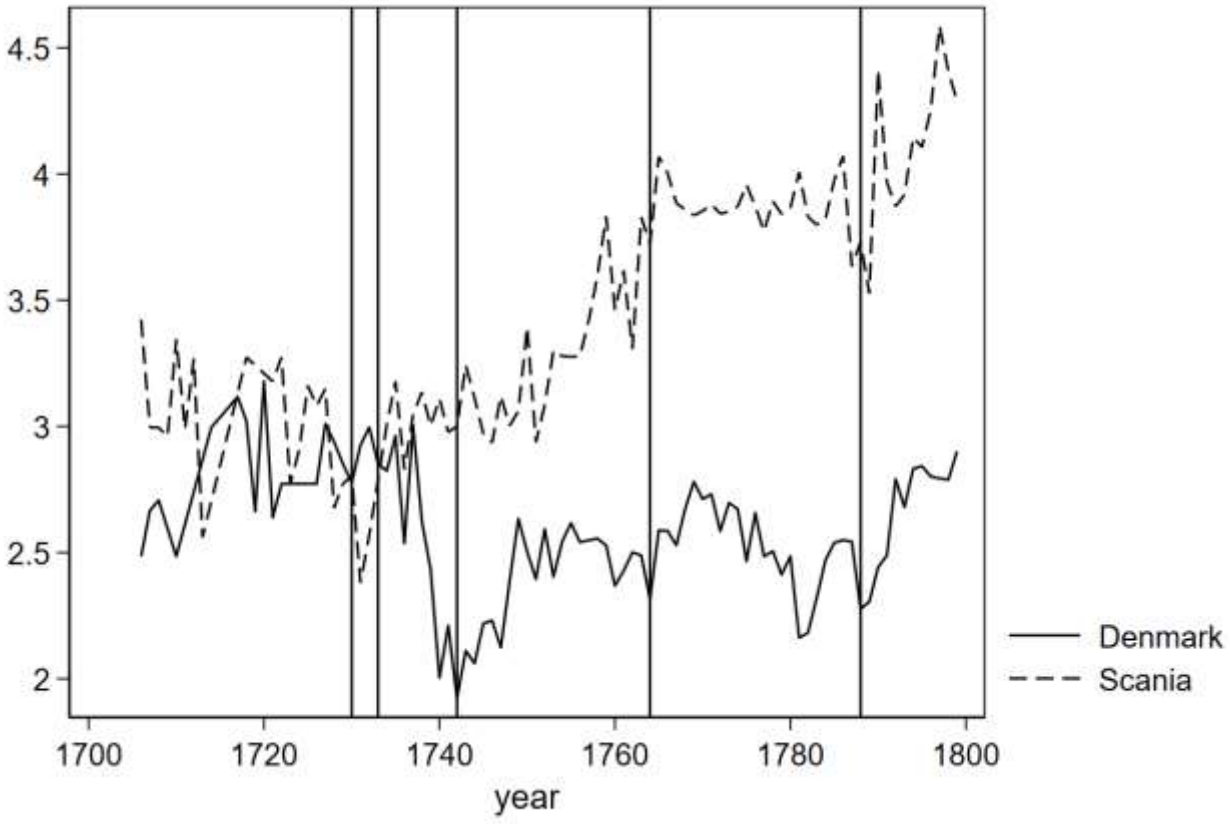
Notes: The variables on the second axis are the average natural log of wages for farmhands and other occupations (the control group) respectively as well as the difference between the two. The vertical lines represent 1730 (the year when the land militia was abolished), 1733 (the year serfdom was introduced), 1742 (the first year serfdom was tightened), 1764 (the second year serfdom was tightened) and 1788 (the year the rules returned to those of 1733) respectively. The horizontal line segments show the average difference in log wages for the periods before and after serfdom and the periods after which serfdom was tightened. The original wage data are nominal day wages measured in the Danish unit skilling.

