Was Domar Right?
Serfdom and Factor Endowments in Bohemia

Alexander Klein

School of Economics
University of Kent
United Kingdom
A.Klein-474@kent.ac.uk

Sheilagh Ogilvie
(corresponding author)
Faculty of Economics
University of Cambridge
United Kingdom
sco2@econ.cam.ac.uk

Abstract

Do factor endowments explain serfdom? Domar conjectured that high land-labor ratios caused serfdom by increasing incentives to coerce labor. But historical evidence is mixed and quantitative analyses are lacking. Using the Acemoglu-Wolitzky framework and controlling for political economy variables by studying a specific serf society, we analyze 11,349 Bohemian serf villages in 1757. The net effect of higher land-labor ratios was indeed to increase coercion. The effect greatly increased when animal labor was included, and diminished as land-labor ratios rose. Controlling for other variables, factor endowments significantly influenced serfdom. Institutions, we conclude, are shaped partly by economic fundamentals.

JEL Codes: J47, N33, O43, P48

Keywords: serfdom; land-labor ratio; institutions; labor coercion; rural-urban interaction

Acknowledgements: We would like to thank Daron Acemoglu, Mark Bailey, Dan Bogart, Chris Briggs, Steve Broadberry, Bruce Campbell, Nick Crafts, Tracy Dennison, Stan Engerman, James Fenske, Iain Fraser, Mark Harrison, John Hatcher, Phil Hoffman, Gary Libecap, Jean-Laurent Rosenthal, Alex Sapoznik, Leigh Shaw-Taylor, Guy Tchuente, Alexander Wolitzky, and Gavin Wright for stimulating comments on earlier drafts; Lenka Matušíková, Pavla Jirková, and Helena Klímová for generous help with the historical data sources; Eduard Maur for illuminating discussions of the Theresian Cadaster; and seminar participants at Caltech, UCL, the University of Cambridge, the University of Economics in Prague, the Economic History Society 2015 meeting in Wolverhampton, the European Historical Economics Society 2015 meeting in Pisa, the 2015 WEast conference in Budapest, and the 2016 conference on "Markets and States in History" at the University of Warwick, for helpful feedback. Renata McDonnell provided excellent research assistance. Finally, we would like to express our particular appreciation to Jeremy Edwards for numerous and extraordinarily helpful discussions of the theoretical framework and the econometric approaches.

1. Introduction

What causes labor coercion? It appears informally in most economies, but in some it prevails as a formal system of slavery or serfdom, with wide economic repercussions. Serfdom existed in most European economies for long periods between c. 800 and c. 1860. In many serf economies, a large percentage of rural families were obliged to do coerced labor for landlords. Since the rural economy produced 80 to 90 percent of pre-industrial GDP, serfdom affected the majority of economic activity. Labor coercion under serfdom reduced labor productivity, human capital investment, innovation, and living standards, so much so that its varying intensity is widely regarded as a major determinant of divergent European economic performance between 1350 and 1861 (Broadberry and Gupta 2006; Klein 2014; Ogilvie 2014a, 2014b; Baten and Szołtysek 2014; Markevich and Zhuravskaya 2017). So what caused this institutionalized labor coercion?

One well-known explanation is Domar's (1970) conjecture that coerced labor systems were caused by high land-labor ratios. In economies where wages were high because labor was scarce relative to land, Domar argued, landowners devised institutions such as serfdom and slavery to ensure they could get labor to work their land at a lower cost than would be the case in a non-coerced labor market.

This hypothesis has been strongly criticized, so much so that it might no longer seem to be of any relevance. Historians such as Postan (1937, 1966) had already argued that, on the contrary, rising land-labor ratios after the Black Death made serfdom decline, an argument generalized by North and Thomas (1973). Brenner (1976) pointed out that rises in the land-labor ratio coincided with the decline of serfdom in some European societies but its intensification in others. Subsequent scholarship argued that country-specific variables decided whether serfdom declined or intensified, with a different explanation proposed for each society (Aston and Philpin 1988; Hatcher and Bailey 2001).

Acemoglu and Wolitzky (2011) provided a general theoretical framework for analyzing how factor proportions affect labor coercion. They pointed out that a rise in the

land-labor ratio could have two countervailing effects. It might increase the price of the output produced by the landlord, which would increase the productivity of labor coercion, and thus increase the quantity of coercion, along the lines hypothesized by Domar. But it might also increase the wage that serfs could earn in outside activities, for instance in the urban sector, which would decrease the productivity of labor coercion, and thus decrease the quantity of coercion, as argued by Postan and North. Acemoglu and Wolitzky thus offered a theoretical framework explaining why Domar's theory might be compatible with the finding that high land-labor ratios could result in different coercion outcomes in different economies.

How factor proportions actually influenced serfdom is therefore an empirical question. Up to now, it has been studied mainly using qualitative evidence and descriptive approaches, in contrast to the quantitative analysis of factor endowments and slavery (e.g. Fenske 2012, 2013). To the best of our knowledge, this paper provides the first investigation of how factor proportions affected serfdom using quantitative evidence and multivariate statistical approaches. We hold constant political-economy variables – power, the state, and the institutional framework legitimizing labor coercion – by analyzing a specific serf society: Bohemia (part of the modern Czech Republic). We calculate quantitative measures of labor coercion, the land-labor ratio, urban potential, and other socio-economic characteristics of over 11,000 serf villages, covering the entirety of Bohemia in 1757. We use these data and the theoretical framework proposed by Acemoglu and Wolitzky (2011) to investigate how the land-labor ratio affected labor coercion, controlling for other causal variables.

We find that where the land-labor ratio was higher, labor coercion was also higher, and thus that the Domar effect outweighed any countervailing outside options effect. The net effect was not huge, but nor was it trivial, and it was much larger when labor coercion included both human and animal energy. The relationship between the land-labor ratio and labor coercion under serfdom displayed a nonlinear shape, arising from the technical limits on coercion in conditions of extreme labor scarcity. We also present evidence which supports Acemoglu and Wolitzky's conjecture that serfdom was

strong in eastern Europe partly because the urban sector was too weak to generate outside options for serfs that reduced the productivity of labor coercion.

Our findings demonstrate that factor proportions affected coercion. Even if political economy factors play a dominant role in explaining differences across countries and many other variables influenced landlord extraction from serfs, the land-labor ratio influenced labor coercion and thus contributed to serfdom as a broader institutional system. This in turn implies that institutions are influenced, at least to some degree, by economic fundamentals.

2. Theories of Labor Coercion under Serfdom

Why is labor coercion systematically strong in some places and not in others? Domar (1970) ascribed it to high land-labor ratios. Where labor is scarce relative to land, the cost of labor in a non-coerced labor market will be high. The owners of land as employers therefore have strong incentives to extract large amounts of coerced labor to ensure that the land will be worked at low cost, and will therefore establish and maintain institutions enabling systematic labor coercion. Domar argued that his conjecture was supported by the importance of slavery in under-populated parts of the Americas and serfdom in lightly-settled parts of Europe, notably in the east. Serfdom intensified in seventeenth-century Russia, he hypothesized, because the Muscovite colonial conquests increased the area of land relative to the existing population, motivating landlords to extract coerced labor from scarce peasant workers. According to Domar, conditional on employers having coercive power, slavery and serfdom were their market-driven responses to relative factor prices.

On the face of it, however, the historical evidence for serfdom raises serious problems for Domar's conjecture. Postan (1937, 1966) argued that low land-labor ratios caused by population growth in twelfth- and thirteenth-century England led to an intensification of labor coercion by reducing outside options for serfs, while high land-labor ratios caused by population losses during the Black Death (1348-9) conversely caused labor coercion to decline by increasing outside options for peasants in vacant rural

farms and urban workshops. North and Thomas (1970, 1973) used this reasoning to explain the decline of coerced serf labor in western Europe more generally, while Małowist (1973) argued that in thirteenth- and fourteenth-century eastern Europe, high land-labor ratios caused by low population densities stimulated feudal lords to make concessions to peasants and relax labor coercion.

Brenner (1976, 1982) went even further, completely dismissing all claims that factor proportions affected serfdom. Neither increases nor decreases in labor scarcity could explain extraction of coerced labor from serfs, he argued, since the continent-wide increase in land-labor ratios after the Black Death saw serfdom declining in some societies but intensifying in others. Brenner argued that it was class struggle, not factor proportions, that decided whether serfdom survived or disappeared.

Subsequent historical scholarship has also tended to dismiss Domar's idea. The fact that coerced labor under serfdom responded in widely varying ways to the huge changes in the land-labor ratio after the Black Death suggested that country-specific variables such as class struggle, state power, urban strength, and the overall institutional framework were decisive – although there remained huge disagreement about these variables, and a different story was told for each European society (Aston and Philpin 1988; Hatcher and Bailey 2001). In any case, the fact that similar changes in land-labor ratios affected serfdom in diametrically opposite ways in different societies seemed to imply that Domar's conjecture could be abandoned.

Acemoglu and Wolitzky (2011) breathed new life into the Domar hypothesis by providing a theoretical framework which explained why land-labor ratios might affect labor coercion differently in different contexts. In their model, assuming that labor coercion is possible, the quantity of coercion observed results from the interaction between a producer (the landlord under serfdom) and a worker (the serf), given the market price for the good produced by the landlord and the wage the serf can earn in outside activities. The land-labor ratio can affect both the price of the landlord's good and the outside option wage for the serf, with the relative size of these two effects determining the quantity of labor coercion. The first effect is the one hypothesized by

Domar: an increase in the land-labor ratio increases labor scarcity, in turn increasing the market price of the landlord good, the value of a successful productive outcome for the landlord, the value of serfs' effort, and hence the value of coercion to extract effort, resulting in more labor coercion. The second effect – the outside option effect – goes in the opposite direction: an increase in the land-labor ratio increases labor scarcity, in turn increasing the wage serfs can earn in outside options; serfs with more valuable outside options will be induced to work less hard for the landlord; less hard-working serfs will deliver less successful effort, reducing the value of coercion to the landlord, resulting in less labor coercion. A rise in the land-labor ratio can thus increase labor coercion via its effect on the price of the landlord good (the Domar effect) but decrease it via its effect on serfs' outside opportunities, for instance in the urban sector (the outside-option or Postan-North effect). The relative size of these two effects will vary with market demand for landlords' goods and wages for serfs outside the coerced sector, so the same rise in land-labor ratios can result in different labor coercion outcomes in different societies.

Acemoglu and Wolitzky thus offer a major advance over previous approaches to labor coercion by showing how a higher land-labor ratio can give rise to different outcomes in different contexts. In this paper, therefore, we use the Acemoglu-Wolitzky framework as a theoretical basis for carrying out what is, to the best of our knowledge, the first quantitative analysis of how the land-labor ratio affected labor coercion under serfdom, controlling for other potential influences.

3. Serfdom in Bohemia

We analyze the determinants of labor coercion in a specific serf economy: eighteenth-century Bohemia (part of the modern Czech Republic). Bohemia shared with most of Europe the experience of classical medieval serfdom, in which peasants were obliged to deliver coerced labor along with other payments to their landlords in return for being allowed to occupy land. In most of western Europe these obligations broke down in the late medieval period, but in Bohemia and most of eastern Europe, they survived and intensified in a development known as the "second serfdom" (Petráň 1964; Wright 1975;

Maur 1983; Klein 2014; Ogilvie 2014). Most scholars now date the Bohemian second serfdom to c. 1500, when many landlords began increasing the coerced labor they extracted from serfs, demanding it from previously exempt groups, and using it not just for farm work but also for textile manufacturing, ironworking, glassmaking, brewing, fish-farming, transportation, and many other activities. To enforce the delivery of coerced labor, as well as other rents and taxes, Bohemian landlords imposed restrictions on geographical mobility, marriage, household formation, settlement, inheritance, and land transfers. Although these developments can be observed in parts of Bohemia from c. 1500 onwards, they intensified after the Thirty Years War (1618-48). Almost all inhabitants of rural Bohemia were the enserfed subjects of their landlords, who were entitled to extract coerced labor from them and to regulate their economic and demographic decisions. Even after the formal abolition of Bohemian serfdom in 1781, villagers continued to be obliged to deliver coerced labor to their landlords until the Revolutions of 1848. Throughout the existence of serfdom in Bohemia, the state enforced landlords' legal prerogatives over serfs and prohibited competition among landlords for serfs, for instance by offering less labor coercion.

