

# The introduction of serfdom and labor markets<sup>†</sup>

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**Abstract:** This research provides evidence on how restrictions on labor mobility, such as serfdom and other types of labor coercion, impact labor market outcomes. To do so, we estimate the impact of a large shock to labor mobility in the form of the reintroduction of serfdom in Denmark in 1733, which was targeted at limiting the mobility of farmhands. While many economists, historians and others have argued that serfdom had an impact on the mobility and wages, revisionist historians have countered that workers found ways to circumvent the restrictions imposed by serfdom. Using a unique data source based on 18<sup>th</sup> century estates, we test whether serfdom affected the wages of farmhands more strongly than other groups in the labor market using a differences-in-differences approach, and find evidence consistent with a strong negative effect on serfdom following its introduction. 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. Thus, our results suggest that serfdom was effectively reducing mobility.

**Keywords:** Serfdom, labor mobility, coercion

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## 1. Introduction

How do efforts to reduce the mobility of workers affect labor market outcomes? The present paper provides new evidence on this question by turning to the transformation from feudal labor markets to modern labor markets in which people can choose where to work and live (Acemoglu, Johnson and Robinson 2005, p.440). According to many authors, feudal institutions such as serfdom undermined incentives and led to underdevelopment because they restricted labor mobility and removed the allocative role of the labor market (Acemoglu et al., p. 441; Ogilvie and Carus 2014, p.467). Many historical accounts are largely in line with this view,<sup>1</sup> but as pointed out by e.g. Dennison (2006, p.74) “a revisionist view has emerged, which portrays serfdom as having had little or no effect on peasants’ social and economic behavior.” Ogilvie (2005, p. 93) similarly points out that “Traditionally, serfdom is portrayed as involving strong manorial controls on serfs’ mobility. But proponents of ‘communal autonomy’ contest this view, adducing evidence that serfs some-times migrated without apparent manorial hindrance.” Thus, revisionist historians have suggested that farmhands found ways of getting around the mobility restrictions associated with serfdom for which reason the effects could very well be negligible.<sup>2</sup>

In this paper, we provide new quantitative evidence on the effects of reducing mobility in the labor market by exploiting the reintroduction of serfdom in Denmark in 1733 which were targeted at tying male farmhands to the estate in the area in which they were born (e.g. Olsen, 1933; Skrubbeltrang, 1961). Agricultural workers were largely unskilled, and as they became bound to a given estate this was likely to decrease their bargaining power inherent in a right to move and seek work elsewhere, and thus their wages (Persson, 2010:p.81). This means that their wages are likely to decrease compared to other groups in the labor market as e.g. the mobility of craftsmen would be much less

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<sup>1</sup> Recent examples include Ogilvie (2007) and Ogilvie and Carus (2014).

<sup>2</sup> 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). Below we also add examples of similar views from the Danish context.

affected by serfdom.<sup>3</sup> Serfdom could also serve to prevent young men from the countryside from moving out of the rural sector via an apprenticeship in a town.

We exploit a unique micro-level dataset, which contains information on the wages, occupation and geographical location as well as other characteristics of individuals selling their labor to an estate. This allows us to evaluate whether there was a differential impact on the farmhands as compared to other groups in the labor market in a differences-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 effects and occupations in all our estimations. Given the micro-level nature of the data, we can also control for fixed effects for region, gender, season and other characteristics of the individuals we observe. We also control for the presence of regional trends by controlling for year fixed effects which vary by geographical area in more demanding specifications. Further, we can control for e.g. agricultural prices, which according to the historical narrative may have driven the re-introduction of serfdom. Still, we continue to find a negative impact on the wages of farmhands in the differences-in-differences setup. Importantly, we can also provide evidence that there was no discernable differential trend between farmhand wages and those of other groups in the labor market prior to the reintroduction of serfdom. We complement this analysis with suggestive evidence on reduced opportunities for apprenticeship for young men from rural areas after serfdom was introduced using micro data for apprentices.

Apart from the unique data source and the possibility to implement differences-in-differences estimation, studying the case of serfdom in Denmark also has several other advantages. First, while there are many historiographical analyses of serfdom (e.g. Domar, 1970; North and Thomas, 1971; Brenner, 1976), there is relatively little quantitative evidence on the effects of serfdom on labor market outcomes for Western Europe. The reason is that serfdom ended in the early sixteenth century in most of Western Europe which means that data are largely unavailable. Some evidence exists for Eastern Europe (as discussed below) where the emancipation of the peasantry came much

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<sup>3</sup> Bobonis and Morrow (2014) show that when unskilled labor is coerced to work for e.g. landowners, then the relative wage of skilled workers increases. We return to potential mechanisms in Section 6.

later, see e.g. Persson and Sharp (2015, p.90-97). Denmark is an exception to the common Western European pattern as the data described above pertain to the period in which serfdom was re-introduced. That Denmark was an exception is also pointed out 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 study the impact of serfdom on a western European country. Second, the rules regarding serfdom 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 which he belonged to 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. While the Danish case study comes with these advantages, it should be kept in mind that the 1733 serfdom in the Danish context was about putting restrictions on the mobility of male peasants to secure the necessary labor for the manors. By contrast, In the Russian context, peasants were practically the property of the gentry and markets for serfs existed (Markevich and Zhuraskaya, forthcoming; Domar and Machina, 1983). Drawing largely on the experience from Russia and Eastern European countries, Ogilvie and Carus (2016, p.474) 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 this description shows, our study exploits variation in one of the dimensions of an economy with serfdom, namely the mobility of farmhands.<sup>4</sup> While this means that the Danish case is somewhat different from the Russian and Eastern European cases, it allows us to consider the mobility dimension of serfdom.

Our paper contributes to the quantitative literature on the effects of serfdom, as well as to the broader literature on institutions and policies that restrict labor mobility. Quantitative studies of

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<sup>4</sup> As pointed out in the Oxford Encyclopedia of Economic History vol. 4 (2003, p. 463): “The precise form taken by serfdom cannot be specified in advance but must be established empirically for each particular time and place.” Yet, it also pointed out in the encyclopedia that the right of movement is typically part of the whole serfdom package.

serfdom include Domar and Machina (1983), Nafziger (2012), Markevich and Zhuraskaya (Forthcoming) and Klein and Ogilvie (2016).<sup>5</sup> Domar and Machina (1983) study the correlates of the prices of serfs in the context of Russia; Nafziger (2012) studies the impact of abolishing serfdom in Russia for the non-farm activity of serfs as compared to non-serfs. Markevich and Zhuraskaya (Forthcoming) study the effects on agricultural productivity, industrial output and peasant's nutrition of the abolition of serfdom in Russia. Klein and Ogilvie (2013) study the non-farm activity of peasants using cross-sectional data from Bohemia in the present day Czech Republic. None of these studies focus on wage effects due to mobility restrictions, none of them evaluate the impact of the introduction of serfdom, and finally all of them focus on Eastern European serfdom.<sup>6</sup> Below, we give a more detailed discussion of the extant empirical literature.

Our paper is also strongly related to evidence on the effects of other historical institutions that limited labor market mobility. Naidu (2010) presents an analysis of anti-enticement fines and demonstrates that these reduced the mobility of share croppers using data for Arkansas. He also provides suggestive evidence that these laws reduced state level agricultural wages in the American south.<sup>7</sup> Naidu and Yuchtman (2012) study the impact of the master and servant law, which made breach of contract a criminal offence in Great Britain. They study how the number of prosecutions responds to demand shocks as well how county level wages responded to the abolition of the law with the effect being larger in areas with more prosecutions. Two features of our study set it apart from these studies. First, we focus on serfdom which is the prime example of an institution reducing labor mobility in historical Europe. Second, we can use individual level data in our differences-in-differences estimation as well as individual level data on whether apprentices were recruited from the countryside.

Our paper further speaks to the literature on restrictions on labor mobility in developing countries today as pointed out by many authors. One example is Genicot (2002, p. 102), who notes that: "The

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<sup>5</sup> See also Buggle and Nafziger (2016) who explore the link between historical serfdom and present day well-being.

<sup>6</sup> Ogilvie and Edwards (2000) analyze data for Bohemian villages, but do not consider the effects on wages.