Figure 2: Average (log) barley prices and farmhand wages



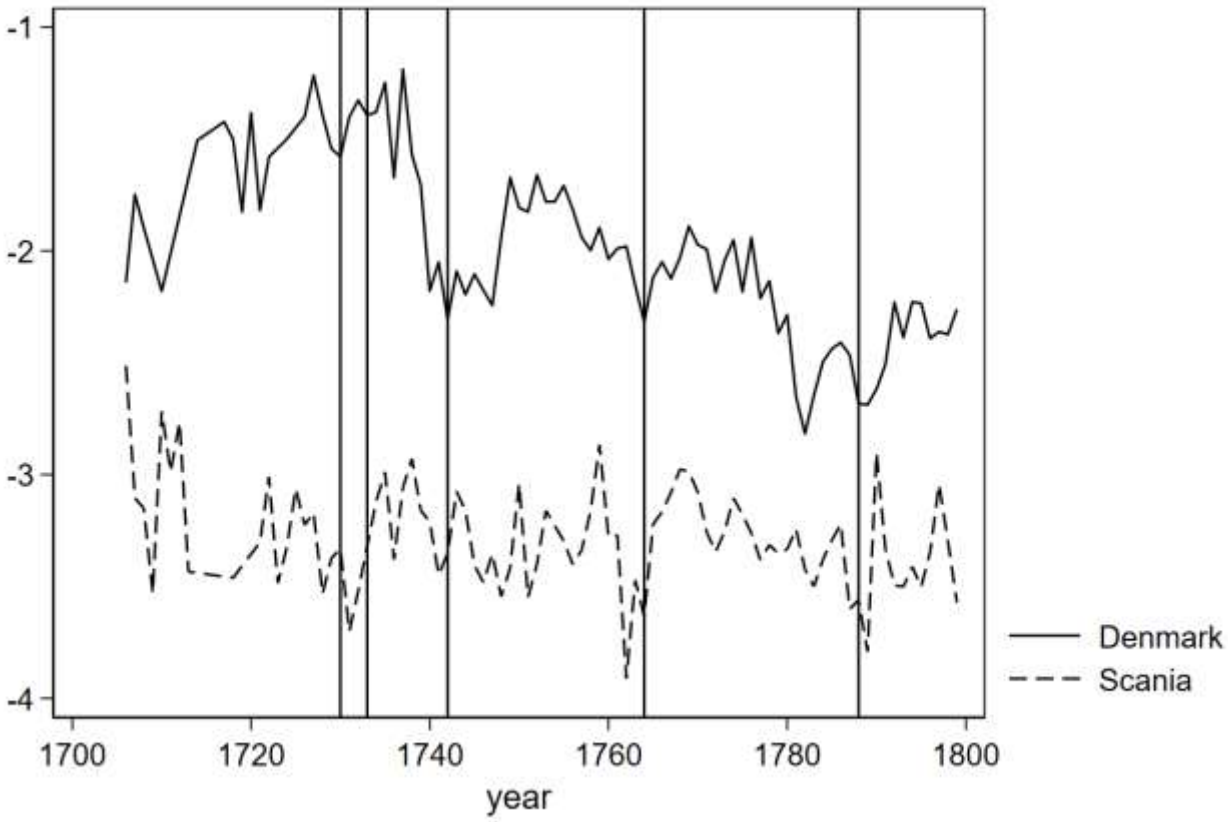
Notes: The variables on the second axis are the average natural log of the wages for farmhands and the average natural log of the price of barley. The vertical lines represent 1730 (the year when the land militia was abolished), 1733 (the year serfdom was introduced), 1742 (the first year serfdom was tightened), 1764 (the second year serfdom was tightened) and 1788 (the year the rules returned to those of 1733) respectively. The original wage data are nominal day wages measured in the Danish unit skilling.