Bohemian serfdom thus corresponds with the assumptions of the Acemoglu and Wolitzky model in that it was a coerced labor system in which the institutional entitlements through which landlords coerced their serfs were legitimized by the political authorities. Bohemia provides a good context for investigating the effect of factor proportions on labor coercion, since its institutional framework granted landlords the right to extract labor from serfs by coercion, and within Bohemia the same political framework prevailed across the entire territory, making it possible to hold constant potential political economy influences.

We compiled data on all the villages in Bohemia in 1757, using a comprehensive tax register known as the Theresian Cadaster (*Tereziánský katastr*). This register was meticulously drawn up over a period of years, during which data were collected, checked, and corrected in four stages (Hradecký 1956; Chalupa et al. 1964-70). In the first stage, the state authorities required each landlord to provide a report on each householder in

each village on his estate; reports were certified by local village officials and manorial administrators from neighboring estates. In a second stage, landlord reports delivered in the first stage were checked by a state commission that visited each village. Finally, in the third and fourth stages, the emended reports were reviewed by a central commission and corrected on the basis of further local information. The results of these four stages of reporting, recording, checking, and correcting were published in 1757 as the so-called "final version" (*konečný elaborát*) of the Theresian Tax Cadaster, from which we draw our data.

The Theresian Cadaster recorded serfs' coerced labor obligations at the level of the village, which is therefore our unit of analysis. Table 1 shows descriptive characteristics of all 11,670 Bohemian villages in 1757. The state required landlords to report coerced labor obligations (*robota*) for all villages, but for unknown reasons 321 villages (2.8 percent of the total) were listed without this information. These missing values cannot be interpreted as zeros since, as Table 1 shows, the cadaster explicitly recorded 1,845 villages as having zero coerced labor obligations. We therefore excluded the 321 villages with missing values for coerced labor obligations, leaving a data set of 11,349 serf villages. A large majority of these, 84 percent of the total, were villages in which the inhabitants owed coerced labor obligations to the landlord. The inhabitants of the other 16 percent of villages, although not obliged to perform coerced labor, were still subject to the other constraints of serfdom, including restrictions on migration and property transfers, as well as the obligation to deliver to the landlord a variety of payments in money and kind (Ogilvie 2001, 2005a, 2005b; Klein 2014).

Our first step was to measure the level of labor coercion in each village. The 1757 cadaster recorded, for each village, the number of serf households required to provide coerced labor and the number of days they had to do it. Coerced labor obligations were sometimes recorded for the entire village, sometimes separately for each social stratum (full peasants, half-peasants, smallholders, cottagers), sometimes for distinct geographical sections of the village, sometimes by the year instead of by the week, sometimes for several villages together (fortunately in just 2.5 percent of cases), and many other

Table 1:
Characteristics of Villages, Bohemia 1757

Characteristic of village	No.	%
Is listed in Terezianský Catastr	11,670	100.0
Has no information on coerced labor obligations (*robota*)	321	2.8
Has information on coerced labor obligations (*robota*)	11,349	97.2
Has coerced labor obligations (*robota*) that are:		
Zero	1,845	16.3
Non-zero	9,504	83.7
Total villages of known coerced labor obligations (*robota*)	11,349	100.0
On estates with:		
No town	5,335	47.0
Any type of town (*město* or *městys*), of which:	6,014	53.0
Only full towns (*město*)	1,160	10.2
Only agro-towns (*městys*)	2,930	25.8
Both types of town (*město* and *městys*)	1,924	17.0
Total villages of known coerced labor obligations (*robota*)	11,349	100.0
On estates subject to landlord who is:		
Noble	10,063	88.7
Crown	315	2.8
Town	188	1.7
Religious institution	83	0.7
Small free estate	46	0.4
Mining estate	7	0.1
Other type of landlord	647	5.7
Total villages of known coerced labor obligations (*robota*)	11,349	100.0
Located in a settlement subject to:		
Unitary lordship	8,564	75.5
Fragmented lordship	2,785	24.5
Total villages of known coerced labor obligations (*robota*)	11,349	100.0

Notes: For variable definitions, see text.
Source: Tereziánský katastr 1757.

variants corresponding to specific local practice. Some serfs owed just human labor but many were required to supply work teams combining human and animal labor. We reduced these complexities to two alternative measures of coerced labor. The first

focused solely on human time, and comprised the total number of days of human labor the village was obliged to provide to its landlord each week. The second focused on the total work energy extracted from serf households: animal energy was converted to a numeraire (explained in Appendix 1) and combined with human labor to yield the total number of "serf-equivalent" days the village had to provide each week. As Table 2 shows, the average Bohemian village in 1757 owed its landlord 27 days of human-only labor, but 321 "serf-equivalent" days of human plus animal labor each week.

The 1757 cadaster also enabled the land-labor ratio in each village to be calculated. For the denominator, we used the number of "householders" (*hospodaře*) in the village. The most comprehensive information on Bohemian historical demography,

Table 2:
Summary Statistics for Villages, Bohemia 1757

Variable	Mean	Std. Dev.	Min.	Max.
Coerced labor services (human-only)	27.42	32.20	0.00	792.00
Coerced labor services (human-animal)	321.04	388.09	0.00	6,149.96
Total land-labor ratio (arable + pastoral + forest)	31.48	27.29	0.00	989.00
Arable + pastoral land-labor ratio	29.91	25.96	0.00	989.00
Arable-only land-labor ratio	27.74	25.34	0.00	989.00
Village size (number of households)	13.47	14.20	1.00	407.00
Urban potential of full towns inside estate	13.81	45.65	0.00	535.00
Urban potential of agro towns inside estate	8.86	19.72	0.00	255.62
Urban potential of full towns outside estate	325.33	59.40	132.59	875.47
Urban potential of agro towns outside estate	270.04	49.10	96.93	829.85
Urban potential of royal/free towns (outside estate)	169.94	80.52	57.15	854.13
Share of estate population in full towns	5.83	11.93	0.00	100.00
Share of estate population in agro-towns	7.11	11.38	0.00	100.00
Share of estate population in all towns (full & agro-towns)	12.94	15.51	0.00	100.00
Latitude	14.44	0.96	12.22	17.69
Longitude	49.88	0.53	48.61	51.04

Notes: N=11,349 (all villages with information on coerced labor obligations). Coerced labor obligations are measured as "serf-equivalent" days per week (see text). Land-labor ratios are measured as *strych* per household (1 *strych* = 0.29 hectares); 15 *strych* was minimum required for family of 4.5 persons to survive wholly from agriculture.
Source: Tereziánský katastr 1757.

derived from the 1651 religious census (*Soupis poddaných podle viry*), yields a mean household size of approximately 4.5 persons, which did not vary greatly across regions, villages, or social strata; available evidence indicates that mean household size did not expand or contract meaningfully between the seventeenth and the eighteenth century (Horská 1994; Horský and Maur 1994; Cerman 1994; Seligová 1996; Cerman and Štefanová 2002; Pazderová 2002). This justifies treating the number of households in a village as a proxy for its total labor supply.[1]

As Table 2 shows, Bohemian serf villages in 1757 were very small on average. Although the largest village had 407 households (and thus, based on a mean household size of 4.5, a total of c. 1,800 inhabitants), the smallest had only 1 household. The average village had only 13.5 households and thus a total of about 60 inhabitants.

To calculate the numerator of the land-labor ratio, we used the amount of land in the village. The Theresian Cadaster records the area of arable (crop-bearing) land, pasture, and forest, all measured in *strych* (a unit equivalent to 0.29 hectares). This yielded the three alternative measures of the land-labor ratio shown in Table 2. The total (arable-pastoral-forest) land-labor ratio was an average of 31.5 *strych* per household, the arable-pastoral land-labor ratio was 29.9, and the arable-only land-labor ratio was 27.7. We explored all three measures of the land-labor ratio in our econometric analyses, and found that they yielded virtually identical results (see Appendix 2). Our preferred measure uses total land (arable plus pastoral plus forest) on the grounds that it reflects all the land from which serfs in that village had to support themselves as well as to pay off the state and the landlord, and is thus the best measure in terms of influencing local factor prices, the core of the Domar conjecture and the Acemoglu-Wolitzky framework.

As mentioned above, Acemoglu and Wolitzky (2011) postulate that a higher land-labor ratio could not only increase labor coercion via the Domar effect, but decrease it by raising serfs' wages in outside options. They conjecture, based on historical studies

[1] This measure of the total labor supply in the village can be converted to the same units as the measure of coerced human labor in the village (days of coerced serf labor delivered to the landlord per week), by multiplying it by 4.5 (the mean number of persons per household) and then by 5 (the approximate number of working days per week).

arguing that towns weakened serfdom in western Europe, that a major source of outside options was the urban sector. To explore this possibility, we compiled information on Bohemian towns in 1757. The Theresian Cadaster divided towns into three main categories: seigneurial agro-town (*městys*), seigneurial full town (*město*), and royal town (*královské město*). Seigneurial full towns enjoyed greater institutional privileges than seigneurial agro-towns, were typically larger, and had an occupational structure more oriented to crafts and commerce. But both types of seigneurial town were located on lords' estates and their inhabitants were subject to a form of serfdom. Royal towns, by contrast, were not located on the estate of any lord, and their inhabitants were not subject to serfdom; typically they were larger than seigneurial towns and their occupational structure was more industrial and commercial (Míka 1978).

Bohemian towns varied along two dimensions: migration restrictions, which made towns on the home estate more accessible to serfs than towns outside the estate; and institutional type, in which royal towns, full towns, and agro-towns differed in size and occupational structure. We calculated urban potential by assigning each town to one of five categories: agro-town on or outside the same estate as a given village, full town on or outside the same estate as a village, and royal town. Then for each village in 1757, the distance from the village to each town in Bohemia was multiplied by the population of the town, giving the urban potential offered by that town to that village. This gave rise to five variables, measuring the urban potential to serfs in each village of full towns on the home estate, agro-towns on the home estate, full towns outside the home estate, agro-towns outside the home estate, and royal towns (by definition outside the home estate).

The 1757 Theresian Cadaster also provides information on a number of basic village characteristics, for which we control by including them as variables in our regression analyses. One feature of serfdom in Bohemia, as in most serf societies, is that it was exercised by a number of different types of landlord. As Table 1 shows, the vast majority of Bohemian villages (89 percent of the total in 1757) were subject to landlords who were individual nobles. The remaining 11 percent were subject to the crown, towns, religious institutions, or other miscellaneous types of landlord. We control for possible

influences of differing lordship by including dummy variables for each type of landlord in our regression models.

A further characteristic of serfdom in Bohemia, as in many other serf societies, was that some villages were located in settlements that lay under fragmented lordship, where different parts of the settlement or subsets of inhabitants were the serfs of different landlords. Since the existence and level of coerced labor obligations arose from the feudal relationship between a particular group of serfs and a particular landlord, and since each part of a settlement under fragmented lordship was administered by a separate manorial office and typically also a separate village headman, we treated each part of such a settlement as a separate village. Such villages comprise 24.5 percent of our sample. As robustness checks, we estimated our regressions both including a dummy variable registering whether a village was part of a settlement under fragmented lordship and dropping such villages altogether. As discussed in Appendix 3, fragmented lordship itself significantly reduced labor coercion, which we ascribe to such villages' typically being located outside the boundaries of the estates to which they belonged, increasing the costs of labor coercion. However, controlling for fragmented lordship had no effect on the impact of the other regression variables.

In 1757, Bohemia was divided into 1,316 estates (*panství*). Each estate differed from others in ways that cannot be observed. Estates were administered differently, according to the preferences, resources, administrative traditions, and customs of each generation of landlords, manorial officials, communal officials, and serfs (Weizsäcker 1913; Krofta 1919; Vacek 1916; Mika 1960; Hanzal 1964; Barbarova 1969; Longfellow 1978; Winkelbauer 1993; Ogilvie 2005a, 2005c). Some estates had elaborate administrations, others had modest manorial offices, and still others were administered personally by a single official or minor lord. Some landlords were permanently absent in Prague or Vienna and merely enjoyed the revenues delivered by their distant manorial administrators, while others were resident lords who monitored the behavior of their serfs and intervened in extraction of coerced labor and other dues. The customary rights, privileges, and jurisdictions of village courts, councils, officers, and headmen also varied

from one Bohemian estate to another. The jurisdictional and administrative autonomy of landlords combined with the migration restrictions of serfdom to sustain such differences across Bohemian estates. To allow for these unobserved estate-level influences on labor coercion, we incorporate estate-level fixed effects into our regressions, as discussed in detail in the next section.