<sup>7</sup> Another important example of a coercive labor market institutions is slavery as known in e.g. the American South. Slavery and serfdom share some similarities, but as pointed out in the Oxford Encyclopedia of Economic History vol. 4 (2003), Serfs have legal rights and can often provide for their own subsistence, which is not true of slaves (p. 463).

incidence of bonded labor and serfdom has been amply documented throughout history and in all parts of the world. [...] Perhaps less well-known is the extent to which these institutions persist in more recent times.” Acemoglu and Wolitzky (2011) concur with this observation and argue that labor transactions throughout most of history and a significant fraction of such transactions in developing countries today are coercive.<sup>8</sup> Yet another example is the Chinese Hukou system, which serves to restrict rural-urban migration, see Whalley and Zhang (2007) who provide model simulations of the impacts of this system. Our empirical analysis provides evidence from plausibly exogenous variation to cast light on the quantitative impact of mobility restrictions, which is arguably difficult in a modern context.

We finally speak to the historiographical literature on serfdom. This is relevant for the eastern European and Russian contexts. It is clearly also relevant for the Danish historical context. Mirroring the international literature, the traditional view has been that serfdom was effective in restricting mobility and wage growth for farmhands. The Danish economic historian Hansen (1984, p. 43), for example, suggested that serfdom was effective in securing the estates a cheap, dependent labor force. In a similar vein, Andersen et al. (2004, p.46) argue that ‘access to unfree labor supplied by the adscripted men on the estate must have guaranteed an upper ceiling for the wages of others.’<sup>9</sup> By contrast, Holmgaard (1990, p. 269) notes that historians have increasingly recognized that the mobility restrictions associated with serfdom were less important than believed previously. An example of this is Løgstrup (1987a, 1987b), who takes a revisionist position, and emphasizes the existence of geographical mobility during the period of serfdom. Kjærgaard (1991) argues that while serfdom might have had some local effects on wages, its impact was marginal compared to other factors. Yet, none of these authors offer econometric analyses for these assertions and, with unique data at hand, this paper will begin to fill this gap in our understanding of serfdom.

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<sup>8</sup> Coerced labor 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).

<sup>9</sup> The traditional view can be traced back at least to Falbe-Hansen (1888). The Danish literature often uses adscription rather than serfdom to refer to the fact that people living in the countryside were bound to the land.

The rest of the paper is organized as follows. Section 3 gives a brief history of serfdom in Denmark. Section 4 describes the empirical strategy. Section 5 describes the data. Section 6 presents the analysis. Section 7 offers interpretation and discusses mechanisms, whereas Section 8 concludes.

## **2. Existing empirical literature on serfdom**

In Table A1 in the Online Appendix, we provide an overview of the existing empirical studies in the literature. Two studies rely on cross-sectional variation only. Domar and Machina (1984) provide tests of whether Russian serfdom was unprofitable by investigation whether serf prices are similar across regions and are different from zero. 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 this way, both study do not speak to the effects of re-introducing serfdom and the effects on labor mobility and wages. Nafziger (2012) and Buggle and Nafziger (2017) 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 (2017) find evidence on long run persistent effects using Russian data. Our study speaks to the immediate effects of serfdom on labor mobility and not the long run effects, and our work is different in this way. Finally, Markevich and Zhuraskaya (Forthcoming) use a differences-in-differences strategy, which exploits the variation produced by abolishing serfdom and the intensity of serfdom across Russian regions. Our study also uses a differences-in-differences strategy, but exploits differential effects across occupations and the re-introduction of serfdom. Also, unlike the other studies, we use micro level data rather than more aggregate province or regional level data.

## **3. Historical background**

This section briefly details the history of serfdom in Denmark to inform our investigation of its labor 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 in 1733. Finally, we discuss the changes made to serfdom during the 18<sup>th</sup> century.

### *Early serfdom – vornedskab*

At the end of the 15<sup>th</sup> century, serfdom known as vornedskab was established on the eastern islands of Denmark. As is also true for the re-introduced serfdom it was directed at male farmhands. Christensen, Milthers and Hansen (1934, p.40) note that the sons of farmhands were tied to the same estates as their fathers. Thus, they were not at liberty to move. Farmhands would not be the property of a landlord, but they were tied to a particular estate. If the farmhand could pay a fee, he could be allowed to work elsewhere. While the farmhands were not slaves, Christensen et al. (1934) does mention that the buying and selling of farmhands by landlords did take place. While there were earlier attempts at abolishing the “vornedskab”, it was not until 1702 that it was finally abolished,<sup>10</sup> and then only for children born after 25<sup>th</sup> August 1699. This leads Munch (1974, p.308) to conclude that it would not start to have any effect until 1717, when the first free workers would start to enter the labor market. In a similar vein, Løgstrup (2015) concludes that vornedskabet would be gradually abolished while those subject to it died.

### *Reintroduction of serfdom*

As mentioned in the introduction, serfdom was reintroduced in 1733 for all males in the age group 14 to 36 years old, and this time for the whole of the country. More precisely, serfdom was reintroduced on February 4<sup>th</sup>, 1733, when a royal decree was released, which stated that any peasant of age 36 or younger was not allowed to leave the estate at which he was born if the landlord had work for him. From 1735, it was made clear that the intended group was from 14 to 36 years of age, though the original decree might have signified that originally by using the word “bondekarl” (which translates to peasant), which indicates someone, who is clearly not a young child (Holmgaard, 2003: p. 28).<sup>11</sup> The reintroduction was mainly aimed at ensuring farmhands for the estates, but in principle every male in

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<sup>10</sup> This was done by a royal decree of 21<sup>st</sup> February 1702, see Holmgaard (1999, p.130), who also provides a discussion of the process that led to abolition.

<sup>11</sup> In 1735, it was made clear that those who had moved with 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).



the age group 14 to 36 years was now tied to the estate. 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 on the eastern islands. Yet, in the western part of the country, the 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 labor as compared to land (Holmgaard, 2003). This is in line with earlier work by Domar (1970) who proposed a theory of serfdom introduction based on scarcity of labor as compared to land. 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 landlords regarding labor scarcity and have interpreted attempts at repressing wages as discussed above. Still, what we do in the present paper is to test for the impact of serfdom. Yet, if the agricultural crisis or the end of *vornedskab* were creating conditions of labor scarcity, this could have been important for which reason we investigate whether there were early impacts on the wages of farmhands prior to serfdom. Holmgaard (1990) suggests that the introduction of serfdom is related to the (temporary) abolition of the land militia in 1730. As young peasants were no longer required to serve in the land militia, some seem to have decided to be drafted into the army rather than work at an estate (Holmgaard, p. 248-250). The land militia was also believed to be a reason that young peasants fled the country, so in principle the move could have increased the number of available farmhands. Nonetheless, a process in which it was argued that the freedom of the peasants had to be constrained was set in motion. This process culminated with the introduction of serfdom in 1733 and the re-establishment of the land militia. During the process, 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, p. 259). By this account, the introduction of serfdom was more driven by a desire by the estate owners to reduce the mobility of its labor force than of actual labor scarcity. Figure 1 gives suggestive evidence on whether increased wages after 1730 among farmhands, due to the abolition of the land militia, drove adoption of serfdom. As seen in the figure, farmhands experienced average wage increases prior to serfdom, but so did other groups. After serfdom was introduced in 1733 and towards the end of the 1740s, the

farmhands experienced substantial decreases in wages compared to other groups. While the raw data fails to control for composition effects of the groups, the visual impression is that average wages among farmhands did fall.

**Figure 1 about here**

The reintroduction of serfdom has also been associated with falling agricultural prices following the Great Northern War from 1709-1720 (Christensen et al., p. 71) which turned into an agricultural crisis leading to farmhands leave the estates.<sup>12</sup> When we plot our data for the (log) barley price along with (log) farmhand wages in Figure 2, we find that in the aggregate that grain prices were falling in the years prior to the reintroduction of serfdom. Yet, we note that the barley price was starting to increase from 1732. In fact, barley prices were increasing in the 1730s during the same time that farmhand wages are decreasing. This suggests that it is unlikely that agricultural prices were driving the reintroduction of serfdom.<sup>13</sup> Yet, we will run some estimations in which we control for the barley price.