Figure 3: Comparison of Farmhands and unskilled workers from Scania



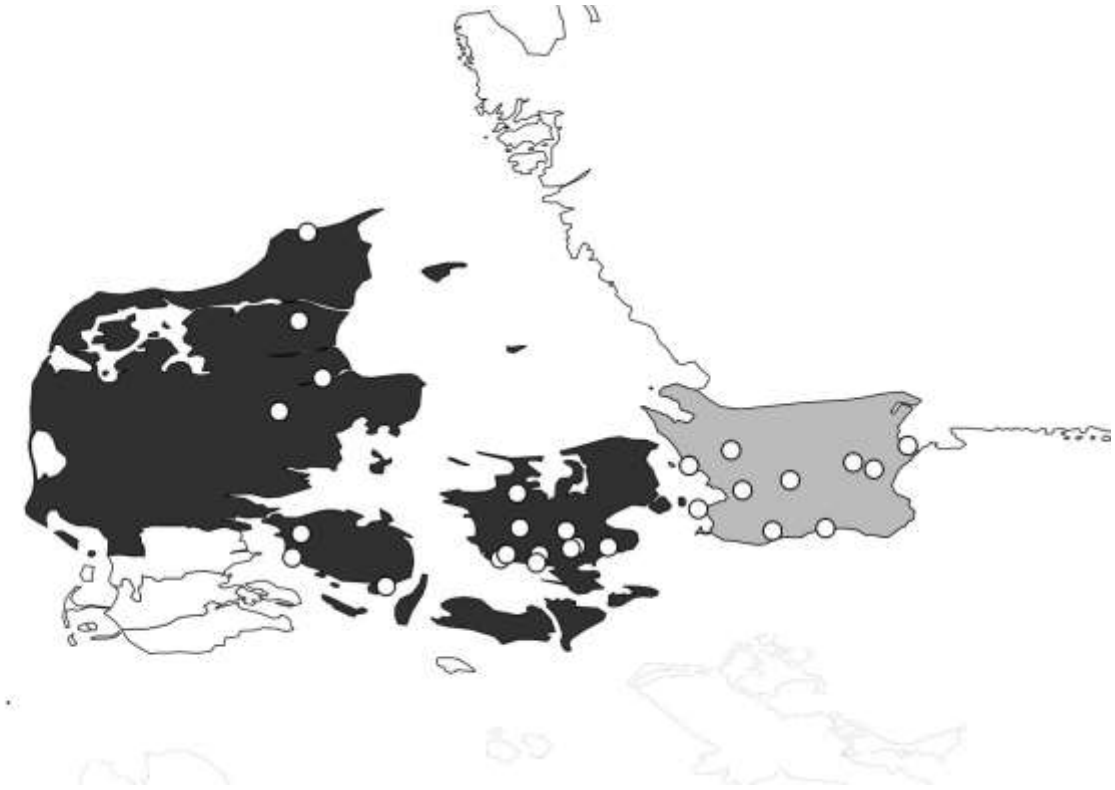
Notes: The variables on the second axis are the average natural log of the wages for farmhands and the natural log of the unskilled workers in Scania. The vertical lines represent 1730 (the year when the land militia was abolished), 1733 (the year serfdom was introduced), 1742 (the first year serfdom was tightened), 1764 (the second year serfdom was tightened) and 1788 (the year the rules returned to those of 1733) respectively. The original wage data are nominal day wages measured in the Danish and Swedish unit skilling.

Figure 4: Comparison of average log real wages in Denmark and Scania, 1705-1799



Notes: The variables on the second axis are the average natural log of the real wages for farmhands and the average natural log of the unskilled workers in Scania. The vertical lines represent 1733 (the year serfdom was introduced), 1742 (the first year serfdom was tightened), 1764 (the second year serfdom was tightened) and 1788 (the year the rules returned to those of 1733) respectively. The original wage data are nominal day wages measured in the Danish and Swedish unit skilling.

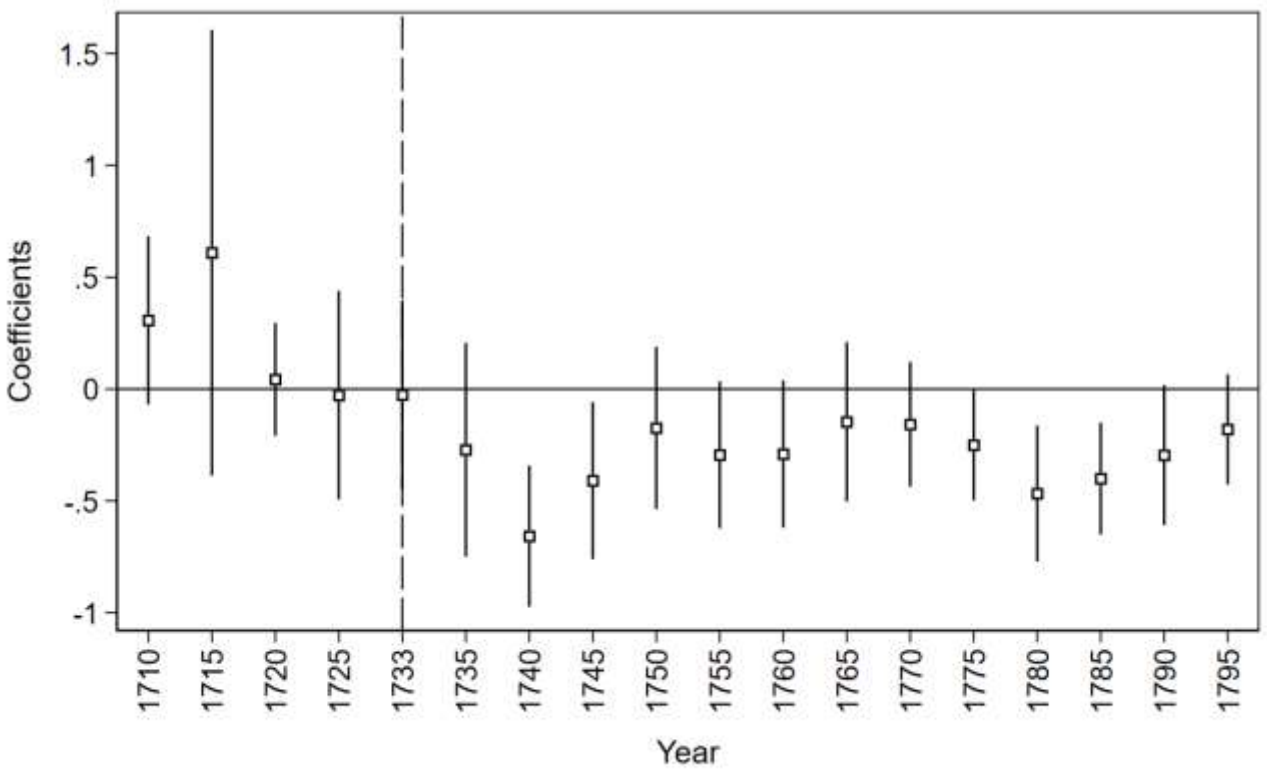
Figure 5: Map of Denmark and Scania with the location of estates in our sample