4. Estimation Strategy

To investigate the effect of factor proportions on coerced labor under serfdom, we used our data on 11,349 Bohemian villages in 1757 to estimate a reduced-form relationship between labor coercion and the land-labor ratio, controlling for urban potential and other village characteristics. Generally, our regression specification can be written as follows:

$$Coercion_{i,j} = f(Land\text{-}Labor_{i,j}, \boldsymbol{Urban\ Potential_{i,j}}, \boldsymbol{X_{i,j}}, \varepsilon_{i,,j})$$

where *i* denotes a village and *j* an estate and *f* is the function relating coerced labor to the regressors. $Coercion_{i,j}$ denotes the number of days of coerced labor extorted from serfs per week village *i* on estate *j*. $Land\text{-}Labor_{i,j}$ denotes the land-labor ratio in village *i* on estate *j*. The vector $\boldsymbol{Urban\ Potential_{i,j}}$ is a vector of five variables denoting the potential for towns to offer serfs outside options in village *i* on estate *j*. The vector $\boldsymbol{X_{i,j}}$ includes village, estate, and region controls: the number of households in village *i* on estate *j*, village-level latitude and longitude, dummies for each type of estate lordship (noble, royal, ecclesiastical, etc.), and controls for the region (*kraj*) in which the village was located. We also allow for estate-level fixed effects although, for the reasons explained below, we do not estimate them directly. The error term in the equation is denoted by $\varepsilon_{i,,j}$.

The regression we estimate is a reduced-form one, so the coefficients on the land-labor ratio obtained from estimating this equation do not measure the Domar effect. They measure the net outcome of the two possible effects pointed out by Acemoglu and Wolitzky (2011), the positive Domar effect and the negative outside options effect. If the net effect of the land-labor ratio is positive, then one can say that the Domar effect dominates, even though the precise sizes of it and the outside options effect are unknown.

Our general regression specification allows for the possibility that the relationship between the land-labor ratio and labor coercion was a non-linear one by including the square of the land-labor ratio as a regressor. As labor scarcity rose, landlords might have approached a technical frontier of coercion, at which they were no longer able to extort additional labor regardless of its value to them. When the land-labor ratio rose above a certain level, labor might become so scarce that most of it was required to keep serfs themselves alive, reducing the increment the landlord could extract despite his intensified demand for it.

We also allow for the possibility of a non-linear relationship between labor coercion and village size by including the square of village size in the general specification. In extracting coerced labor from serfs, landlords were likely to encounter both economies and diseconomies of scale. In a very small village, the return to the minimum quantity of manorial manpower required to extort any coerced labor was low because the number of serfs available to provide labor was small. In a very large village, conversely, the costs of detecting shirking could be inflated by the potential for serfs to conceal their behavior behind larger numbers of other serfs. Such scale effects could give rise to a non-linear relationship between village size and coercion.

Labor coercion might also have been affected by interactions between the land-labor ratio and urban potential. In general, if the urban sector had any impact on a serf economy, one would expect the effect of a change in land-labor ratio to depend on urban opportunities and vice versa. This is because one would expect the extent of labor coercion in a village to depend on both the land-labor ratio in that village and the urban potential faced by serfs in that village. If two villages had identical land-labor ratios but differing urban potential because of differing location with respect to urban centers of different sizes, labor coercion in the two villages would typically differ, so there is no reason to expect an increase in land-labor ratio to have the same effect in both villages. To accommodate this possibility, our general regression specification included interaction terms between the land-labor ratio and measures of urban potential.

In estimating this regression using our data for eighteenth-century Bohemia, there are four main econometric issues to be addressed: concentration of the dependent variable at zero; unobserved estate effects; sample selection; and endogeneity. A substantial part of the distribution of our dependent variable is concentrated at zero, with about 16 percent of villages delivering no coerced labor although, as discussed in Section 3, landlords required serfs in these villages to deliver other payments in money and kind, controlled their migration and access to land, and restricted their demographic and economic choices. In addition, our 11,349 villages in Bohemia in 1757 are clustered into 1,361 estates; as already discussed, each estate had a distinct administrative regime and history, creating the possibility of unobserved estate-level effects that might influence labor coercion.

We follow Wooldridge (2010, Ch. 17) in regarding the villages that delivered no coerced labor as exhibiting a corner solution response rather than being left-censored, since these zero values are true zeros, not values that reflect the censoring of some hypothetical negative coerced labor values. In such circumstances OLS will give consistent estimates of the parameters of the regression model. However, the OLS assumption that the mean of coerced labor is a linear function of the regressors is unlikely to be satisfied, and the marginal effects of the regressors on coerced labor are unlikely to be constant for different regressor values. A natural alternative to OLS which does not suffer from these drawbacks is a Tobit regression model, and the resulting estimates will be consistent provided that the errors in the model are homoskedastic and normally distributed. However, when we estimated the Tobit model using our Bohemian data, the null hypotheses of homoskedastic and normal errors were strongly rejected. Thus there are also drawbacks to using a Tobit model for our data.

Our preferred estimation approach is one in which in which we allow for the possibility that different mechanisms generate the zero and the positive values of labor coercion. In this two-part model, the first part is a logit regression which models the probability that a village has positive coerced labor, while the second part uses OLS to estimate a linear model of coerced labor conditional on such labor being positive. The

same set of regressors was used in both parts. We estimated this two-part model using the Stata command *twopm* of Belotti et al. (2015). Although the two-part specification is the one on which we place the most emphasis, we also report the results of using the OLS and Tobit specifications.

Turning to unobserved estate-level effects on coerced labor, we do not want to assume that these are uncorrelated with the observed regressors. At first sight, the natural way of dealing with this would be to include estate fixed effects as regressors. With a linear regression, this can be achieved by using the within transformation, but the within transformation cannot be applied to non-linear regression models such as the Tobit and two-part ones that we wish to use. Thus we have to find a way of allowing for possible correlation between unobserved estate effects and observed regressors that does not involve either using large numbers of estate dummy variables (since if these were included in the non-linear regression models, the incidental parameters problem would mean the resulting estimates were all inconsistent) or the within transformation (which simply cannot be used with non-linear models). The solution we adopt is that of Mundlak (1978): we specify the unobserved estate effect as being a linear function of the estate-level means of all the observable regressors. This specification means that the estimates of the coefficients of the observable regressors in the Tobit and two-part regressions do allow for correlation between these regressors and unobservable estate effects.

Since OLS is a familiar estimation method and the within transformation is a straightforward way of allowing for unobserved estate-level effects that may be correlated with the regressors, we report such estimates of our general regression specification, abbreviating them as OLS within. For the OLS within model, we cluster the standard errors at the estate level, which means that they are robust to heteroskedasticity across the entire sample and also to correlation of errors within each estate. The OLS regression could only be carried out on a smaller sample because the within transformation could not be applied to estates with a single village, and hence these had to be dropped. To check for the effect of this difference in sample size on the difference between the non-linear and the OLS analyses, we estimate the two-part and the RE Tobit

regressions on the sample of 10,886 observations used for the OLS regressions; as Appendix 4 shows, the difference in sample size had little effect on the results.

Since the Tobit regression model is commonly used for cases in which the dependent variable is concentrated at zero, we also report Tobit estimates. The particular Tobit model that we estimate, which we abbreviate as RE (random effects) Tobit, is one in which we assume that there are unobserved estate-level random effects as well as the estate-level fixed effects that are a linear function of the estate-level means of the observable regressors. Panel data techniques can be used to estimate this model, with the panel comprising 1,357 estates for each of which there are observations of villages on the estate. Within each estate there is clustering so that errors are correlated across villages for a given estate. To allow for this clustering, we estimate the standard errors of our RE Tobit point estimates by bootstrapping with 500 replications, sampling the estates with replacement.

In our preferred two-part regression model, we assume that unobserved estate effects which are linear functions of the estate-level means of the observable regressors are present in both parts of the model. We cannot include unobserved estate-level random effects as in our Tobit model: instead we pool the data over the entire sample and cluster the standard errors at the estate level to allow for possible correlation of errors within estates.

A third econometric issue is that a sample selection problem may arise if the process determining whether a village has zero or non-zero coerced labor obligations is correlated with the process determining the level of non-zero coerced labor. Fortunately, the historical literature provides reassurance that this problem does not arise in our data. Studies of coerced labor in medieval and early modern Bohemian villages show that whether a village had coerced labor obligations at all was determined at an early date, typically at the foundation of the village, whereas the level of coerced labor obligations in those villages that had them was determined in a separate and later process. The types of rent paid by serfs in each village were laid down in the manorial rent-roll (*urbař*) issued when the village was founded. Most Bohemian villages were founded in the medieval

period; although some were established at later dates as forest was cleared and new settlements were set up, virtually all had been founded by 1700. The 16 percent of villages that had zero coerced labor obligations in 1757 thus enjoyed this status by virtue of privileges issued generations and in most cases centuries before 1757. Once the status of zero coerced labor obligations was granted, it was difficult to change it since a village could oppose the introduction of non-customary obligations much more effectively than it could resist incremental increases in the level of customary obligations. Changing from zero to non-zero coerced labor obligations was a process that was extremely long and costly, often involving litigation, appeals, and revolts; changing from lower to higher coerced labor was less difficult for the landlord and took place in a separate process (Strauß 1929; Míka 1960; Wright 1975; Klíma 1975; Macek 1982). There are substantial historical reasons, therefore, to justify our assumption that selection bias is not an issue that needs to be addressed.

A final econometric issue is the possibility that the quantity of coerced labor extracted by a landlord exerted a causal influence on the urban sector, the land-labor ratio, or both – i.e., that the dependent variable influenced the independent variables. Fortunately, such reverse causation is ruled out by the historical evidence. The institutional framework of serfdom itself deliberately made factor markets very rigid, precisely so as to facilitate extraction from serfs. This prevented the endogenous adjustment of the urban sector and the land-labor ratio to labor coercion, which might otherwise have created identification problems.

First, could high coerced labor in a village cause nearby towns to be weaker or stronger? Alternatively, could a landlord make a joint decision to impose heavy or light labor services in a village and regulate the strength or size of towns near that village? The historical evidence concerning the establishment and development of town privileges in Bohemia rules out these possibilities. Royal towns were by definition not affected by landlords' imposition of coerced labor on serf villages, since territorially they were located outside lords' estates and institutionally they were independent of seigneurial lordship. Seigneurial towns were subject to landlords, but the privileges of towns and the

coerced labor of serf villages came into being through historical processes that were widely separated. A town obtained its privileges in a particular time-period, from a particular lord, and in a particular set of economic and institutional circumstances; coerced labor obligations were imposed on villages at different time-periods, by a different lord or lords, and in different economic and institutional circumstances. Institutional rigidities internal to Bohemian serfdom meant that the processes of granting urban privileges and imposing coerced labor obligations on serfs were orthogonal to one another, so it was not possible for the latter to cause the former or for the two to be decided jointly (Pekař 1913; Klepl 1932; Placht 1957; Míka 1978; Dědková 1978; Maur 1983, 2002; Mumenthaler 1998; Miller 2007; Česáková 2013; Dvořák 2013; Ďurčanský 2013).

A second avenue of reverse causation might arise if coerced labor influenced the land-labor ratio. For this to happen, higher coerced labor obligations would have to cause land supply to change, labor supply to change, or both. With regard to land supply, the Bohemian property system made it extremely difficult to increase or decrease the size of farms. The size of each farm in the village was laid down in the village foundation charter, typically in the course of the medieval period. From that point on, it was forbidden to divide, combine, add to, or subtract from any farm through inheritance, sale or purchase (Procházka 1963). The only land not affected by the impartibility restrictions were fragments of waste and fallow which were too few and small to affect the total land supply to any economically meaningful degree. Empirical studies confirm that these institutional restrictions on changing farm size were enforced by communal and manorial institutions (Procházka 1963; Ogilvie 2005a; Klein 2014; Klein and Ogilvie 2016).