**Figure 2 about here**

As noted above, vornedskabet was gradually abolished on Zealand, and so by February 1733, all farmhands of age 33 years or below were not subject to it anymore on Zealand (Skrubbeltrang, 1978). Moreover, farmhands whose parents had migrated from Jutland and Funen had been free of vornedskab (Skrubbeltrang, 1978:p. 178). This means that it is likely that the effect of serfdom might plausibly smaller on Zealand as it would not affect all those aged between 33 and 36. To investigate this, we have plotted the average farmhand wages by two regions Zealand and Funen/Jutland in Figures A3 and A4. While we have fewer observations to compute the averages, the impression is

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<sup>12</sup> The Great Northern War was a war between Sweden and most other Northern European countries ([http://denstoredanske.dk/Danmarks\\_geografi\\_og\\_historie/Danmarks\\_historie/Danmark\\_1536-1849/Store\\_Nordiske\\_Krig](http://denstoredanske.dk/Danmarks_geografi_og_historie/Danmarks_historie/Danmark_1536-1849/Store_Nordiske_Krig)). It is either dated from 1700-1721 or 1709-1720. Denmark was allied with Prussia, Russia and Saxony. Major battles include a failed attempt by the Danish army to conquer 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 Karl XII's death, peace was made on February 1<sup>st</sup> in 1720. (Jespersen, pp. 306-315).

<sup>13</sup> In Figure 2A in the appendix, we show that a similar development is true for rye prices.

similar for both regions in the sense that wages fall after 1733. Yet, the fall seems more pronounced for Funen and Jutland. This result also holds up when we estimate in a differences-in-differences setup as explained below.

### *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., 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.<sup>14</sup>

Tightening the rules could have been an endogenous response to problems with e.g. runaways leaving the country. Yet, if we consider the 1742 changes, Holmgaard (2003, p.31) notes that a main reason given by the estate owners for changing the rules was that males younger than 14 years of age were leaving the estate to avoid serfdom. It was highlighted that they were leaving the country as a response to the initial introduction of serfdom in 1733. Holmgaard (2003) reviews the reports available for Danish counties and finds that this was happening to a very limited extent. There was some movement between estates, but as noted by Holmgaard (2003, p. 46), this would not represent uniform interests among estates. Again, this suggests that the tightening of 1742 was not linked to runaways out of the country which would represent a common problem for all estate owners.<sup>15</sup> Returning to figure 1, we do not see any visible effect of the 1742 tightening, but notice that this would have been a smaller shock than the initial one. When we plot the figure for the period 1705-1799 as in Figure 1A in the supplementary appendix, we also do not see visible effects of subsequent tightenings.<sup>16</sup>

## **4. Empirical strategy**

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<sup>14</sup> The agrarian reforms also included e.g. the enclosure movement, see e.g. Christensen (1925) and Løgstrup (2015)..

<sup>15</sup> 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.

<sup>16</sup> We also note that there is no visible upward trend in wages of either groups in Table 1A of the supplementary appendix.

We carry out a difference-in-difference estimation on a dataset, which covers the period 1705-1799. We pursue two strategies. One in which we treat the reintroduction as from running from 1733 to 1799, and another in which we allow the effects to be time-varying according to the periods described above.

We proceed by estimating the following equation for the period 1705-1799 for the log of wages as denoted by  $lnw_{it}$ :

$$lnw_{it} = farmhand_{it}serfdom_{1733}\beta + \alpha_t + X_{it}'\gamma + \varepsilon_{it},$$

$i$  indicates individual and  $t$  indicates time.  $farmhand_{it}$  indicates whether the individual observed is a farmhand, and finally  $serfdom_{1733}$  is a dummy which is equal to 1 from 1733-1799. The parameter of interest is  $\beta$ , which measures the impact of serfdom on the farmhands.  $\alpha_t$  indicates year fixed effects.<sup>17</sup>  $X_{it}$  is a vector of control variables which include e.g. fixed effects for occupations (as described below), region fixed effects,<sup>18</sup> gender fixed effects, seasonal fixed effect and other control variables. As mentioned, 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. By testing for pre-existing trends, we can offer indirect evidence for this assumption.

The fact that serfdom was tightened and then loosened subsequently might influence the result and we therefore also more flexible estimate models in which take advantage of this fact and estimate:

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<sup>17</sup> Effects for years capture common shocks such as the cattle plague in the 1740s.

<sup>18</sup> We include region fixed effects as the literature argues that the group of manor owners must have comprised a kind of employer organisation at the regional level, and they may have had some agreements on wages (Andersen and Pedersen, 2004: p.46).

$$\ln w_{it} = \text{farmhand}_{it} \text{serfdom}_{1733} \beta_1 + \text{farmhand}_{it} \text{serfdom}_{1742} \beta_2 \\ + \text{farmhand}_{it} \text{serfdom}_{1764} \beta_3 + \text{farmhand}_{it} \text{serfdom}_{1788} \beta_4 + \alpha_t X_{it}' \gamma + \varepsilon_{it}$$

$\text{serfdom}_{1742}$  is a dummy equal to one from 1742-1763,  $\text{serfdom}_{1764}$  is a dummy equal to 1 from 1764-1787,  $\text{serfdom}_{1788}$  is a dummy equal to one for the period 1788-1799.

The more flexible model also allows for a check on the common pre-trend assumption which assumes that the trend of farmhands was parallel to those of other occupations. In some models, we therefore include a dummy for the period after the earlier serfdom ceased to have influence.

## 5. Data

For implementing the regression models, we need a measure of individual (log) 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 data were collected from accounts and material from the Danish government, the royal court and its property, the army, firms, churches, and from local and private archives. Although these data are referenced and briefly summarized in the two-volume *History of Prices and Wages in Denmark 1660-1800*, they have not otherwise been exploited. The period covered is 1660-1800, which overlaps the age of absolutism, and represents a unique dataset on labor and product markets during that time, unrivaled to our knowledge in detail by anything available for other countries. For our purposes, we need a measure for our left-hand side variable, namely wages. Radu (2015) details how the wage data are harmonized at the individual level, though we note that the wage series have been corrected for in-kind payments. We are not able to track individuals across time, but we have data for individuals working for a total of 16 estates in the full dataset covering 1705-1799.<sup>19</sup> This implies that 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

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<sup>19</sup> We stop our data in 1799, the last year in which serfdom was in place.

to the HISCO system (Historical International Standard of Classification of Occupations).<sup>20</sup> We code as farmhands those who are designated as “farm laborers”, “day laborers” and “laborers”<sup>21</sup> as our baseline, but also consider specifications in which we only use “farm laborers”, which arguably captures most closely those who worked the field. To provide an impression of the data, Table 1 summarizes data for the three regions of Jutland, Funen and Zealand. The table reveals that for many estates, we do not have observations in the period which contains the reintroduction of serfdom (1705-1741), and data are thickest for the period 1764-1787. The price history does not provide detailed descriptions of who these laborers were, but it indicates that the day laborers working in the field were usually cottagers renting a house, often with a small field. They had to work as farmhands with a contract for 6 or 12 months. The day laborers would receive money wages and if they worked on middle sized farms they would typically receive payment in kind (Andersen and Pedersen, pp. 19-20). The data may also include farmhands who were allowed to work on other estates (see Olsen, 1950) for wages temporarily. Yet, 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 is also likely to be affected.

Table 1 about here

Other than occupation 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 craftsmen), whether the individual is a child, and the location of the individual as given by region and estates.

Figure 3 about here

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<sup>20</sup> See <http://historyofwork.iisg.nl/>.

<sup>21</sup> Farm laborers perform a variety of tasks in growing crops and breeding and raising livestock according to the HISCO classification scheme. Laborers performs lifting, carrying, stacking, shoveling, digging, cleaning and similar tasks by hand, using simple laboring tools such as pick, shovel, wheelbarrow and street broom where necessary. Day laborers perform the same range of tasks as laborers using the same types of tools as a laborer (9-99.10), but is specifically hired and paid by the day.