Notes: The blue dots identify the location of the estates and places in the Kingdom of Denmark and Scania in our dataset in a Geographical Information System map. The dark gray area identifies the Kingdom of Denmark. The light gray area identifies Scania. The white area to the south of Denmark identifies the region of Schleswig and Holstein.

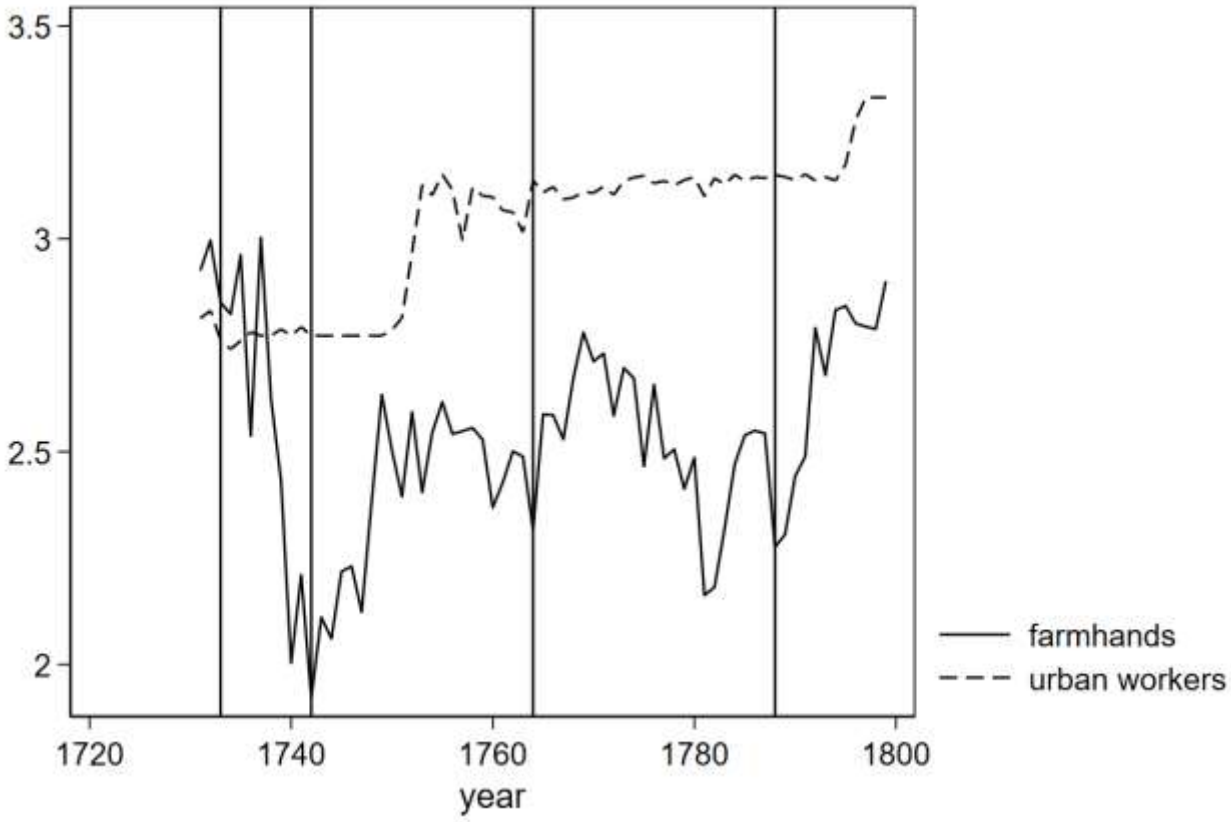
Source: Geographical coordinates collected by the authors.