With regard to labor supply, although migration into or out of villages in response to changes in coerced labor obligations was compatible with the incentives of serfs, it was not compatible with the incentives of landlords. Indeed, landlords maintained and enforced the system of serfdom precisely in order to prevent serfs from avoiding labor coercion and other seigneurial burdens by migrating. To emigrate from his village, particularly to leave the estate but even to move inside the estate to a seigneurial town or

to a different village, a serf had to obtain the landlord's consent. Even temporary labor migration required a serf to obtain manorial permission, pay fees, provide personal or monetary guarantees, find a replacement worker, or satisfy some combination of these conditions. Serfs who migrated without permission were penalized – by fining, jailing, coerced servanthood, or retribution against family members – as were those who assisted illegal emigrants. Illegal migration was sufficiently costly and risky that serfs were willing to pay substantial fees to obtain migration permits from their landlords. Inside the estate, villages reported to the landlord any illegal emigration by their members because such emigration increased burdens for the remaining serfs. Between estates, landlords cooperated with one another and the state in penalizing illegally migrating serfs. A serf who emigrated illegally confronted a non-trivial expectation of direct penalties inflicted by the landlord, and if he succeeded in absconding had to abandon his property, family ties, and social capital (Klíma 1975; Maur 1983; Ogilvie 2005a, 2005b; Klein 2014). Mobility restrictions were a long-term, universal component of the institutional system of serfdom, which were put in place to prevent evasion of *all* exactions landlords imposed on peasants, not a short-term tactic endogenously adopted by landlords as a component of their current decision about the existence and quantity of coerced labor obligations in a village. Empirical studies of such serf migration as did take place in Bohemia show that it was almost completely restricted to non-householders, land-poor serfs, those who secured a replacement household or laborer, and others whose departure would not reduce the capacity of their village to deliver coerced labor or other payments and whose migration was therefore tolerated by communal and manorial authorities (Petráň 1964; Maur 1983; Štefanová 1999; Grulich 2005; Grulich 2013). The institutional framework of serfdom itself, which legitimized landlords' extraction of coerced labor from serfs, also legitimized restrictions on serf mobility to facilitate that extraction. This prevented labor supply in a village from declining in response to coerced labor, and thus rules out a causal dependence on labor coercion.

5. Econometric Analysis

We estimated the regression equation using the two-part approach discussed above, as well as using RE Tobit and OLS. As already mentioned, our initial regressions included interaction terms between the land-labor ratio and the urban potential variables, in order to explore whether the impact of urban potential on labor coercion depended on the land-labor ratio in the village or the impact of the land-labor ratio in the village depended on the urban potential available to serfs in that village. For human-only coerced labor, none of the coefficients on the interaction terms was significantly different from zero at conventional levels. For human-animal coerced labor, two of the interaction terms were significantly different from zero in the two-part regressions and one in the RE Tobit regressions. However, the economic significance of these interaction terms was almost non-existent, and taking account of them had very little effect on the estimated marginal effects of the land-labor ratio and the urban potential variables. For ease of exposition, therefore, the interaction effects are dropped from the regressions in Tables 3 and 4; they are presented and discussed in Appendix 5.

Tables 3 and 4 show the results of estimating the regression equation discussed above for human-only and human-animal coerced labor respectively, after dropping the interaction terms between urban potential and the land-labor ratio. The tables report the marginal effects implied by the two-part and RE Tobit regressions for easier comparison with the OLS coefficients. All three estimation methods yield virtually the same marginal effects and statistical significance for all variables except the land-labor ratio, where the two-part and RE Tobit marginal effects are both approximately twice the size of the OLS coefficient. For the reasons discussed above, the characteristics of our data strongly indicate the use of the two-part model, so we focus mainly on the two-part results in the discussion that follows.

What light do our regression results shed on the Acemoglu-Wolitzky theory about coerced labor under serfdom? As Tables 3 and 4 show, for both definitions of coerced labor, the marginal effect of the land-labor ratio is significantly different from zero, as is its squared term, implying a curvilinear relationship. Figure 1 graphs the elasticity of

Table 3: Regression Analysis of Determinants of Human-Only Coerced Labor, Bohemia, 1757

Variables	Two-part marginal effects	RE Tobit marginal effects	OLS within coefficients
Land-labor ratio	0.2934	0.2830	0.1415
	(0.0425)	(0.0436)	(0.0193)
	[0.2101 - 0.3766]	[0.1975 - 0.3685]	[0.1037 - 0.1793]
Land-labor ratio squared	-0.0014	-0.0016	-0.0002
	(0.0004)	(0.0004)	(0.0001)
	[-0.0022 - -0.0007]	[-0.0023 - -0.00078]	[-0.0004 - -0.00002]
Village size	2.296	1.933	2.195
	(0.0928)	(0.0815)	(0.0871)
	[2.114 - 2.478]	[1.773 - 2.092]	[2.024 - 2.366]
Village size squared	-0.0090	-0.0080	-0.0088
	(0.0019)	(0.0016)	(0.0014)
	[-0.0127 - -0.0052]	[-0.0111 - -0.0050]	[-0.0115 - -0.0061]
Urban potential of full towns inside estate	0.0056	0.0105	0.0072
	(0.0076)	(0.0062)	(0.0065)
	[-0.0092 - 0.0204]	[-0.0017 - 0.0227]	[-0.0055 - 0.0200]
Urban potential of agro-towns inside estate	-0.0192	-0.0166	-0.031
	(0.0253)	(0.0197)	(0.0265)
	[-0.0688 - 0.0304]	[-0.0552 - 0.0220]	[-0.0831 - 0.0211]
Urban potential of full towns outside estate	0.0089	0.0065	0.0053
	(0.0075)	(0.0078)	(0.0094)
	[-0.0058 - 0.0236]	[-0.0088 - 0.0217]	[-0.0132 - 0.0239]
Urban potential of agro-towns outside estate	-0.0141	-0.0073	-0.0108
	(0.0162)	(0.0134)	(0.0162)
	[-0.0457 - 0.0176]	[-0.0335 - 0.0189]	[-0.0426 - 0.0210]
Urban potential of royal towns	0.0085	0.0095	0.0083
	(0.0055)	(0.0044)	(0.0037)
	[-0.0022 - 0.0193]	[0.0009 - 0.0180]	[0.0012 - 0.0155]
Latitude	3.359	1.751	2.904
	(2.887)	(2.206)	(2.686)
	[-2.298 - 9.017]	[-2.572 - 6.074]	[-2.368 - 8.176]
Longitude	-4.397	-4.356	-3.158
	(3.908)	(3.546)	(3.605)
	[-12.06 - 3.263]	[-11.31 - 2.593]	[-10.23 - 3.918]
Region dummy variables	YES	YES	NO
Lordship type dummy variables	YES	YES	NO
Estate-level mean values of regressors	YES	YES	NO
R squared			0.399
Proportion correctly classified in first part	0.865		
R squared in second part	0.429		
Number of observations	11,342	11,349	10,886
Number of estates		1,357	894
Number of estates (first part)	1,355		
Number of estates (second part)	1,057		

Notes: We report marginal effects for two-part and Tobit regressions for ease of comparison with OLS coefficients. Standard errors are in parentheses, confidence intervals in brackets. For two-part and OLS within, standard errors are clustered at the estate level. For RE Tobit, standard errors are bootstrapped with resampling over estates. The two-part regression has only 11,342 observations because when it was estimated, the seven observations for one particular lordship type were dropped since these observations perfectly predicted zero coerced labour in the first-stage logit regression. The Tobit regression was estimated on 11,349 observations but marginal effects could only be calculated using 11,348 observations because for one observation a predicted value of the dependent variable required to calculate the marginal effects was missing. The OLS regression has just 10,886 observations because estates with only a single village cannot be included in the within transformation.

Table 4: Regression Analysis of Determinants of Human-Animal Coerced Labor, Bohemia, 1757

Variables	Two-part marginal effects	RE Tobit marginal effects	OLS within coefficients
Land-labor ratio	10.633	8.788	5.398
	(0.761)	(0.817)	(0.536)
	[9.141 - 12.125]	[7.187 -10.389]	[4.347 - 6.450]
Land-labor ratio squared	-0.0462	-0.0412	-0.0069
	(0.0064)	(0.0074)	(0.0029)
	[-0.0587 - -0.0336]	[-0.0556 - -0.0267]	[-0.0127 - -0.0012]
Village size	28.82	23.66	27.16
	(1.185)	(1.009)	(1.148)
	[26.50 - 31.15]	[21.69 - 25.64]	[24.91 - 29.41]
Village size squared	-0.1313	-0.1115	-0.1240
	(0.0177)	(0.0137)	(0.0142)
	[-0.1660 - -0.0967]	[-0.1383 - 0.0847]	[-0.1519 - -0.0961]
Urban potential of full towns inside estate	0.0192	0.0811	0.0306
	(0.1035)	(0.0835)	(0.0973)
	[-0.1836 - 0.2221]	[-0.0825 - 0.2447]	[-0.1603 - 0.2215]
Urban potential of agro-towns inside estate	-0.1020	-0.0811	-0.2440
	(0.2390)	(0.1839)	(0.2464)
	[-0.5704 - 0.3664]	[-0.4414 - 0.2793]	[-0.7277 - 0.2396]
Urban potential of full towns outside estate	0.2229	0.1779	0.1782
	(0.0893)	(0.0888)	(0.1117)
	[0.0479 - 0.3979]	[0.0038 - 0.3520]	[-0.0410 - 0.3973]
Urban potential of agro-towns outside estate	0.0781	0.1110	0.0982
	(0.2076)	(0.1828)	(0.2033)
	[-0.3289 - 0.4850]	[-0.2472 - 0.4692]	[-0.3008 - 0.4972]
Urban potential of royal towns	0.1186	0.1214	0.0945
	(0.0717)	(0.0549)	(0.0530)
	[-0.0218 - 0.2591]	[0.0137 - 0.2290]	[-0.0096 - 0.1986]
Latitude	-15.34	-25.94	-25.43
	(31.79)	(26.13)	(30.54)
	[-77.64 - 46.95]	[-77.15 - 25.28]	[-85.36 - 34.50]
Longitude	7.165	-1.368	30.93
	(61.08)	(51.12)	(55.76)
	[-112.56 - 126.89]	[-101.56 - 98.82]	[-78.50 - 140.37]
Region dummy variables	YES	YES	NO
Lordship type dummy variables	YES	YES	NO
Estate-level mean values of regressors	YES	YES	NO
R squared			0.393
Proportion correctly classified in first part	0.865		
R squared in second part	0.445		
Number of observations	11,342	11,349	10,886
Number of estates		1,357	894
Number of estates (first part)	1,355		
Number of estates (second part)	1,057		

Notes: We report marginal effects for two-part and Tobit regressions for ease of comparison with OLS coefficients. Standard errors are in parentheses, confidence intervals in brackets. For two-part and OLS within, standard errors are clustered at the estate level. For RE Tobit, standard errors are bootstrapped with resampling over estates. The two-part regression has only 11,342 observations because when it was estimated, the seven observations for one particular lordship type were dropped since these observations perfectly predicted zero coerced labour in the first-stage logit regression. The Tobit regression was estimated on 11,349 observations but marginal effects could only be calculated using 11,348 observations because for one observation a predicted value of the dependent variable required to calculate the marginal effects was missing. The OLS regression has just 10,886 observations because estates with only a single village cannot be included in the within transformation.

labor coercion with respect to the land-labor ratio according to the regression models in Tables 3 and 4, setting all other regressors at their sample mean values. All three estimation approaches imply that the elasticity of coercion with respect to the land-labor ratio is positive, indicating that the Domar effect outweighs the outside options effect, over virtually the whole range of values. For the reasons discussed in Section 4, one would expect the two non-linear models to give much more variation in the elasticities over the same range than does OLS, and this is exactly what we observe in Figure 1. The two-part and RE Tobit regressions imply elasticities that are initially higher than those for the corresponding OLS regressions but decline more steeply as the land-labor ratio rises, so that above a land-labor ratio of 65-70 they become lower than the OLS ones. As Appendix 4 shows, when the non-linear models are estimated on the OLS sample, the difference between the non-linear and OLS elasticities is even greater (see Figure A9).

For human-only coerced labor, the two-part regression yields an elasticity with respect to the land-labor ratio that is non-positive only in villages where the land-labor ratio is above 105 *strych* (c. 30 hectares) per household; this is true of just 1.3 percent of villages in 1757. The elasticity is modest but non-trivial, lying in the 0.20-0.34 range, for the three-quarters of villages where the land-labor ratio is below 40 *strych* (c. 12 hectares) per household. For the one-fifth of villages where the land-labor ratio is 40-70 *strych* (12-20 hectares) per household, the elasticity is smaller, lying between 0.1 and 0.2. Defining the land-labor ratio more narrowly, in terms of solely agricultural or solely arable land, results in a slightly larger effect of the land-labor ratio, at least for the approximately four-fifths of villages where those land-labor ratios lie below c. 40 *strych* per household (see Appendix 2, Figure A1). The findings reported in Table 3 concerning the effect of the land-labor ratio on human-only coerced labor are robust to differences in variable definitions, sample size, and estimation approaches (see Appendices 3-5).