## 6. Results

This section presents the results from the estimation of our equation of interest. All standard errors are clustered at the level of the estate or alternatively at occupational level. We begin by discussing the main results as reported in Table 2. Column 1 shows a negative and statistically significant estimate of  $\beta$ , which is consistent with the view that serfdom did affect farmhands more strongly compared to other groups in the labor market. In column 2, we add region by year effects for Funen and Jutland, which never experienced mobility restrictions as other parts of the countries. The estimate remains negative, but significance is reduced to the ten percent level. The size of the coefficient is nonetheless of a similar magnitude as the one reported in column 1. In Column 3, 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. In column 4, we add begin to investigate whether there are pre-existing trends in the form of the abolition of the early serfdom as discussed above. As this would start to matter from 1717, when the first farmhands who were free of early serfdom turned 18, we interact a dummy for 1717-1732 with the farmhand dummy and obtain a positive yet statistically insignificant estimate to this variable. As the mobility restrictions were targeted male farmhands, we check that the results are not driven by using women as the control group, and we find that the results are not driven by this, see column 5 in Table 2.

In addition to the flexible models in columns 3 and 4 in Table 2, we have also estimated a fully flexible model in which the farmhand dummy is interacted with year dummies. While this is a demanding specification, the results indicate that there are no negative and statistically significant coefficients prior to the 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 4. As we do not have many observations per year, this is perhaps not surprising.

**Table 2 about here**

**Figure 4 about here**

We next investigate the degree to which our results depend on using the full sample and the inclusion of certain occupations in the control group. These results are reported in Table 3. First, we re-estimated the baseline model on samples running from 1705-1741, 1705-1763 and 1705-1787. 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 percent 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 less than 800 observations. Once we increase the sample, we get much stronger results. Below, we report results suggesting that the effects on farm laborers were stronger than the other unskilled laborers we use in our measures. If we use the farm laborer category only in our measure, we find that the coefficient is larger than for our main result with a statistically coefficient of -0.37 for the whole sample. If we use the initial period only, then the coefficient is -0.51 and it is significant at the ten percent level. This suggests that the result for the initial period is driven by low power, as when we use those laborers 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 whether including certain occupations in the control group drive the results. A substantial amount of observations is for teachers who worked at the estates. As these may have been more mobile compared to 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 effect as the effect remains negative and significant, though numerically similar. Excluding carpenters or masons also have little effect, and the same is true for farm servants, see columns 6-8.

Table 3 about here

We next include a number of control variables in Table 4. First, we investigate whether prices of grains could be the underlying factor driving wages and serfdom. We do so by using the price of barley. In column (1), 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 (log) price of barley is positive, but not statistically significant. The coefficient on the interaction between farmhand and the serfdom dummy is reduced compared to the baseline, but it remains significant.<sup>22</sup> 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 environment. First, being close to the border might have implied that it was easier to run away as suggested by Holmgaard (2003). When we control for distance to Schleswig-Holstein interacted with the serfdom dummy in column (2), we find little evidence for this, as the coefficient is positive and insignificant. Being closer to a market town or the coast were associated with higher wages after serfdom, see columns (3) and (4). 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. In column (5), we control for all four variables, and find that our conclusions are largely unchanged, though the point estimate on the farmhand interaction is now smaller. We note that the 95 % confidence intervals between the estimates from the different columns overlap.

#### *Additional robustness checks*

We report additional robustness checks in Tables A2 and A3 in the online appendix. In columns (1) and (2) of Table A2, 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 (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, but the effect is still

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<sup>22</sup> The correlation coefficient between the (log) price of barley and the (log) 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. 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% level, see column 2. 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 though precision is reduced.

negative for the Zealand sample, though precision is smaller. In column (3) of Table A2, we show results when we let the sample begin in the 17<sup>th</sup> century and end in 1805. Again, the result for the serfdom period 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 in column (6). In column (7), we investigate whether differential effects on the three groups included in the serfdom measure are present. 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 types. When we do so, we find that the coefficient on the farm laborer interaction is -0.41 and significant at the five percent level. For day laborers it is -0.34, but only significant at the ten percent level. Finally, for laborers the coefficient is -0.257 and significant at the five percent level. These results are suggestive of some differences, though statistically speaking we cannot tell whether they are different, see column (7) of Table A2.

In Table A3, we investigate the extent to which observable or 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 labor markets were to some extent regional. 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-difference estimation. This is not true for estates, as we observe some only before and some only after serfdom. This means that some of the before and after variation will be captured by these fixed effects. With this in mind, we nonetheless run the estimations with estate level fixed effects. In column (1) of Table A3, we show the model in which we allow the effect to vary by periods. The coefficients are negative for all periods, but it is only significant for the first period. In column (2), we show the effect for all periods, and while the coefficient is negative, it is no longer significant. While this may suggest that the effects of serfdom died out, we show in columns (3)-(6) that there are reasons to believe that this is not the case. When we control for the price of barley in columns (3) and (4), we obtain significance at the ten percent level for the overall effect in column (4). The effect in the initial period is the strongest, and is significant at the 5 percent level in column (3). In column (5),

we exploit that farm laborers may have been exposed more heavily, and again we find the strongest effect initially, and the then effect becomes less significant for later periods. Again, we find that the overall effect is negative and significant at the ten percent level in column (6). Overall, our reading of the evidence is that estate level effects do not explain the results, but that controlling for them adds substantial noise. As an alternative to estate fixed effects, we control for observable characteristics of the estates in column (7) and (8). In particular, we control for distance to Schleswig-Holstein, distance to the coast, distance to a market town and productivity adjusted size of the estate. When we do so, we find that results are similar to our baseline. In sum, these results suggest that the estimated effect of serfdom is not driven by observed or unobserved estate level characteristics

## **Discussion**

All the results presented are consistent with the view that farmhands were negatively affected by the introduction of serfdom. In this section, we discuss interpretations as well mechanisms.

We noted that serfdom has partly been viewed as a response to an agricultural crisis with falling prices beginning after 1720. Yet, we note that wages of farmhands were not statistically significantly different from other groups in society in the period 1717-1732, which marks the time that abolishment of *vornedskab* would start working. We also note that using annual variation that there were no discernable pre-trends.

Our results necessarily allow for several mechanisms as to why the wages of farmhands are relatively low. One mechanism alluded to in the introduction 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 better options if they were to run away and migrate to another country (Olsen, 1933). Olsen (1933, p. 75) argues that the young craftsmen tied to an estate could easily find jobs abroad, and also argues that they were relatively numerous among those that ran away.

A 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 in craftsmen in both cities and the rural sector which would mean that the relative wages of farmhands would decrease. To get some suggestive evidence on this mechanism, we employ micro-level data for the city of Odense for which information of the birthplaces of apprentices has been coded. These data include information on what type of guild the apprentice joined (e.g. for shoemakers, tailors etc.) with data points for 1700-1790 yielding a total of 516 observations.<sup>23</sup>

While we cannot employ a differences-in-differences approach in this setting, it is possible to test whether the probability the apprentice is recruited from the countryside declines from 1733. We do this by estimating the following linear probability model:

$$pr(\text{apprentice from country side} = 1) = \alpha_{\text{guild}} + \text{serfdom}_{1733-1790}\gamma + \varepsilon_{it},$$

$\alpha_{\text{guild}}$  indicates a guild fixed effects and  $\gamma$  indicates the effect of serfdom on the probability of recruiting from the country. Now  $\gamma < 0$  would be consistent with this mechanism. When we estimate the model, we find that  $\tilde{\gamma} = -0.267$  (standard error clustered by guild = 0.028). Though, 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.

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

$$pr(\text{apprentice from country side} = 1) = \alpha_{\text{guild}} + \text{serfdom}_{1733}\gamma_1 + \text{serfdom}_{1742}\gamma_2 + \text{serfdom}_{1764}\gamma_3 + \text{serfdom}_{1788}\gamma_4 + \varepsilon_{it},$$

The estimated equation becomes:

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<sup>23</sup> We do not have data points for each year meaning that for some years we have at least one observations, whereas for others we have none.

$$pr(\text{apprentice from countryside} = 1) = \alpha_{\text{guild}} - \text{serfdom}_{1733}0.188 - \text{serfdom}_{1742}0.309 - \text{serfdom}_{1764}0.251 - \text{serfdom}_{1788}0.24 + \varepsilon_{it},$$

The coefficients are all negative and statistically significant. 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 – especially the tightening in 1742 seems to have mattered. Holmgaard (2003) argues that the background 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 effects in case 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% level, and so this suggests that the observed pattern is associated with serfdom and not simply general trends. The tightening in 1764 does not seem to have changed much and we find no detectable difference between the period 1742-1763 and the period 1764-1790. This may suggest that reducing the lower age from 9 to 4 would not matter much for the supply of apprentices.