Figure 6: Event study for the introduction of serfdom, coefficient on farmhands multiplied by time dummies



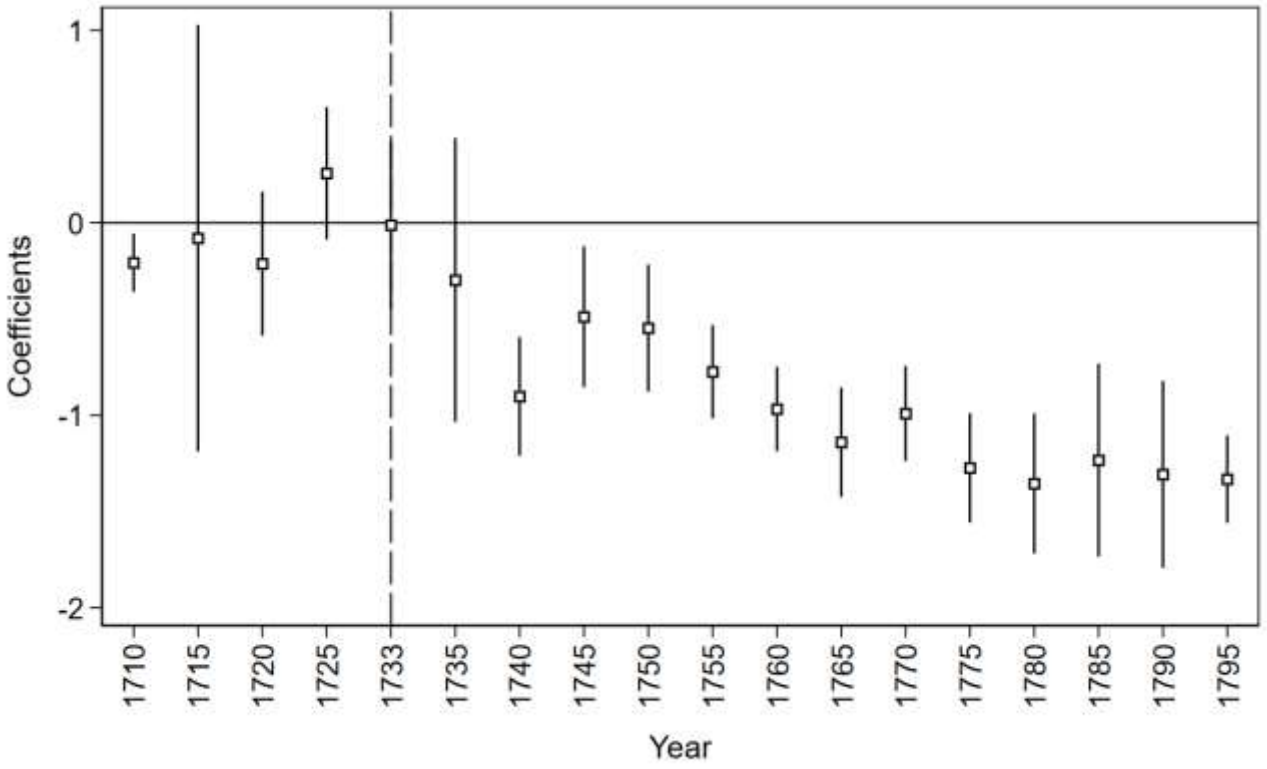
Notes: Coefficients obtained from the flexible-form with 95% confidence interval using the Danish dataset. 1705-1709 is the omitted year. The periods all include five years except 1725 (1725-1732) and 1733 (1733-1734).

Figure 7: Urban-rural wage differences, 1731-1799



Notes: The variables on the second axis are the natural log of the wages for farmhands and the natural log of the unskilled workers in Copenhagen. The vertical lines represent 1733 (the year serfdom was introduced), 1742 (the first year serfdom was tightened), 1764 (the second year serfdom was tightened) and 1788 (the year the rules returned to those of 1733) respectively. The original wage data are nominal day wages measured in the Danish and Swedish unit skilling.

Figure 8: Event study for the introduction of serfdom, coefficient on farmhands multiplied by time dummies



Notes: Coefficients obtained from the flexible-form with 95% confidence interval using the Danish and the Scanian data on farmhands and unskilled workers. 1705-1709 is the omitted first period. The periods all include five years except 1725 (1725-1732) and 1733 (1733-1734).