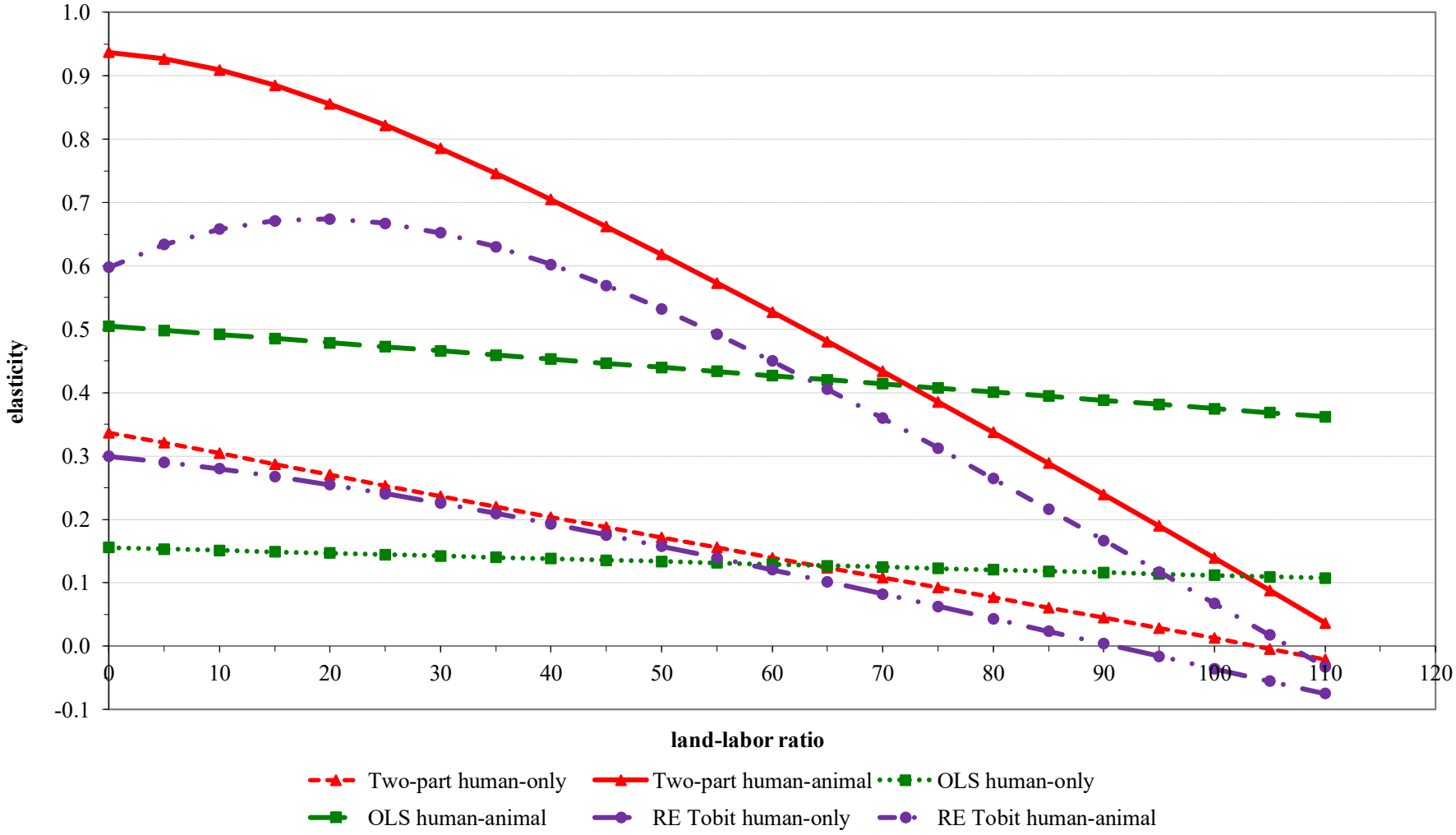
Our alternative definition of coerced labor includes animal alongside human labor to take account of the fact that landlords were extracting from serfs not just human time but work energy, which was often delivered by human-animal teams. The estimates in Table 4 reveal that the land-labor ratio had a larger effect on human-animal than human-

only coerced labor. As Figure 1 shows, the elasticities of human-animal labor with respect to the land-labor ratio are approximately three times as high as for human-only labor, and the difference between the two widens as the land-labor ratio rises. The OLS and two-part elasticities decrease across the entire range, while the RE Tobit elasticities follow an inverted-U shape peaking at around 20 *strych* per household; as Appendices 3 and 4 discuss, this inverted-U shape is robust to changes in sample size. The elasticities generated by our favoured two part model are substantial, lying in the 0.5-1.0 range, for the 92 percent of villages where the land-labor ratio is below 60 *strych* (c. 17 hectares) per household. They are still non-trivial, lying in the 0.2-0.5 range, for the 6 percent of villages where the land-labor ratio is between 60 and 90 *strych* (17-26 hectares) per household. Again, adopting a narrower definition of the land-labor ratio in terms of solely agricultural or solely arable land leads to a larger estimated effect of the land-labor ratio, at least for the 85 percent of villages where those land-labor ratios lie below 45 *strych* (13 hectares) per household (see Appendix 2).

For both human-only and human-animal coerced labor, therefore, the Domar effect outweighs the outside options effect across the vast majority of the range of land-labor ratios observed in eighteenth-century Bohemia. As the appendices show, this result is completely robust to different estimation approaches and alternative measures of the land-labor ratio. But our results reveal two interesting features: the effect of the land-labor ratio is larger on human-animal than human-only coerced labor; and the effect decreases as the land-labor ratio rises.

What explains the larger effect of the land-labor ratio on human-animal than on human-only labor coercion? We interpret it as reflecting both an enhanced Domar effect and a decreased outside options effect. The Domar effect was likely to be enhanced both by complementarities between human and animal work (increasing their value to the landlord in conditions of labor scarcity) and by the fact that animal labor was particularly useful for activities such as transporting grain to manorial breweries, wood to manorial glassworks, and ore to manorial ironworks (further increasing the value landlords placed on animal labor) (Klein 2014). The outside options effect was likely to be reduced by the

Figure 1: Elasticity of Coerced Labor with Respect to Land-Labor Ratio, Bohemia 1757



paucity of other uses for serfs' draft animals. The urban sector had much less demand for animal than for human labor, since its occupational structure was based on crafts and commerce which required manual dexterity, communication, and calculation more than brute force. In principle, peasants' own non-farm enterprises might have created other uses for animal labor, but landlords used their institutional powers to constrain serfs' crafts and trades where they threatened manorial interests (Ogilvie 2005a, 2005b, 2005c; Klein and Ogilvie 2016). Serfs might have deployed human-animal teams to take advantage of outside options illicitly, but the greater visibility of draft animals than humans meant landlords could detect, penalize, or tax the illicit use of animals more readily than serfs' illicit deployment of their own labor. All these factors reduced outside options for animal labor even more than for human labor.

What explains the second feature of our results, the decline in the elasticity of coerced labor with respect to the land-labor ratio as the latter rose? It could arise from the Domar effect being smaller in such villages, the outside options effect being larger, or both. We ascribe it to a smaller Domar effect. A higher land-labor ratio in a single village could hardly affect its serfs' outside option wage.[2] By contrast, a high land-labor ratio had a much greater capacity to affect the landlord's calculations in that village. In villages with very high land-labor ratios, labor was so scarce that even the impressive coercive capacities of landlords reached a technical frontier at which it became impossible to extract more coerced labor, regardless of the price of the landlord good, the consequent strength of landlord demand for labor, and the resulting high productivity of coercion. There was an irreducible minimum of labor which serf households themselves required in order to ensure survival and availability of any coerced labor. In villages with very high land-labor ratios, labor was so scarce that most of it was needed just to keep serfs themselves alive, so lords encountered technical constraints in extracting more of it.

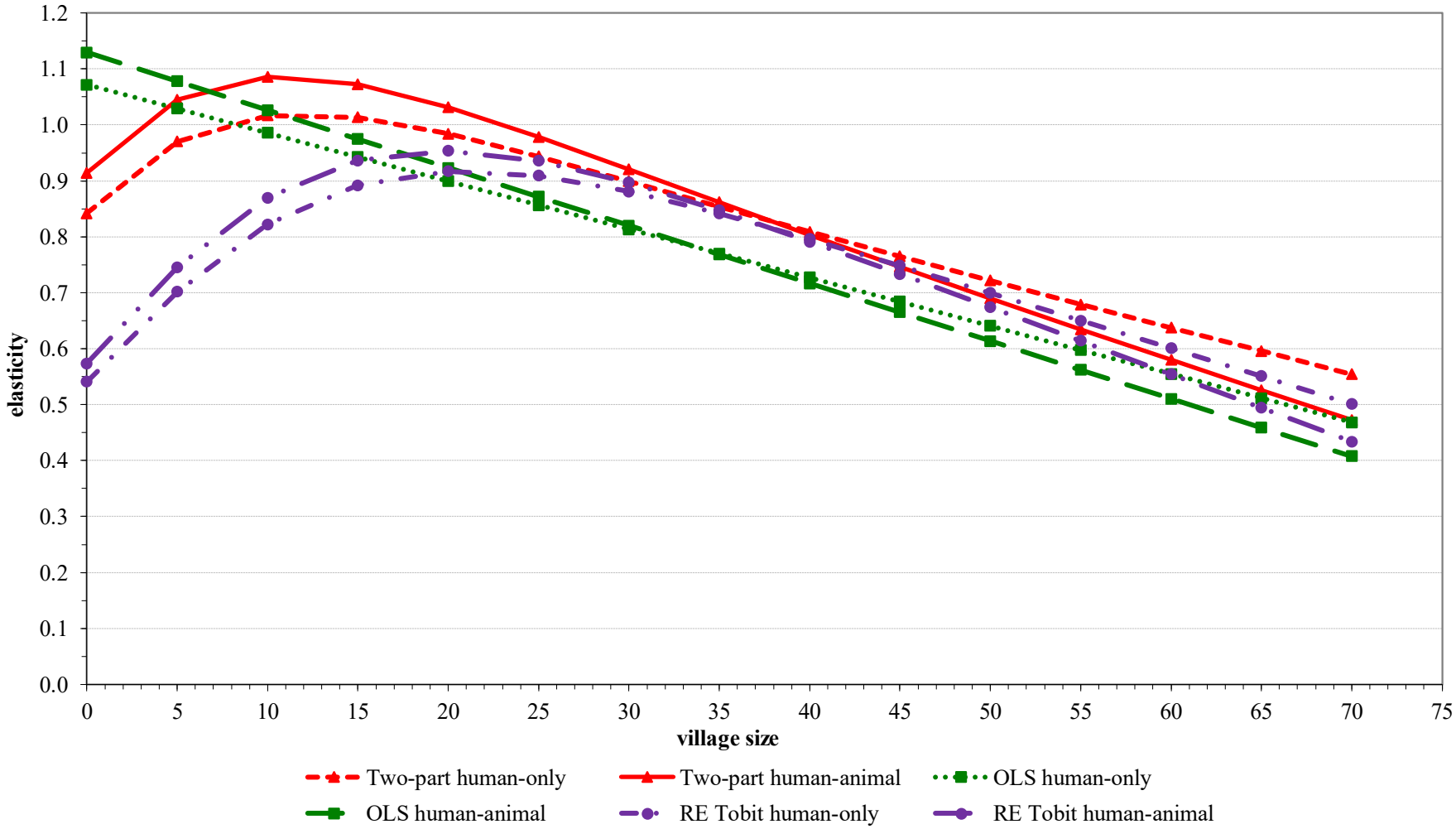
[2] The only exception might be if a particular village comprised a large share of the potential labor supply for a town located on the home estate. This case is theoretically possible but empirically irrelevant. First, the vast majority of Bohemian towns in 1757 were located on estates with multiple villages. Second, as we report below, there is no evidence that towns exercised a statistically or economically significant impact on labor coercion in Bohemian villages, implying that they did not offer significant outside options for serfs in any case.

This accounts for the declining, and ultimately zero or negative, elasticity of labor coercion with respect to the land-labor ratio when the latter reached very high values. In other words, when labor reached a state of extreme scarcity, market pressures broke through and even highly effective coercive techniques could not counteract them. This interpretation is borne out by the lenient behavior of Bohemian landlords in extracting coerced labor from serfs in drastically depopulated villages immediately after the Thirty Years War (see Cerman 1996; Ogilvie 2005a, 2005b, 2005c; Štefanová 1999; Zeitlhofer 2014).

In summary, our findings confirm the conjecture of Acemoglu and Wolitzky (2011) that in eastern European societies under the second serfdom, any outside options effects of high land-labor ratios that might have reduced labor coercion were outweighed by positive Domar effects. Controlling for the institutional framework, as we do here by analyzing labor coercion inside a specific society, the net effect of a higher land-labor ratio was to increase coercion. This effect is obscured in cross-country comparisons, such as those of Brenner (1976, 1988), in which the institutional framework varied from one society to the next, endowing lords with differing degrees of power, making labor coercion respond differently to changes in labor scarcity. By controlling for other potential influences, we provide clear evidence of the land-labor ratio increasing labor coercion.