In sum, these results suggest that possibilities 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 laborers was, in fact, affected by serfdom.

## 7. Conclusion

This paper offers new quantitative evidence on the impact of serfdom on labor mobility by considering the effects of the wages on farmhands. The evidence is consistent with the view that serfdom did matter for the mobility of workers and the labor market in general. While, it is plausible that runaways as well as other ways to leave an estate alleviated the effect of serfdom, the mobility

restrictions introduced by serfdom had negative effects on wages for farmhands as demonstrated by the robustness of our main result.

Thus, this evidence is in line with the view that institutions (such as serfdom) matter as suggested by economists and economic historians such as Acemoglu et al. (2005) and Ogilvie (2007). Our evidence can therefore be read as suggesting that restrictions on mobility do have negative impacts on the labor market and development overall.

Nonetheless, it should be recalled that serfdom may not only have had negative impacts. Olsen (1933), for example, links serfdom with the adoption of the labor-intensive field system of *Koppelwirtschaft*, which in the Danish context was associated with the establishment of modern dairying. Recent work by Jensen, Lampe, Sharp and Skovsgaard (2017) suggests that the dairies established in the 18<sup>th</sup> century were important for spreading knowledge on how to run dairies to ordinary peasants in the late 19<sup>th</sup> century. Since the cooperative dairies established in this period played a large role in the Danish economic take-off, it is possible that serfdom played some role. Research on other contexts (See Dennison, 2006) also highlight that serfdom may have had some positive effects, and we believe that investigating whether this was, in fact, the case is an important topic for future research.

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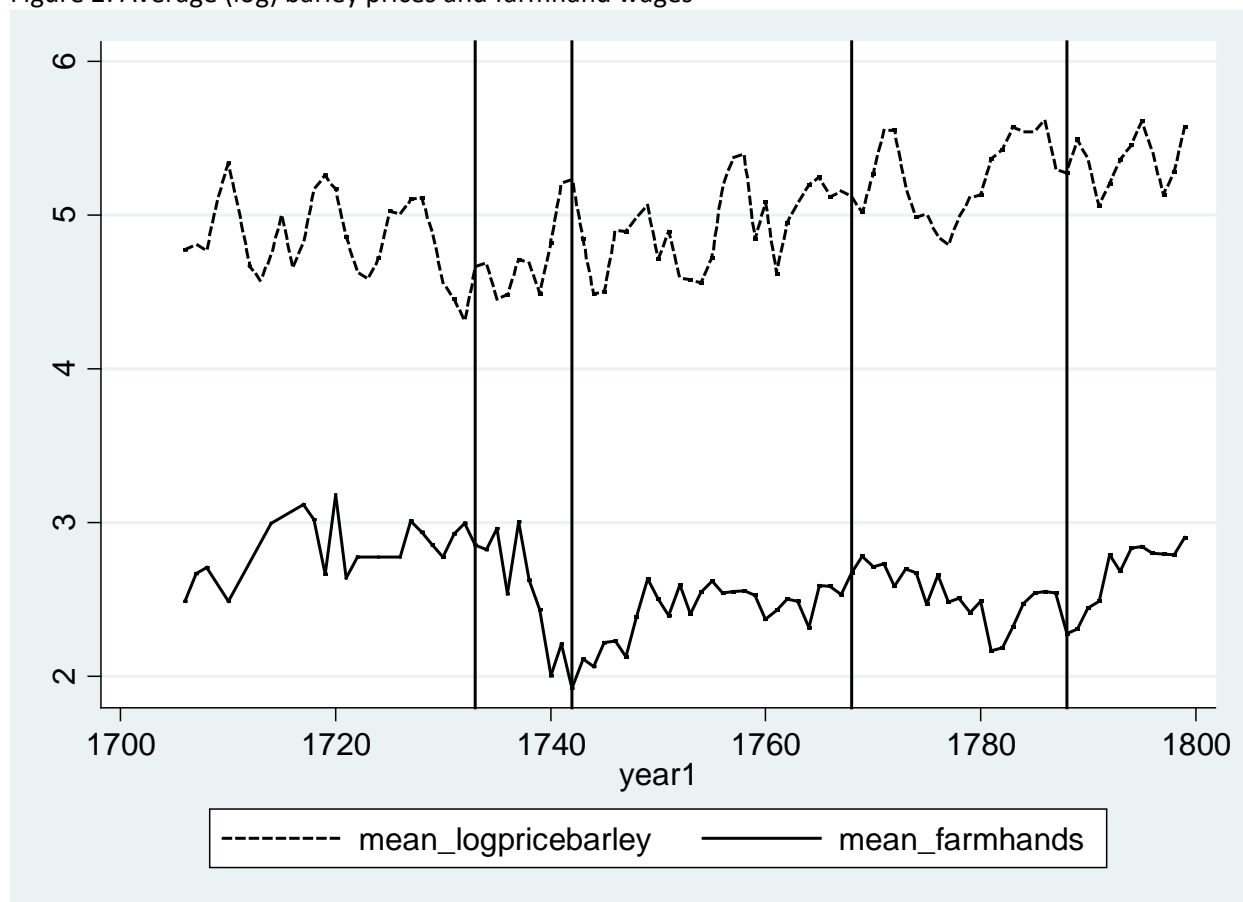
## Tables and Figures

Figure 1: Log wages for farmhands and other groups in the labor market, 1730-1760.

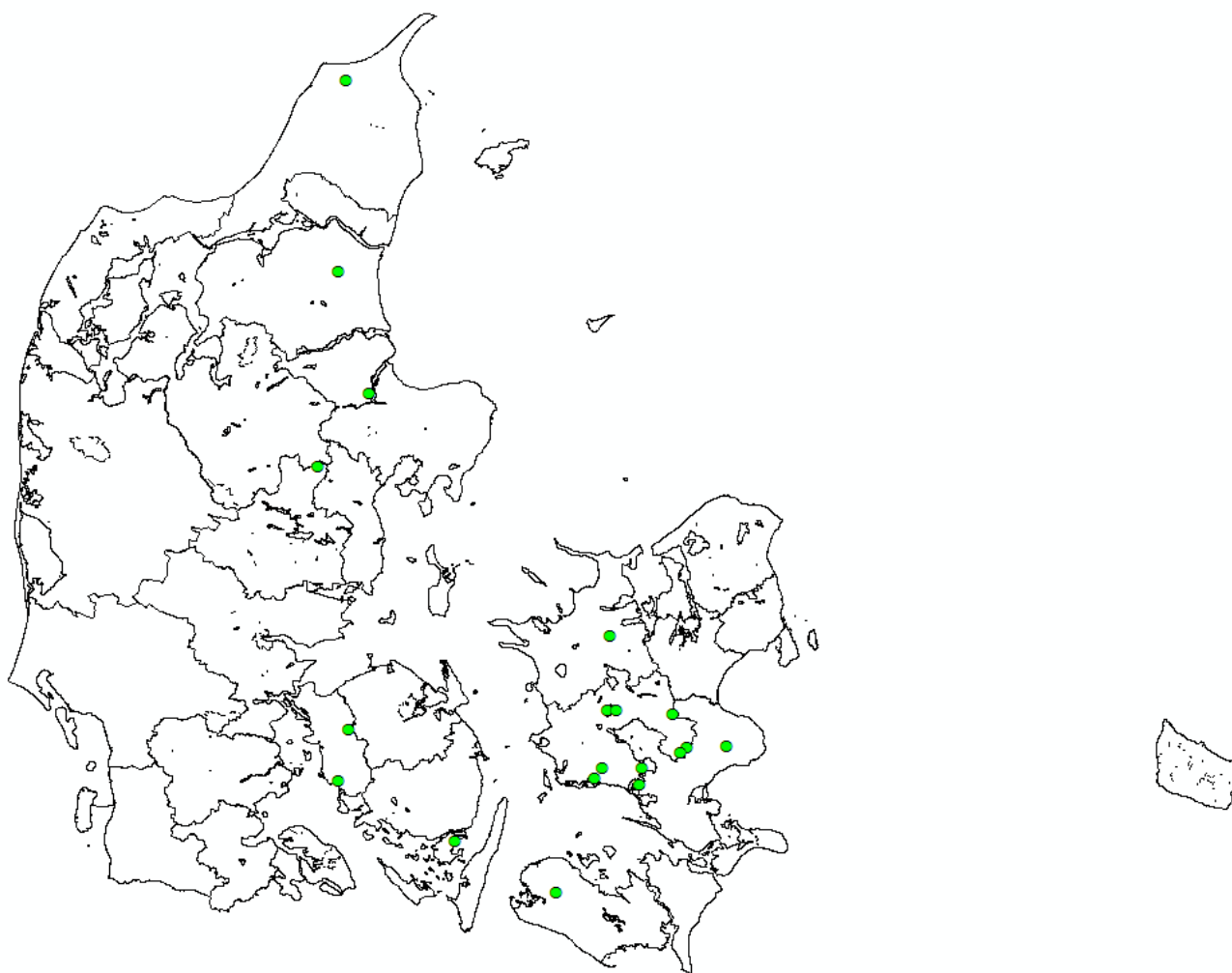


Notes: The variables on the second axis are log wages by farm hands and other occupations respectively. The vertical lines represent 1733 and 1742 respectively.