Table 1: descriptive statistics

Variable	#Observations	Mean	Std. Dev.	Min	Max
Individual level controls					
log (day wages)	20927	2.536	0.820	-2.526	5.951
Share farmhands	20927	0.287	0.452	0.000	1.000
Share of data from Jutland and Funen	20927	0.414	0.493	0.000	1.000
Share of women	20927	0.084	0.277	0.000	1.000
Share of children	20927	0.064	0.245	0.000	1.000
Estate level controls					
log barley price	18459	5.189	0.314	4.159	5.951
log rye price	17552	5.575	0.347	4.407	6.349
Distance to market town (in km)	20927	10.567	5.004	3.600	22.931
Distance to coast (in km)	20927	7.109	6.192	0.192	22.340
Distance to Schleswig/Holstein (in km)	20927	134.191	55.211	66.230	343.437
Herfindahl index (10 km)	20967	0.148	0.053	0.100	0.250
Herfindahl index (20 km)	20967	0.037	0.006	0.026	0.050
Herfindahl index (30 km)	20967	0.019	0.002	0.012	0.025
Herfindahl index (40 km)	20967	0.012	0.001	0.009	0.018
Koppelwirthschaft adoption	20927	0.158	0.365	0.000	1.000
Koppelwirtschaft adoption, 1782	20927	0.123	0.328	0.000	1.000
Clover adoption, 1775	20927	0.201	0.357	0.000	0.854
Change of owner dummy	20927	0.039	0.194	0.000	1.000
New building finished	20927	0.008	0.087	0.000	1.000
Other improvements to the estate	20927	0.001	0.025	0.000	1.000

Sources: The individual level data were collected for the Danish price history and so were the barley and rye prices (Andersen and Pedersen, 2004); Most other variables are taken from Boberg-Fazlic et al. (2020) except the last three variables, which were coded by the authors using data from the website of the Danish Research Centre for Manorial Studies (https://herregaardsforskning.dk).

Table 2: Main results

	Dependent variable: log day wages					
Sample	Denmark	Funen and Jutland	Zealand	Denmark	Denmark, Men only	Denmark
	(1)	(2)	(3)	(4)	(5)	(6)
Farmhand x Serfdom	-0.351**	-0.505***	-0.297*	-0.261*	-0.397**	-0.347**
	[-2.322]	[-8.477]	[-1.976]	[-1.816]	[-2.610]	[-2.315]
Sample	Full	Funen and Jutland	Zealand	Full	Men only	Full
Fixed effects for:	Yes	Yes	Yes	Yes	Yes	Yes
Region	Yes	Yes	Yes	Yes	Yes	Yes
Occupation	Yes	Yes	Yes	Yes	Yes	Yes
Gender	Yes	Yes	Yes	Yes	Yes	Yes
Child	Yes	Yes	Yes	Yes	Yes	Yes
Master	Yes	Yes	Yes	Yes	Yes	Yes
Season	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Region x Year FE	No	No	No	Yes	No	No
Add in-kind payment dummy	No	No	No	No	No	Yes
Observations	20,927	8,670	12,257	20,927	19,004	20,927
R-squared	0.649	0.779	0.603	0.657	0.553	0.651

Notes: This table shows the effect of serfdom on farmhand wages (measured as the natural logarithm of the daily wage); coefficients are reported with the robust t-statistics in parentheses (*** p<0.01, ** p<0.05, * p<0.1); the standard errors are clustered at the estate level.

Table 3: Results with different control groups

	Dependent variable: log day wages							
Control group	Non-skilled & unskilled	Medium and high skilled	Non-skilled & unskilled, men only	Medium and high skilled, men only	Teachers excluded	Carpenter excluded	Farm servants excluded	Bricklayer excluded
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Farmhand x Serfdom	-0.337**	-0.318*	-0.414***	-0.306*	-0.300**	-0.345**	-0.316**	-0.361**
	[-2.661]	[-1.792]	[-3.516]	[-1.803]	[-2.527]	[-2.153]	[-2.258]	[-2.197]
Observations	14,109	12,816	12,591	12,160	20,492	19,738	20,472	18,556
R-squared	0.615	0.526	0.461	0.496	0.657	0.649	0.641	0.630

Notes: This table shows the effect of serfdom on farmhand wages (measured as the natural logarithm of the daily wage); The control variables are the same as in column (1) in Table 2 unless only men are included; these control variables are fixed effects for region, occupation, year and season as well as dummy variables for whether an individual is a master craftsman, a woman or a child; coefficients are reported with the robust t-statistics in parentheses (*** p<0.01, ** p<0.05, * p<0.1); the standard errors are clustered at the estate level.