One of the other potential influences on labor coercion was the size of serf villages. As can be seen in Tables 3-4 and Figure 2, the estimates from the two non-linear models are very similar to those from OLS, differing only for small villages with fewer than c. 15 households. The two-part and RE Tobit estimates show an inverted-U relationship between both measures of coerced labor and village size, while the OLS estimates show a downward-sloping relationship. This is what would be expected since, for the reasons discussed in Section 4, the two non-linear models should yield much more variation in the elasticities over the same range than OLS does. Appendices 2-5 show that this holds true for all alternative specifications of the regression model.

Figure 2: Elasticity of Coerced Labor with Respect to Village Size, Bohemia 1757



For human-only coerced labor, as Figure 2 shows, the elasticity with respect to village size rises from about 0.8 to about 1.1 as village size increases from 1 to 15 households, after which it declines gradually to 0.5 as village size increases from 15 to 70 households. For human-animal coerced labor, the elasticity rises from 0.9 to 1.1 as village size rises from 1 to 15 households, after which it declines gradually to 0.5.

Why would the elasticity of coercion with respect to village size follow this inverted-U shape? We argue that it reflects the fixed costs of coercion. To extract coerced labor, the landlord had to deploy some minimum amount of manpower, in terms of either his own visits to the village or the personal presence of officials. In a very small village, the aggregate return to the fixed cost of coercion was low, simply because there were so few serf households to deliver labor services. As villages became larger, the returns to coercion rose and hence a proportional increase in village size gave rise to a greater proportional increase in the quantity of coerced labor extracted. In the largest villages, the productivity of coercion fell again as it became progressively less possible to monitor the behavior of all serfs and penalize those who failed to deliver the amount of coerced labor demanded.

Opportunities in the urban sector also had the potential to affect labor coercion under serfdom. Historical studies describe towns as offering outside options to European serfs wishing to avoid labor coercion (Postan 1937, 1966; Carsten 1954; Blum 1957), and Acemoglu and Wolitzky (2011) identify the urban sector as a plausible source of outside opportunities which might make a rise in the land-labor ratio reduce the productivity of labor coercion by increasing serfs' outside option wage. However, an implication of the Acemoglu-Wolitzky model is that not only a change in the land-labor ratio but also a change in urban opportunities could have two countervailing effects on labor coercion – one by improving options for serfs, the other by doing so for landlords. This emerges from the basic idea behind the Acemoglu-Wolitzky model. Consider the situation in which urban opportunities expand while the land-labor ratio is held constant; this is the situation reflected by the coefficients on the urban potential variables in our regression model. Suppose first that an exogenous increase in the size of nearby towns does not

increase the price of the landlord good but increases both the outside option wage and general opportunities for serfs. Then serfs with better options in towns will be induced to work less hard in the village, hence will deliver less successful effort, reducing the value of coercion to the landlord, resulting in less labor coercion. Now suppose that the increase in nearby town size increases the price of the landlord good without improving serfs' opportunities. This increases the value of a successful productive outcome for the landlord, the value of serfs' effort, and hence the value of coercion, resulting in more labor coercion. The urban potential variables in our regression reflect the operation of these two different effects, and thus have no clear predicted sign.

As Tables 3 and 4 show, most categories of town exercise no statistically significant effect on either measure of labor coercion. For agro-towns both inside and outside the home estate, the effects on labor coercion are mostly negative, but not statistically significantly different from zero. For full towns inside the home estate, the effects are all positive, but either not statistically significantly different from zero or of borderline statistical significance (for human-only coerced labor, in the RE Tobit regression only); the appendices show the effects of this town type are not robust to alternative specifications. For full towns outside the estate, too, all effects are positive, but are statistically significantly different from zero only for human-animal labor, and only in the two-part and RE Tobit regressions; the effects of this town type are weakened but not wholly extinguished in the alternative specifications explored in the appendices. For royal towns, the effects are again all positive, but are not statistically significantly different from zero in the two-part model, although they are statistically significant in the RE Tobit and OLS regressions for human-only labor, and in the RE Tobit regressions for human-animal labor; the effect of this town type is not robust to the alternative specifications explored in the appendices.

More important than mere statistical significance, the *economic* significance of almost all measures of urban potential is very minor. According to the two-part estimates, the largest elasticity of coerced labor with respect to any category of town is 0.226 assessed at the sample mean, for full towns outside the estate on human-animal labor; as

discussed in the appendices, most alternative specifications generate smaller elasticities and lower statistical significance for this urban variable. For no other type of town does the absolute value of the two-part elasticity assessed at the sample mean exceed 0.14, and for towns on the home estate (those with lowest institutional barriers to access by serfs) the absolute value of the elasticity is always below 0.006. For royal towns, whose effect is most consistently significantly different from zero on both human and human-animal coerced labor, the elasticity at the sample mean of urban potential is just 0.053 for human-only labor and 0.063 for human-animal labor. These elasticities are of no conceivable economic significance.

What interpretation should be placed on the finding that no measure of urban potential has an economically significant effect on labor coercion, with the single exception of the mildly positive and not very robust effect of full towns outside the estate on human-animal labor? As already discussed, the Acemoglu-Wolitzky framework implies two countervailing effects of urban potential: increasing labor coercion via the price of the landlord good and reducing it via serfs' outside options. If we see towns having very little effect on coercion, this might because towns are having a big effect on both the landlord good and the serfs' outside wage and the two large effects are cancelling each other out. Or it might be that towns are having hardly any effect on either the landlord good or the serfs' outside wage, and hence little effect on coercion.

In the case of eighteenth-century Bohemia, everything that is known about the urban sector suggests the second explanation: towns were too feeble to affect the economy, whether by increasing the price of the landlord good or by increasing the serfs' outside wage. Bohemia, like other European societies in which serfdom survived into the eighteenth century, had an urban sector that was demographically and economically weak, limiting its capacity to provide an escape valve for serfs (Kahan 1973). This is illustrated by Table 5, which presents the European urbanization rates given in Malanima (2010). Across all of Europe in 1750 and 1800, about 12 percent of people lived in towns with at least 5,000 inhabitants and 8-9 percent in ones with at least 10,000. But urbanization was much higher in the eleven societies in which serfdom no longer

Table 5:
Urbanization Rates in European Societies, 1750 and 1800

Society	% population in towns over 5,000		% population in towns over 10,000	
	1750	**1800**	**1750**	**1800**
Economies with some surviving serfdom				
Austria-Hungary-Bohemia	3.2	3.9	2.6	3.2
Balkans	14.0	15.3	12.3	12.8
Germany	10.8	9.7	5.7	6.1
Poland	7.9	7.7	3.4	4.1
Russia (European)	3.2	4.6	2.5	3.6
Scandinavia	5.7	5.1	4.6	4.8
Average for societies with some serfdom	6.8	7.0	4.7	5.2
Societies without serfdom				
England & Wales	22.3	29.9	16.4	22.3
Scotland	15.3	36.6	11.5	23.9
Ireland	6.8	8.5	5.1	7.3
Netherlands	39.5	37.7	29.6	28.6
Belgium	25.9	24.2	15.8	16.6
France	12.5	12.5	8.7	8.9
Italy (Central & Northern)	13.6	14.2	14.2	13.4
Italy (Southern & Island)	19.4	21.0	19.4	21.0
Spain	14.0	19.3	9.1	14.7
Portugal	12.5	14.3	7.5	7.8
Switzerland	11.7	6.2	4.6	3.7
Average for societies without serfdom	**17.2**	**19.2**	**11.7**	**13.7**
Europe	11.7	12.4	8.0	9.0

Note: Average for serf and non-serf categories is calculated on the basis of total population.
Source: Calculated from Malanima (2010), pp. 260-2.

survived, where 17-19 percent of the population lived in towns with at least 5,000 inhabitants and 12-14 percent in towns with at least 10,000 inhabitants. By contrast, in the six European societies where serfdom survived into the eighteenth century, just 7 percent of the population lived in towns with at least 5,000 people and just 5 percent in towns with at least 10,000. The Austrian Habsburg possessions, which included Bohemia, had a low urbanization rate even by the standards of serf societies, with just 3-4 percent of the population living in towns over 5,000 inhabitants and just 2-3 percent in

towns over 10,000. In Bohemia as late as 1830, just 5.4 percent of the population lived in towns over 5,000 inhabitants and 3 percent in towns over 10,000; the average size of towns excluding Prague (the capital city) was just 2,103 inhabitants (Láník 1986).

This low degree of urbanization is reflected in the Theresian Cadaster. As Table 6 shows, of the total population of Bohemia in 1757 outside Prague, 72 percent were serfs living in villages. The settlements where the remaining 28 percent lived were designated as "towns", but this was based on legal and institutional status rather than demographic or economic importance. Prague itself had a population of about 60,000 in 1750 and Cheb had a population of 7,000-8,000, but no other Bohemian town exceeded 5,000 inhabitants (Míka 1978; De Vries 1984). According to the Theresian Cadaster, outside Prague only 12 Bohemian towns had more than 400 households (c. 1,800 inhabitants). The places recorded as towns in mid-eighteenth-century Bohemia were not characterized by large population size or economic importance, but were merely settlements that had succeeded,

Table 6:
Distribution of Population by Type of Settlement, Bohemia 1757

Total households living in:	**No.**	**%**
Villages	152,020	72.1
Any type of seigneurial town, of which:	46,248	21.9
Agro-towns	19,791	9.4
Full towns	26,457	12.6
Royal towns	12,517	5.9
All types of settlement	210,785	100.0
Total village households living on estates with:	**No.**	**%**
No type of town	65,835	43.1
Any type of town, of which:	87,016	56.9
Agro-towns only	39,232	25.7
Full towns only	19,651	12.9
Both agro-towns and full towns	28,133	18.4
Total households in villages	152,851	100.0

Notes: For definitions of different types of settlement, see text. Excludes Prague.
Source: Tereziánský katastr 1757.

centuries earlier, in obtaining urban "privileges" (Hoffmann 2009; Maur 2002; Miller 2007; Dvořák 2013).

As Table 7 shows, 8 percent of these settlements were royal towns, 31 percent were seigneurial full towns, and 61 percent were seigneurial agro-towns. Royal towns were the largest and most independent type of urban center, but they were few in number, with only 43 across the entirety of Bohemia. They were also small, so much so that they would not even register in the European urbanization rates shown in Table 5: the largest royal town had 749 households (less than 3,400 inhabitants), and the average royal town had just 291 households (about 1,300). The capacity of royal towns to offer outside options to serfs was additionally limited by their loss of political power and independence after 1547. Furthermore, they lay outside the feudal estates on which serfs lived, so a serf wishing to use their markets had to get his lord's permission or incur risks and penalties (Maur 1983; Ogilvie 2005a). The bulk of the urban sector in early modern Bohemia – over 92 percent of all urban settlements, sheltering 79 percent of urban households –

Table 7:
Characteristics of Towns, Bohemia 1757

Characteristic	Seigneurial agro-town (*městys*)	Seigneurial full town (*město*)	Royal or free town
Number of towns of that type	328	168	43
% of towns of that type	60.9	31.2	8.0
Total number of households in that type of town	19,791	26,457	12,517
% of total urban households in that type of town	33.7	45.0	21.3
Mean number of households per town of that type	60.3	157.5	291.1
Maximum number of households per town of that type	282	535	749
Minimum number of households per town of that type	3	17	113
Mean % households with more than 15 *strych* arable	32.8	18.0	16.9
Mean arable + pastoral + forest land-labour ratio	15.2	9.1	10.3
Mean arable + pastoral land-labour ratio	15.0	9.0	10.1
Mean arable land-labour ratio	14.3	8.9	9.9

Note: Excludes Prague.
Source: Tereziánský katastr 1757.

consisted of seigneurial towns, which were located inside feudal estates and subject to lords' administration and jurisdiction (Dědková 1978; Maur 2002; Hoffman 2009; Česáková 2013; Dvořák 2013). These were even smaller than royal towns, with an average of only 158 households (c. 700 inhabitants) in seigneurial full towns and 60 households (c. 270 inhabitants) in seigneurial agro-towns.

Many Bohemian serfs did not have any local access to urban opportunities. In 1757, as Table 6 reveals, 43 percent of serfs lived on estates without any type of town and 26 percent lived on estates with only agro-towns; that is, over two-thirds of serfs lived on estates without a full town. Although it was not impossible for serfs to visit towns outside the estate, the need to get manorial permission or migrate illegally increased the costs and risks of accessing the markets of urban centers outside the home estate.