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



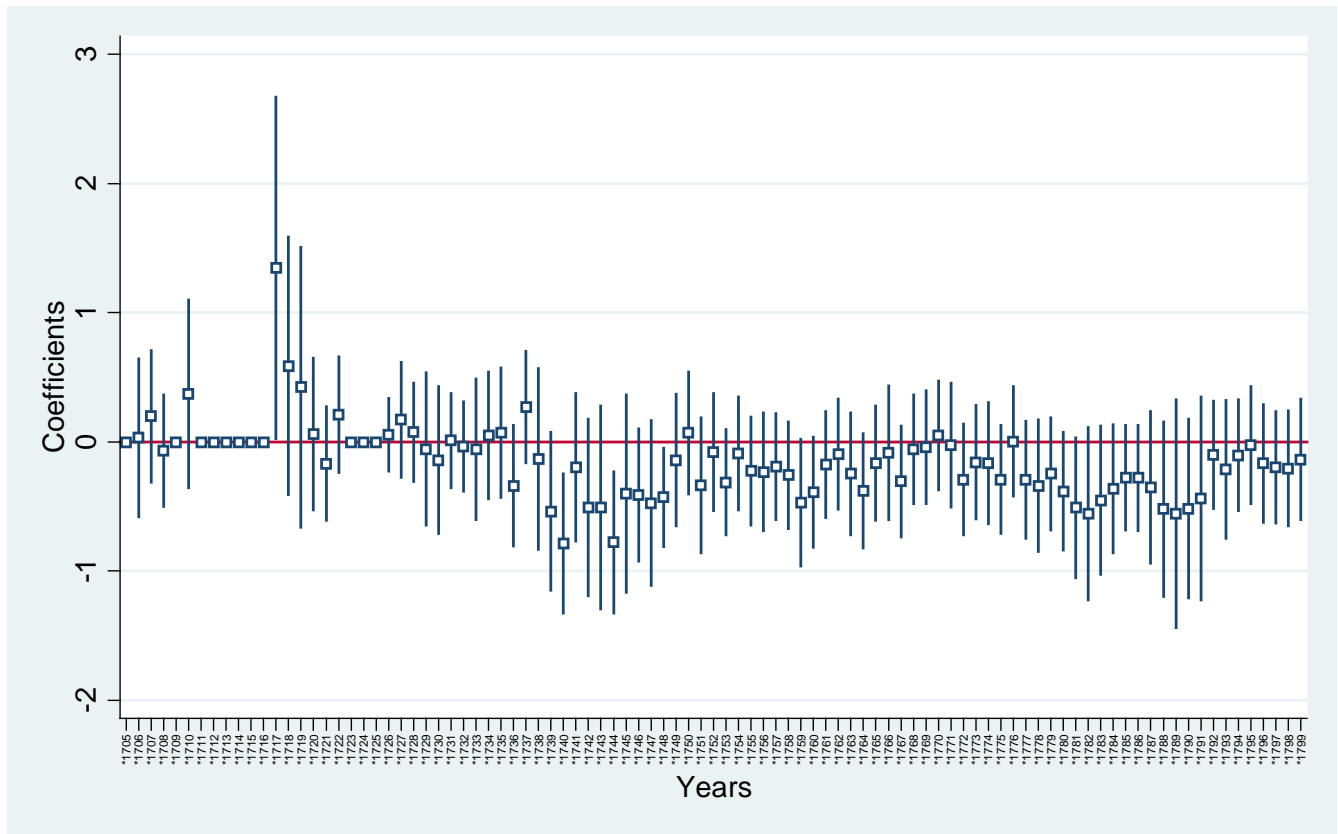
Notes: The variables on the second axis are log wage for farmhands and log price of barley . The vertical lines represent 1733, 1742, 1764 and 1788 respectively.



*Figure 3: Map of Denmark.*

*Note: Green circles indicate location of manor.*

Figure 4: Event study of introduction of serfdom, coefficient on farmhands multiplied by year



Notes: 1705 is the omitted year. Due to an absence of observations on farmhands, some years are set automatically to zero in the period 1710-1720.

Regions and estates	Number of observations (all occupations) Period					Farmhands (% of total occupations)	Women (% of total occupations)
	1661-1800	1705-1741	1742-1763	1764-1788	1789-1800		
<b>Zealand</b>	<b>12554</b>	<b>440</b>	<b>3514</b>	<b>6826</b>	<b>1600</b>		
Giesegaard (1721-1800)	436	12	109	302	13	34%	0%
Bregentved (1746-1800)	989	0	282	559	148	39%	1%
Gisselfeld Household (1706 1740)	317	317	0	0	0	30%	26%
Holsteinborg (1748-1800)	927	0	39	207	681	40%	5%
Fuirendal (1756-1795)	1340	0	324	774	241	25%	18%
Sorø Academy (1740-1800)	466	0	44	261	161	5%	0%
Løvenborg (1752-1794)	6929	0	2427	4403	153	28%	1%
Gauno (1751-1800)	787	0	265	319	203	21%	2%
Juellinge (1726-1748)	136	111	24	1	0	4%	0%
<b>Funen</b>	<b>5573</b>	<b>73</b>	<b>1061</b>	<b>2404</b>	<b>2035</b>		
Taasinge (1725-1800)	3020	52	801	1199	968	26%	11%
Frederiksgade 1773-1800	1932	0	0	996	936	76%	21%
Erholm Søndergade (1723-1800)	621	21	260	209	131	56%	8%
<b>Jutland</b>	<b>3347</b>	<b>218</b>	<b>286</b>	<b>1855</b>	<b>988</b>		
Frijsenborg (1777-1800)	1250	0	0	899	351	23%	9%
Støvringgard (1734-1800)	722	35	132	323	232	24%	16%
Lindenberg (1714-1799)	1309	117	154	633	405	51%	23%
Odden (1703-1732)	66	66	0	0	0	33%	0%



Table 2: The main results, 1705-1799

	Dependent variable				
	Log wage				
	(1)	(2)	(3)	(4)	(5)
Serfdom x Farmhand	-0.351** [-2.322]		-0.255* [-1.765]		-0.394** [-2.561]
Period 1 x Farmhand		-0.395* [-1.895]		-0.387** [-2.223]	
Period 2 x Farmhand		-0.338* [-2.093]		-0.330* [-2.013]	
Period 3 x Farmhand		-0.362** [-2.380]		-0.355** [-2.842]	
Period 4 x Farmhand		-0.336* [-1.914]		-0.329** [-2.690]	
Post Vornedskab x Farmhand				0.00931 [0.0477]	
Observations	20,898	20,898	20,898	20,898	19179
R-squared	0.644	0.649	0.644	0.644	0.548

Notes: The dependent variable is the natural logarithm of the daily wage. Columns (1), (3) and (5) show the results for the non-flexible model; the variable “serfdom” represents a dummy variable which takes the value of 1 in the period in which serfdom affected workers (1733-1799); unskilled is represented by laborers, day laborers and farm laborers; vornedskab is a dummy variable which takes the value of 1 during the working age of those affected by vornedskab (1717-1733); Columns (2) and (4) show the results for the flexible model: Period 1 is defined by the years 1733-1740, period 2 by 1741-1763, period 3 by 1764-1787 and period 4 by 1788-1799. All models include fixed effects for years, occupation, region, season, child, master (if craftsmen) and gender in columns (1)-(4). Jutland Funen year fixed effects are added in column 3; 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: Robustness to sample