Table 4: Results with estate level controls

	Dependent variable: log day wages							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Farmhand x Serfdom	-0.278**	-0.277**	-0.241*	-0.267**	-0.220**	-0.284*	-0.138**	-0.339**
	[-2.180]	[-2.190]	[-1.906]	[-2.266]	[-2.575]	[-1.966]	[-2.404]	[-2.304]
Price of Barley (log)		0.0207						
		[0.386]						
Price of Rye (log)			-0.105*					
			[-1.831]					
Distance to market town x Serfdom				-0.0656**			-0.0745***	
				[-2.179]			[-4.855]	
Distance to coast x Serfdom					-0.0552***		-0.00976	
					[-3.111]		[-0.962]	
Distance to Schleswig x Serfdom						0.00228	0.00312***	
						[1.718]	[3.552]	
Season x Serfdom								-0.107
								[-1.566]
Observations	18,459	18,459	17,552	20,927	20,927	20,927	20,927	20,927
R-squared	0.654	0.654	0.666	0.650	0.650	0.650	0.651	0.649

Notes: This table shows the effect of serfdom on farmhand wages (measured as the natural logarithm of the daily wage); All estimations include the same control variables as in column (1), Table 2; these control variables are fixed effects for region, occupation, year and season as well as dummy variables for whether an individual is a master craftsman, a woman or a child; All regressions with distance interactions also include the distance itself; coefficients are reported with the robust t-statistics in parentheses (*** p<0.01, ** p<0.05, * p<0.1); the standard errors are clustered at the estate level.

Table 5: Results with control for Herfindahl indices at different distances

	Dependent variable: log day wages				
	(1)	(2)	(3)	(4)	(5)
Farmhand x Serfdom	-0.351**	-0.207*	-0.303	-0.728***	-0.878**
	[-2.322]	[-2.038]	[-1.061]	[-3.388]	[-2.517]
Herfindahl (10 km)		-9.470***			
		[-5.986]			
Herfindahl (10 km) x Serfdom		10.53***			
		[6.408]			
Herfindahl (10 km) x Farmhand x Serfdom		0.557			
		[0.960]			
Herfindahl (20 km)			-54.19***		
			[-7.187]		
Herfindahl (20 km) x Serfdom			44.51***		
			[4.416]		
Herfindahl (20 km) x Farmhand x Serfdom			6.164		
			[0.714]		
Herfindahl (30 km)				-96.87***	
				[-12.81]	
Herfindahl (30 km) x Serfdom				67.84***	
				[4.856]	
Herfindahl (30 km) x Farmhand x Serfdom				34.95**	
				[2.788]	
Herfindahl (40 km)					-134.7***
					[-7.552]
Herfindahl (40 km) x Serfdom					89.31***
					[3.450]
Herfindahl (40 km) x Farmhand x Serfdom					61.85**
					[2.172]
Observations	20,927	20,927	20,927	20,927	20,927
R-squared	0.649	0.654	0.652	0.654	0.652

Notes: This table shows the effect of serfdom on farmhand wages (measured as the natural logarithm of the daily wage); All estimations include the same control variables as in column (1), Table 2; these control variables are fixed effects for region, occupation, year and season as well as dummy variables for whether an individual is a master craftsman, a woman or a child coefficients are reported with the robust t-statistics in parentheses (*** p<0.01, ** p<0.05, * p<0.1); the standard errors are clustered at the estate level.

Table 6: Results with estate level innovation controls

	Dependent variable: log day wages				
	(1)	(2)	(3)	(4)	(5)
Farmhand x Serfdom	-0.366**	-0.374**	-0.302*	-0.350**	-0.320*
	[-2.441]	[-2.501]	[-1.849]	[-2.325]	[-1.947]
Koppelwirtshcaft, 1782	-0.035				
	[-0.579]				
Farmhand x Koppelwirtschaft 1782	0.162*				
	[2.066]				
clover_ adoption_1775			-0.107		-0.0913
			[-1.494]		[-1.320]
Farmhand x clover adoption			-0.103		-0.126
			[-0.559]		[-0.781]
Koppelwirtschaft adoption		-0.0387			-0.0168
		[-0.603]			[-0.336]
Farmhand x Koppelwirtshcaft adoption		0.185**			0.190*
		[2.208]			[1.853]
Change of owner dummy				-0.168***	-0.158***
				[-5.107]	[-8.834]
New building finished				-0.00713	-0.023
				[-0.105]	[-0.302]
Other improvements to the estate				0.156	0.154
				[1.386]	[1.371]
Observations	20,927	20,927	20,927	20,927	20,927
R-squared	0.649	0.650	0.650	0.650	0.653

Notes: This table shows the effect of serfdom on farmhand wages (measured as the natural logarithm of the daily wage); All estimations include the same control variables as in column (1), Table 1; these control variables are fixed effects for region, occupation, year and season as well as dummy variables for whether an individual is a master craftsman, a woman or a child; coefficients are reported with the robust t-statistics in parentheses (*** p<0.01, ** p<0.05, * p<0.1); the standard errors are clustered at the estate level.