As our regressions show, the only effects of urban potential that are statistically (and in one case economically) significant are positive, and thus indicate that the net effect of those towns was to increase rather than decrease labor coercion. If anything, those towns offered greater outside opportunities for lords than serfs. This is not surprising, since historical evidence shows both seigneurial and royal towns in Bohemia taking actions that stifled rather than increased the outside options open to serfs, specifically by restricting rural crafts and trades that competed with those practised by town citizens (Cerman 1996; Ogilvie 2001; Klein and Ogilvie 2016).

Against this background of a feeble and at times restrictive urban sector, it is not surprising that urban potential did not reduce labor coercion. Even villages located near larger towns experienced an urban potential that was extremely weak by European standards, and hence one that was unlikely to increase either the price of the landlord good (which might have increased labor coercion) or the outside option wage (which might have reduced coercion). Instead, the amount of coerced labor extracted from Bohemian serfs was virtually unaffected by variations in urban potential, resulting from the fact that towns were so weak that they offered few opportunities to either lords or serfs. Few and feeble towns also implied a weak outside option effect for the land-labor

ratio, which is consistent with our finding that the net effect of the land-labor ratio on coercion was always positive.

Our results show that factor proportions contributed significantly to labor coercion under serfdom, and thus that institutions are at least to some extent influenced by economic fundamentals. Serfdom is arguably the most important labor coercion institution ever observed as far as long-term growth effects are concerned. But economic historians since Brenner (1976) have tended to dismiss factor proportions as an influence on it. Because the same increase in land-labor ratios after the Black Death was followed by a decline of serfdom in some societies and an intensification in others, factor proportions were thought to exercise no impact. Serfdom, it was believed, arose from class struggle, royal strength, urban power, or other society-specific variables. Our findings, by contrast, show that when such variables are held constant by carrying out an analysis inside a particular society, factor proportions did indeed affect serfdom. Although political-economy variables were unquestionably important in explaining differences in serfdom across countries, our findings show that Domar was right in hypothesizing that the land-labor ratio also played an explanatory role. Political economy variables influenced whether landlords were entitled to coerce labor, but the degree to which they exercised this entitlement depended on the land-labor ratio. In turn, this had the potential to intensify their incentives to push for such entitlements to be created, maintained, and strengthened. Factor proportions in general, and the Domar effect in particular, are therefore part of the explanation for serfdom as a broader instititutional system. Economic fundamentals, our findings suggest, can influence institutions.

6. Conclusion

To the best of our knowledge, this paper provides the first quantitative, multivariate analysis of the determinants of labor coercion under serfdom. By analyzing a specific society, we hold constant political-economy variables and control for other characteristics of serf villages, both of which may have obscured the impact of factor proportions in previous studies. Our econometric analysis of an entire serf economy

shows that the net effect of a higher land-labor ratio was to increase labor coercion. The effect displays two additional features, both arising from the technology of coercion under serfdom: the effect of the land-labor ratio was much larger for human-animal than for human-only labor, and it declined as the land-labor ratio rose. In terms of the Acemoglu and Wolitzky (2011) framework, the Domar effect, whereby high land-labor ratios increased coerced labor by increasing the price of landlord output, outweighed any countervailing outside option effect, whereby high land-labor ratios might have reduced coercion by improving serfs' wages in outside activities (e.g. in towns).

As far as the effect of towns themselves is concerned, an implication of the model of Acemoglu and Wolitzky (2011) is that strong towns could exert countervailing effects on coercion, weakening it by improving serfs' outside options (as in western Europe) or strengthening it by improving the prices landlords got for their output. In Bohemia, our econometric results show that urban potential exercised little statistically or economically significant effect on labor coercion. Only for royal towns was there an effect that was usually statistically significant for both measures of coercion; one type of seigneurial town also exercised a significant effect, although only on human-animal coerced labor. All significant urban effects were positive, suggesting towns created greater opportunities for lords than serfs, but most were so small as to have no economic significance. In theory, towns' lack of impact on labor coercion might reflect big effects on both serfs' outside wages and the prices of landlords' goods, with the two effects cancelling each other out. But evidence on the urban sector in Bohemia and other parts of eastern-central Europe makes it more likely that it was because towns in serf societies were too few and weak to have any serious impact on serfdom, whether by increasing serfs' wages or the prices of landlords' output.

A final implication of our results is that factor proportions affect institutions. Even though political economy and a number of other variables influenced the extraction of coerced labor from serfs, the land-labor ratio also affected serfdom as a broader institutional system. This in turn implies that economic fundamentals help shape institutions.

Bibliography

Acemoglu, D. and A. Wolitzky (2011). "The Economics of Labor Coercion." *Econometrica* 79(2): 555-600.

Aston, T. H. and C. H. E. Philpin, eds. (1988). *The Brenner Debate*. Cambridge, Cambridge University Press.

Barbarova, E. (1969). "Funkce hejtmanů (úředníků) na rožmbergských panstvích." *Jihočeský sborník historický* 38: 198-207.

Baten, J. and M. Szołtysek (2014). "A Golden Age before Serfdom? The Human Capital of Central-Eastern and Eastern Europe in the 17th-19th Centuries." *MPIDR Working Paper* WP 2014-008.

Belotti, F., P. Deb, W. G. Manning and E. C. Norton (2015). "twopm: Two-part models." *Stata Journal* 15(1): 3-20.

Blodgett, L. L. (1979). "The 'Second Serfdom' in Bohemia: a Case Study of the Rožmberk Estates in the 16th Century." In I. Völgyes, ed., *The Peasantry of Eastern Europe*. New York, Pergamon Press. 1**:** 1-18.

Blum, J. (1957). "The Rise of Serfdom in Eastern Europe." *American Historical Review* 62(4): 807-836.

Brenner, R. (1976). "Agrarian Class Structure and Economic Development in Pre-Industrial England." *Past & Present* 70: 30-75.

Brenner, R. (1982). "The Agrarian Roots of European Capitalism." *Past & Present* 97: 20-112.

Broadberry, S. and B. Gupta (2006). "The Early Modern Great Divergence: Wages, Prices and Economic Development in Europe and Asia, 1500–1800." *Economic History Review* 59(1): 2-31.

Cameron, A. C. and D. L. Miller (2015). "A Practitioner's Guide to Cluster-Robust Inference." *Journal of Human Resources* 50(2): 317-372.

Cameron, A. C. and P. K. Trivedi (2005). *Microeconometrics: Methods and Applications*. Cambridge / New York, Cambridge University Press.

Campbell, B. M. S. (2003). "The Uses and Exploitation of Human Power from the 13th

to the 18th Century." In S. Cavaciocchi, ed., *Economia e energia secc. XIII-XVIII*. Florence, Firenze University Press: 183-211.

Carsten, F. L. (1954). *The Origins of Prussia*. Oxford, Clarendon Press.

Cerman, M. (1994). "Bohemia after the Thirty Years' War: Some Theses on Population Structure, Marriage and Family." *Journal of Family History* 19(2): 149-175.

Cerman, M. (1996). "Proto-industrialisierung und Grundherrschaft. Ländliche Sozialstruktur, Feudalismus und Proto-industrielles Heimgewerbe in Nordböhmen vom 14. bis zum 18. Jahrhundert (1381-1790)". Ph.D. dissertation, Vienna.

Cerman, M. and D. Štefanová (2002). "Wirtschaft und Sozialstruktur in den Herrschaften Frýdlant und Liberec, 1590-1750." In M. Cerman and H. Zeitlhofer, eds., *Soziale Strukturen in Böhmen*. Vienna / Munich, Oldenbourg**:** 70-87.

Česáková, M. (2013). *Rychtářské manuály města Náchoda. Prameny k dějinám náchodské městské správy 1. poloviny 17. století*. Praha, Nakladatelství Karolinum.

Chalupa, A., M. Lišková, J. Nuhlíček and F. Rajtoral (1964-70). *Tereziánky Katastr Český*. Prague, Archiv Ministerstva Vnitra.

De Vries, J. (1984). *European Urbanization 1500-1800*. Cambridge, MA, Harvard University Press.

Dědková, L. (1978). "Ocharana municipální města v Čechách." *Sborník Archivních Prací* 26(1): 292-353.

Domar, E. D. (1970). "The Causes of Slavery or Serfdom: a Hypothesis." *Journal of Economic History* 30(1): 18-32.

Ďurčanský, M. (2013). *Česká města a jejich správa za třicetileté války. Zemský a lokální kontext*. Praha, Nakladatelství Karolinum.

Dvořák, J. (2013). *Vrchnostenské město v ranném novověku. Vztahy k vrchnosti a vennskovskému zázemí na příkladu Svitav v 17. století*. Brno, Matice moravská.

Fenske, J. (2012). "Land Abundance and Economic Institutions: Egba Land and Slavery, 1830–1914." *Economic History Review* 65(2): 527-555.

Fenske, J. (2013). "Does Land Abundance Explain African Institutions?" *Economic*

Journal 123: 1363-1390.

Grulich, J. (2005). "Besitztransfer und regionale Mobilität der untertänigen Bevölkerung (Südböhmen, vom 16. bis 18. Jahrhundert)." In M. Cerman and R. Luft, eds., *Untertanen, Herrschaft und Staat in Böhmen und im "Alten Reich"*. Munich, Oldenbourg**:** 127-151.

Grulich, J. (2013). *Migrace městského a vesnického obyvatelstva*. České Budějovice, Jihočeská univerzita v Českých Budějovicích.

Hanzal, J. (1964). "Vesnická obec a samospráva na počátku 17. století." *Právněhistorické studie* 10: 135-147.

Hatcher, J. and M. Bailey (2001). *Modelling the Middle Ages: the History and Theory of England's Economic Development*. Oxford, Oxford University Press.

Hoffmann, F. (2009). *Středověké město v Čechách a na Moravě*. Prague, Nakladatelství Lidové noviny.

Horská, P. (1994). "Historical Models of the Central European Family: Czech and Slovak Examples." *Journal of Family History* 19(2): 99-106.

Horský, J. and E. Maur (1994). "Die Familie, Familienstrukturen und Typologie der Familien in der böhmischen Historiografie." *Historická demografie* 17: 7-36.

Hradecký, E. (1956). "Tereziánský katastr (rozbor fondu)." *Sborník archivních prací* 6(1): 105-135.

Kahan, A. (1973). "Notes on Serfdom in Western and Eastern Europe." *The Journal of Economic History* 33(1): 86-99.

Klein, A. (2014). "The Institutions of the 'Second Serfdom' and Economic Efficiency: Review of the Existing Evidence for Bohemia." In S. Cavaciocchi, ed., *Schiavitu e servaggio nell'economia europea. Secc. XI-XVIII.* Florence, Firenze University Press**:** 59-82.

Klein, A. and S. Ogilvie (2016). "Occupational Structure in the Czech Lands under the Second Serfdom." *Economic History Review* 69(2): 493-521.

Klepl, J. (1932). "Královská města česká počátkem 18. století." *Český Časopis Historický* 38: 261-284, 489-521.

Klíma, A. (1975). “Probleme der Leibeigenschaft in Böhmen.” *Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte* 62(2): 214-228.

Krofta, K. (1919). *Přehled dějin selského stavu u Čechách a na Moravě*. Prague, Tiskem Rolnické tiskárny v Praze.

Láník, J. (1986). “The Development of Czech Towns in the 1830’s to 1850’s.” *Hospodářské dějiny* 15: 377-410.

Macek, J. (1982). “The Emergence of Serfdom in the Czech Lands.” *East Central Europe / L'Europe du Center-Est* 9(1-2): 7-23.

Malanima, P. (2010). “Urbanization.” In S. Broadberry and K. H. O’Rourke, eds., *The Cambridge Economic History of Modern Europe, Vol 1: 1700-1870*. Cambridge, Cambridge University Press. 1: 235-263.

Małowist, M. (1973). *Wschód a Zachód Europy. Konfrontacja struktur społecznogospodarczych*. Warsaw.

Markevich, A. and E. Zhuravskaya (2017 forthcoming). “The Economic Effects of the Abolition of Serfdom: Evidence from the Russian Empire.” *American Economic Review*.

Maur, E. (1983). “K demografickým aspektům tzv. druhého nevolnictví.” *Historická demografie* 8: 7-43.

Maur, E. (2002). “Urbanizace před urbanizací.” In P. Horská, E. Maur and J. Musil, eds., *Zrod velkoměsta. Urbanizace českých zemí a Evropa* Prague, Paseka.