	Dependent variable							
	Log wage							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Serfdom x farmhands	-0.124	-0.349**	-0.350**	-0.351**	-0.300**	-0.345**	-0.316**	-0.351**
	[-0.735]	[-2.187]	[-2.397]	[-2.322]	[-2.527]	[-2.153]	[-2.26]	[-2.33]
Occupations excluded	All	All	All	All	Teachers	Carpenter	Farm servants	Masons
Time period	1705-1741	1705-1763	1705-1787	Full	Full	Full	Full	Full
Observations	731	5,592	16,288	20,927	20,492	19,738	19,585	20,482
R-squared	0.76	0.679	0.655	0.644	0.652	0.652	0.64	0.641
Controls for								
Occupation	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Child	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Master	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is the natural logarithm of the daily wage. Serfdom represents a dummy variable which takes the value of 1 in the period in which serfdom affected workers (1733-1799); unskilled is represented by laborers, day laborers and farm laborers; the analysis is conducted for the period 1705-1799; Yes and No indicate if a control variable is included in the specification; 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.

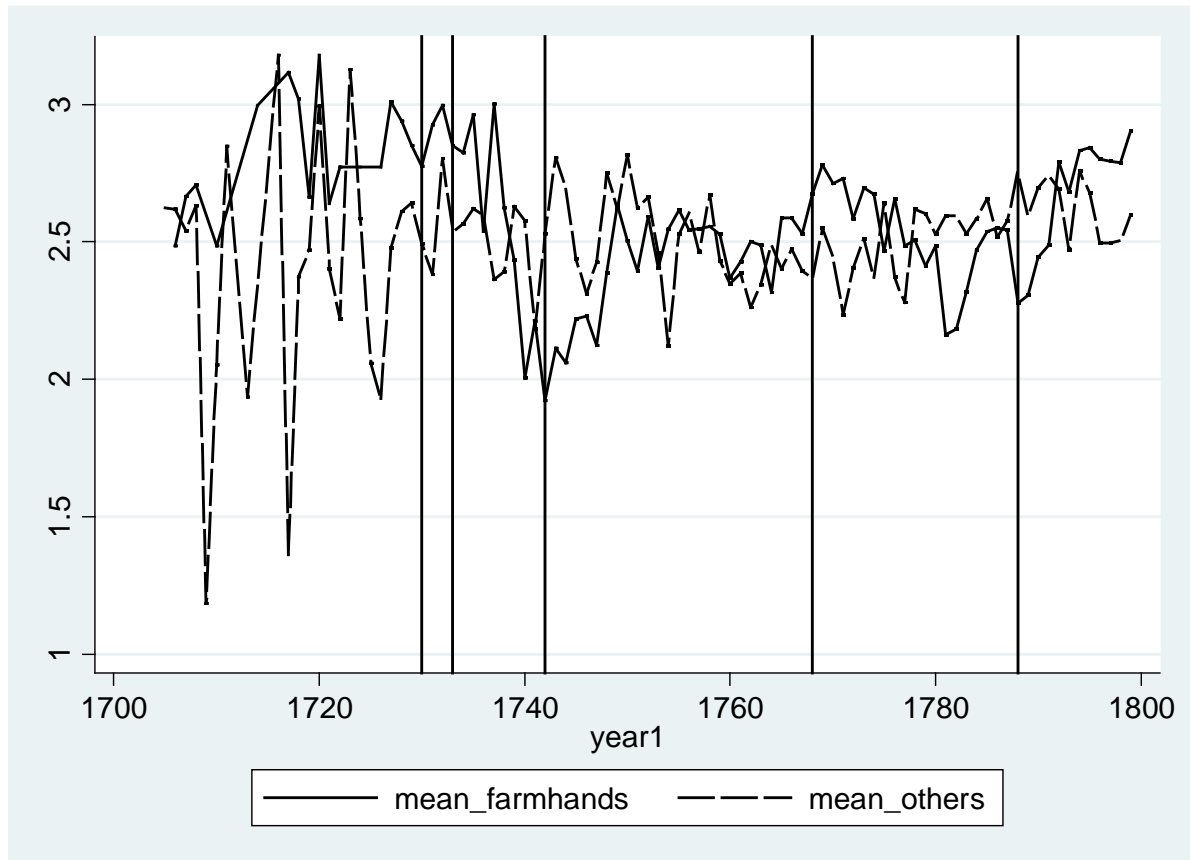
Table 4: Additional control variables

	Dependent variable				
	Log wage				
	(1)	(2)	(3)	(4)	(5)
Serfdom x Farmhand	-0.277** [-2.190]	-0.284* [-1.966]	-0.267** [-2.266]	-0.220** [-2.575]	-0.148** [2.48]
Price of Barley (log)	0.0207 [0.386]				0.002 [0.05]
Serfdom x Distance to Schleswig		0.00228 [1.718]			0.0029*** [3.57]
Serfdom x Distance to market town			-0.0656** [-2.179]		-0.0017** [2.10]
Serfdom x Distance to coast				-0.0552*** [-3.111]	-0.0059*** [3.54]
Observations	18,459	17,552	20,927	20,927	18,459
R-squared	0.654	0.666	0.65	0.65	0.65

## Supplementary appendix (for Online Publication)

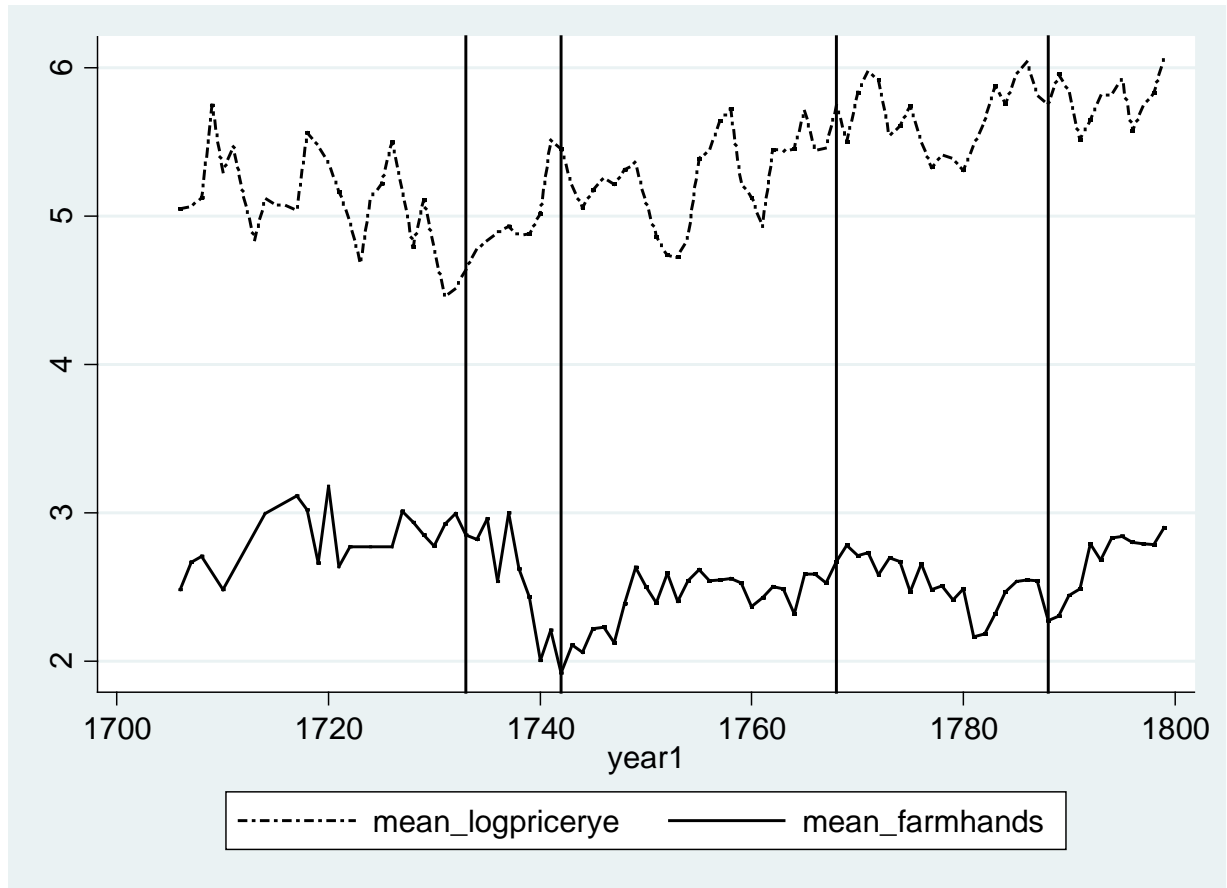
This supplementary appendix contains additional tables and figures mentioned in the main text.