Table 7: Results with estate fixed effects

Dependent variable: log day wages									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Farmhand x Period 1	-0.158**								
	[-2.701]								
Farmhand x Period 2	-0.0139								
	[-0.172]								
Farmhand x Period 3	-0.0599								
	[-0.861]								
Farmhand x Period 4	-0.0226								
	[-0.207]								
Farmhand x Serfdom		-0.0445							
		[-0.717]							
Farm labourer x Serfdom			-0.265*	-0.361**					
			[-1.755]	[-2.137]					
Labourer x Serfdom			0.0278		0.0411				
			[0.498]		[0.816]				
Day labourer x Serfdom			-0.306*			-0.314*			
			[-1.904]			[-1.841]			
Farm labourer x Period 1							-0.704***		
							[-4.774]		
Farm labourer x Period 2							-0.429*		
							[-1.951]		
Farm labourer x Period 3							-0.369*		
							[-2.059]		
Farm labourer x Period 4							-0.271		
							[-1.730]		
Labourer x Period 1								-0.0728	
								[-1.302]	
Labourer x Period 2								0.146**	
								[2.455]	

Labourer x Period 3								0.0226	
								[0.329]	
Labourer x Period 4								-0.136	
								[-0.405]	
Day labourer x Period 1									-0.364**
									[-2.265]
Day labourer x Period 2									-0.455**
									[-2.803]
Day labourer x Period 3									-0.312
									[-1.687]
Day labourer x Period 4									-0.227
									[-1.380]
Observations	20,927	20,927	20,927	15,797	17,638	17,350	15,797	17,638	17,350
R-squared	0.670	0.670	0.670	0.743	0.688	0.745	0.743	0.689	0.746

Notes: This table shows the effect of serfdom on farmhand wages (measured as the natural logarithm of the daily wage) controlling for estate fixed effects; All estimations include the same control variables as in column (1), Table 1 except that region fixed effects are replaced by estate fixed effects; these control variables are fixed effects for estate, occupation, year and season as well as dummy variables for whether an individual is a master craftsman, a woman or a child coefficients are reported with the robust t-statistics in parentheses (*** p<0.01, ** p<0.05, * p<0.1); the standard errors are clustered at the estate level.

Table 8: The effect of the Introduction of Serfdom on Wages using the Scanian control group

	Dependent variable: log wages								
Sample	Full	Full	Full	Funen, Jutland, Scania	Funen, Jutland, Scania	Funen, Jutland, Scania	Zealand, Scania	Zealand, Scania	Zealand, Scania
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Farmhand x Serfdom	-1.016***	-0.590***	-0.496***	-0.814***	-0.405*	-0.527**	-1.025***	-0.579***	-0.482***
	[-10.100]	[-5.818]	[-5.630]	[-3.936]	[-2.096]	[-2.597]	[-9.100]	[-5.256]	[-4.657]
Exchange rate Correction	No	Yes	No	No	Yes	No	No	Yes	No
Real wage	No	No	Yes	No	No	Yes	No	No	Yes
Observations	10,735	10,735	10,735	7,115	7,115	7,115	8,357	8,357	8,357
R-squared	0.962	0.700	0.594	0.975	0.788	0.654	0.957	0.664	0.583

Notes: This table shows the effect of serfdom on farmhand wages (measured as the natural logarithm of the daily wage) using data for Danish farmhands and Scanian unskilled workers; the variable "serfdom" represents a dummy variable which takes the value of 1 in the period in which serfdom affected workers (1733-1799); Farmhand is represented by labourers, day labourers and farm labourers; All models include fixed effects for years, gender and region; coefficients are reported with the robust t-statistics in parentheses (*** p<0.01, ** p<0.05, * p<0.1); the standard errors are clustered at the location level.

Table 9: The effect of serfdom on probability that new apprentices were recruited from the countryside

Dependent variable: pr(apprentice from country =1)		
Variable	(1)	(2)
period 1		-0.188***
		[-2.925]
period 2		-0.310***
		[9.515]
period 3		-0.252***
		[-6.864]
period 4		-0.241**
		[-2.272]
Serfdom	-0.268***	
	[-9.323]	
Observations	516	516
R-squared	0.104	0.110

Notes: This table shows the correlation between the serfdom periods and the probability of an apprentice being recruited from the countryside.