Figure A1: Log wages, 1705-1799



Notes: The variable on the second axis is log wages. The vertical lines represent 1730, 1733, 1742, 1764 and 1788 respectively.

Figure A2: Log price of rye and farmhand wages, 1705-1799



Notes: The variable on the second axis is the log of the prices of rye and log farmhand wages. The vertical lines represent 1733, 1742, 1764 and 1788 respectively.

**Table A1: Existing empirical studies of serfdom**

Study	Data	Findings
<i>Cross-sectional design</i>		
Domar and Machina (1984)	(Estimated) Prices of serfs in Russian regions from different years in the period 1854-1858.	Unequal prices of serfs across Russian regions. Serf prices were different from zero indicating that serfdom was still profitable in the 1850s.
Klein and Ogilvie (2016)	Cross-sectional village level data for Bohemia for 1654	Presence of landholding of landlord/share of land held by landlord positively associated with more non-agricultural activity up to a point.
<i>Panel data</i>		
Nafziger (2012)	Village level panel data for the Moscow province, 1876-1899	Some persistent negative effects after abolition of serfdom on labor mobility.
Buggle and Nafziger (2017)	Mainly Russian district level data for various years	Persistent negative effects of serfdom on well-being.
<i>Differences-in-differences</i>		
Markevich and Zhuraskay (forthcoming)	Panel data for Russian provinces, 1800-1920.	Positive effect of abolition of serfdom on agricultural productivity.

**Table A2: Additional robustness checks.**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Serfdom x farmhand	-0.505***	-0.297*	-0.340**	-0.374**	-0.351***	-0.351***	
	[-8.477]	[-1.976]	[-2.242]	[-2.588]	[-4.058]	[-2.622]	
Serfdom x Farm laborer							-0.419**
							[-2.296]
Serfdom x laborer							-0.340*
							[-1.939]
Serfdom x day laborer							-0.257**
							[-2.535]
Observations	8,670	12,257	21,557	19,585	20,927	20,927	20,927
R-squared	0.779	0.603	0.656	0.551	0.649	0.649	0.649

Notes: The dependent variable is the natural logarithm of the daily wage. Columns (1) and (2) run the baseline model for Jutland & Funen and Zealand respectively; column (3) uses the full sample from 1661 to 1805, column (4) excludes children; Columns (6) and (7) cluster at the occupation level and the combined occupation and estate level respectively; column (7) split the main variable into three types of laborers used to code farmhand. All models include fixed effects for years, occupation, region, season, child (excluded in column 3), master (if craftsmen) and gender in columns (1)-(4). Jutland Funen year fixed effects are added in column 3; 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 A3: Results for log wages with Estate Fixed Fixed effects and estate time invariant control variables**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Period 1 x Farmhand	-0.158**		-0.231**				-0.401*	
	[-2.701]		[-2.273]				[-1.917]	
Period 2 x Farmhand	-0.0139		-0.0864				-0.352*	
	[-0.172]		[-0.981]				[-2.091]	
Period 3 x Farmhand	-0.0599		-0.139*				-0.377**	
	[-0.861]		[-1.985]				[-2.362]	
Period 4 x Farmhand	-0.0226		-0.131				-0.342*	
	[-0.207]		[-1.220]				[-1.906]	
Serfdom x Farmhand		-0.0445		-0.124*				-0.363**
		[-0.717]		[-1.866]				[-2.301]
Period 1 x Farmhand_alt					-0.594***			
					[-4.524]			
Period 2 x Farmhand_alt					-0.351*			
					[-1.841]			
Period 3 x Farmhand_alt					-0.270*			
					[-1.760]			
Period 4 x Farmhand_alt					-0.174			
					[-1.236]			
Serfdom x Farmhand_alt						-0.269*		
						[-1.814]		
Observations	20,927	20,927	18,459	18,459	20,927	20,927	20,927	20,927
R-squared	0.669	0.67	0.671	0.671	0.67	0.67	0.649	0.65

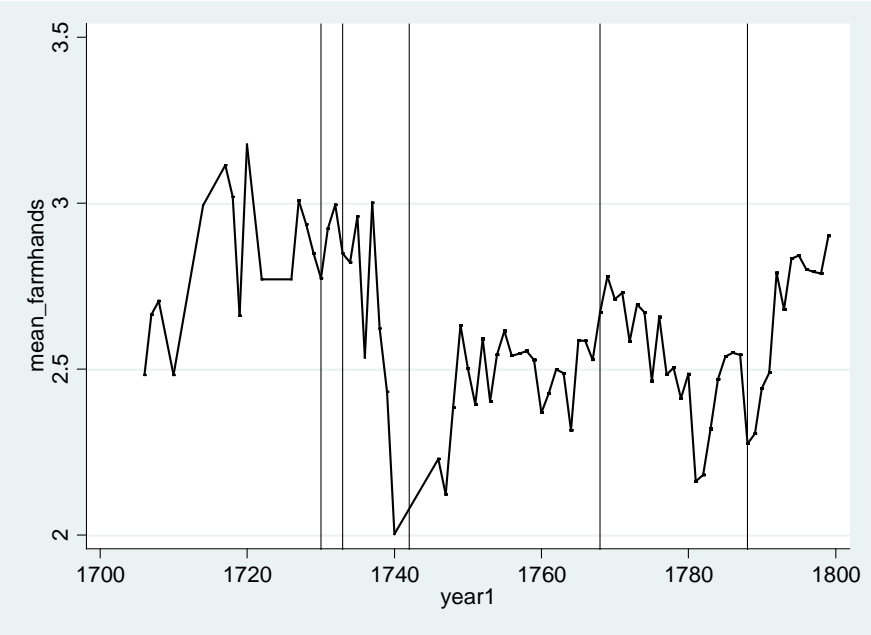
Notes: The dependent variable is the natural logarithm of the daily wage. Columns (1), (3) and (7) show the results for the non-flexible model; the variable “serfdom” represents a dummy variable which takes the value of 1 in the period in which serfdom affected workers (1733-1799); unskilled is represented by laborers, day laborers and farm laborers; Columns (2), (4) and (8) show the results for the flexible model: Period 1 is defined by the years 1733-1740, period 2 by 1741-1763, period 3 by 1764-1787 and period 4 by 1788-1799. Columns (5) and (6) use only farm laborers. The models in columns (1) to (6) include fixed effects for years, occupation, estate, season, child, master (if craftsmen) and gender. Columns (3) and (4) control for barley prices. Columns (7) and (8) use regional fixed effects a(nd not estate fixed effects) and add distance to a market town, distance the coast, distance to Schleswig and productivity adjusted size of the estate; 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 A4: Descriptive statistics for main variables**

Variable	Observations	Mean	Std. Dev.	Min	Max
<i>Individual level controls</i>					
log day wages	20,927	2.536	0.820	-2.526	5.951
Share farmhands	20,927	0.287	0.452	0.000	1.000
Share of data from Jutland and Funen	20,927	0.414	0.493	0.000	1.000
Share of women	20,927	0.084	0.277	0.000	1.000
Share of children	20,927	0.064	0.245	0.000	1.000
<i>Estate level controls</i>					
log barley price	18,459	5.189	0.314	4.159	5.951
Distance to market town	20,927	10.567	5.004	3.600	22.931
Distance to coast	20,927	7.109	6.192	0.192	22.340
Distance to Schleswig/Holstein	20,927	134.191	55.211	66.230	343.437
Productivity adjust size	20,927	140.588	116.476	38.000	547.000

**Figure A3: Farmhand wages on Zealand**



**Figure A4: Farmhand wages on Funen and Jutland**