Míka, A. (1960). *Poddaný lid v Čechách v první polovině 16. století*. Praha, Nakladatelství Československé akademie věd.

Miller, J. (2007). *Urban Societies in East-Central Europe, 1500-1700*. Aldershot, Ashgate.

Mumenthaler, R. (1998). “Spätmittelalterliche Städte West- und Osteuropas im Vergleich: Versuch einer verfassungsgeschichtlichen Typologie.” *Jahrbücher für Geschichte Osteuropas* 46(1): 39-68.

Mundlak, Y. (1978). “On the Pooling of Time Series and Cross Section Data.” *Econometrica* 46(1): 69-85.

North, D. C. and R. P. Thomas (1971). “The Rise and Fall of the Manorial System: a Theoretical Model.” *Journal of Economic History* 31(4): 777-803.

North, D. C. and R. P. Thomas (1973). *The Rise of the Western World*. Cambridge, Cambridge University Press.

Ogilvie, S. (2001). “The Economic World of the Bohemian Serf: Economic Concepts, Preferences and Constraints on the Estate of Friedland, 1583-1692.” *Economic History Review* 54(3): 430-453.

Ogilvie, S. (2005a). “Communities and the ‘Second Serfdom’ in Early Modern Bohemia.” *Past & Present* 187: 69-119.

Ogilvie, S. (2005b). “Staat und Untertanen in der lokalen Gesellschaft am Beispiel der Herrschaft Frýdlant (Böhmen).” In M. Cerman and R. Luft, eds., *Untertanen, Herrschaft und Staat in Böhmen und im ‘Alten Reich’*. Munich, Oldenbourg**:** 51-86.

Ogilvie, S. (2005c). “Village Community and Village Headman in Early Modern Bohemia.” *Bohemia* 46(2): 402-451.

Ogilvie, S. (2014a). “Serfdom and the Institutional System in Early Modern Germany.” In S. Cavaciocchi, ed., *Schiavitu e servaggio nell’economia europea. Secc. XI-XVIII*. Florence, Firenze University Press**:** 33-58.

Ogilvie, S. (2014b). “Slavery and Serfdom in the European Economy: Contribution to Tavola Rotunda.” In S. Cavaciocchi, ed., *Schiavitu e servaggio nell'economia europea. Secc. XI-XVII*. Florence, Firenze University Press: 689-693.

Pazderová, A. (2002). “Die wirtschaftliche und soziale Entwicklung in der Herrschaft Rychnov nad Kněžnou.” In M. Cerman and H. Zeitlhofer, eds., *Soziale Strukturen in Böhmen*. Vienna / Munich, Oldenbourg**:** 43-58.

Pekař, J. (1913). “Ceský katastry 1654-1789.” *Český Časopis Historický* 19: 301-468.

Petráň, J. (1964). *Poddaný lid na prahu třicetileté války*. Prague, Nakladatelství československé akademie věd.

Placht, O. (1957). *Lidnatost a společenská skladba českého státu v 16.-18. století*. Prague, Nakladatelství československé akademie věd.

Postan, M. (1937). “The Chronology of Labor Services.” *Transactions of the Royal Historical Society* 20: 169-193.

Postan, M. M. (1966). “Medieval Agrarian Society in its Prime: England.” In M. M. Postan, ed., *The Cambridge Economic History of Europe, Volume 1: The Agrarian Life of the Middle Ages*. Cambridge, Cambridge University Press**:** 548-632.

Procházka, V. r. (1963). *Česká poddanská nemovitost v pozemkových knihách 16. a 17. století*. Praha,, Nakl. Československé akademie věd.

Seligová, M. (1996). “Die Entwicklung der Familie auf der Herrschaft Děčín in der Mitte des 17 Jh. unter Berücksichtigung seines wirtschaftlichen Charakters.” *Historická demografie* 20: 119-175.

Štefanová, D. (1999). *Erbschaftspraxis und Handlungsspielräume der Untertanen in einer gutsherrschaftlichen Gesellschaft. Die Herrschaft Frýdlant in Nordböhmen, 1558-1750.* Munich, Oldenbourg.

Strauß, E. (1929). *Bauernelend und Bauernaufstände in den Sudetenländern*. Prague, Verlag des Parteivorstandes der Deutschen Sozialdemokratischen Arbeiterpartei in der Tschechoslowakischen Republik.

United Nations Statistical Office (1987). *Energy Statistics: Definitions, Units of Measure, and Conversion Factors*. New York, United Nations.

Vacek, F. (1916). “Právo veské obce v 15. století.” *Agrární archiv* 28: 23-45.

Weizsäcker, W. (1913). “Das deutsche Recht der bäuerlichen Kolonisten Böhmens und Mährens im XIII. und XIV. Jahrhunderte.” *Mitteilungen des Vereines für Geschichte der Deutschen in Böhmen* 51: 476-542.

Wooldridge, J. M. (2010). *Econometric Analysis of Cross Section and Panel Data*. Cambridge, MA, MIT Press.

Wright, W. E. (1975). “Neo-Serfdom in Bohemia.” *Slavic Review* 34(2): 239-252.

Zeitlhofer, H. (2014). *Besitzwechsel und sozialer Wandel. Lebensläufe und sozioökonomische Entwicklungen im südlichen Böhmerwald, 1640-1840*. Wien, Böhlau.

Was Domar Right? Serfdom and Factor Endowments in Bohemia

Alexander Klein and Sheilagh Ogilvie

Appendices

These appendices report the robustness checks referred to in the body of the paper, exploring the sensitivity of our regression results to alternative definitions of variables, alternative estimation approaches, inclusion of additional village-level control variables, and differences in sample size across estimation approaches.

We conducted each robustness check separately, but also explored them in various combinations as well as including them all in a single joint robustness check. For clarity and concision, we present the results of the main robustness checks separately, but other results are available upon request.

Appendix 1:
Conversion between Human and Animal Labor

As discussed in the main text, we define coerced labor obligations in two ways: first, in terms of human workers only; and second, taking into account the fact that all but one category of coerced labor recorded in the 1757 Theresian Cadaster required serfs to send draft animals along with human workers. The first definition focuses solely on the coercive extraction of human time, while the second measures the work energy that landlords were extracting from serf households. Such work energy was often delivered by human-animal teams, and both contemporary economic agents and modern economists regard human and animal energy as fungible.

In the Theresian Cadaster, the coerced labor (*robota*) which serfs were required to carry out for landlords fell into nine categories: labor provided by a human worker (known as "hand labor"); labor provided by a human worker together with one, two, three, or four horses; and labor provided by a human worker together with one, two, three, or four oxen. With few exceptions (see below), the cadaster recorded the number of serf households in each village that had to provide each category of coerced labor, and the number of days they had to do it each week.

We convert animal to human energy using a conversion factor drawn from medieval England, which treated a horse as equivalent to 12.5 men and an ox to 6.6 (Campbell 2003, Table 1). An alternative conversion factor derived from modern developing economies treats a horse as equivalent to 10 men and an ox to 7.5 (United Nations Statistical Office 1987, Table 21). On the grounds that both medieval England and eighteenth-century Bohemia pre-dated the Agricultural Revolution and the Industrial Revolution, and both were subject to the institutional framework of serfdom, we conclude that agricultural practice, technology, incentives, and relative size and strength of animals and human beings in eighteenth-century Bohemia resembled medieval England more than modern developing economies. We therefore calculate human-animal coerced labor based on the conversion factor in Campbell 2003, and this is the measure used in our

regressions. Estimating the regressions using the conversion factor drawn from modern LDCs gives rise to very similar results, which are available on request.

As noted above, for most villages the cadaster recorded the number of serfs in each coerced labor category and the number of days the serfs in that category had to work for the landlord each week. For a small subset consisting of 195 villages, the cadaster recorded an aggregate number of days of coerced labor for the whole village for the year, but did not specify how many days were owed by each coerced labor category. In those 195 cases, we allocate the aggregate number of days of coerced labor across the different coerced labor categories that were present in that particular village according to the number of serfs in each category. In a few villages, the cadaster did not record the number of serfs in all coerced labor categories; in those cases, we assume that there was one serf in each coerced labor category involving animal labor that existed in that village, and that all other serfs in that village owed only human ("hand") labor; this yielded a minimum calculation of the amount of animal labor the village had to deliver to the landlord. We estimated the human-animal regressions in Table 4 with and without these 195 observations, and it made no difference to the results; details of these regressions are available on request.

Appendix 2:
Alternative Measures of the Land-Labor Ratio

For the reasons discussed in the body of the paper, our preferred measure of the land-labor ratio is calculated by defining the numerator in terms of the total land area of the village, consisting of arable (crop-growing) land, pastoral land, and forest. However, to test the robustness of our results we also estimate our regressions using two alternative measures of the land-labor ratio.

The first defines the numerator in terms of arable and pastoral land only, excluding forest. This is motivated by the consideration that the disproportionate extent of forest land in more mountainous parts of Bohemia might distort the results, and that we should focus solely on agricultural land (the arable land used for cultivating crops plus the pastoral land used for raising livestock). The results of estimating the regressions in Table 3 using this definition of the land-labor ratio are shown in Table A1.

Our second approach is to define the numerator solely in terms of arable fields, i.e. land used to cultivate crops. Arable cultivation was the most important livelihood source in early modern European rural economies and, as Table 2 shows, arable fields comprised the majority of land in Bohemian villages in 1757. This definition deals with the possibility that the disproportionate size of pastures and forests in more lightly settled parts of Bohemia might distort the results. The regression results for this measure of the land-labor ratio are shown in Table A2.

Regardless of how the land-labor ratio is measured, it has an effect on both measures of coerced labor which is positive and statistically significant at conventional levels according to all three estimation approaches (two-part, RE Tobit, and OLS). The negligible impact of using alternative measures of the land-labor ratio can be seen in Figure A1, which graphs the elasticities of coerced labor with respect to all three measures of the land-labor ratio, based on the two-part estimates in Tables 3, A1, and A2. For all measures of the land-labor ratio, the curve slopes downwards, with much larger elasticities and slightly more curvature in the case of human-animal coerced labor. The

Table A1: Arable and Pastoral Land-Labor Ratio (Excluding Forest)

Variables	Human-Only Labor			Human-Animal Labor		
	Two-part marginal effects	RE Tobit marginal effects	OLS within coefficients	Two-part marginal effects	RE Tobit marginal effects	OLS within coefficients
Land-labor ratio	0.3275	0.3226	0.1520	11.603	9.806	5.821
	(0.0448)	(0.0462)	(0.0193)	(0.748)	(0.825)	(0.521)
	[0.2396 - 0.4154]	[0.2321 - 0.4132]	[0.1141 - 0.1899]	[10.137 - 13.069]	[8.189 - 11.424]	[4.798 - 6.845]
Land-labor ratio squared	-0.0017	-0.0019	-0.0002	-0.0528	-0.0493	-0.0071
	(0.0004)	(0.0004)	(0.0001)	(0.0067)	(0.0077)	(0.0029)
	[-0.0026 - -0.0009]	[-0.0028 - -0.0011]	[-0.0004 - -0.00003]	[-0.0658 - -0.0397]	[-0.0643 - -0.0342]	[-0.0127 - -0.0015]
Village size	2.297	1.933	2.197	28.90	23.72	27.23
	(0.0853)	(0.0816)	(0.0871)	(1.104)	(1.011)	(1.150)
	[2.130 - 2.464]	[1.773 - 2.093]	[2.026 - 2.368]	[26.73 - 31.06]	[21.74 - 25.70]	[24.97 - 29.49]
Village size squared	-0.0090	-0.0080	-0.0088	-0.1320	-0.1118	-0.1244
	(0.0018)	(0.0016)	(0.0014)	(0.0161)	(0.0137)	(0.0142)
	[-0.0126 - -0.0054]	[-0.0111 - -0.0050]	[-0.0116 - -0.0061]	[-0.1636 - -0.1003]	[-0.1386 - -0.0851]	[-0.1524 - -0.0965]
Urban potential of full towns inside estate	0.0056	0.0103	0.0071	0.0180	0.0744	0.0250
	(0.0076)	(0.0062)	(0.0065)	(0.1041)	(0.0837)	(0.0979)
	[-0.0093 - 0.0205]	[-0.0018 - 0.0224]	[-0.0057 - 0.0198]	[-0.1861 - 0.2221]	[-0.0896 - 0.2384]	[-0.1671 - 0.2171]
Urban potential of agro-towns inside estate	-0.0174	-0.0151	-0.0298	-0.0401	-0.0299	-0.1987
	(0.0255)	(0.0199)	(0.0267)	(0.2555)	(0.1955)	(0.2631)
	[-0.0673 - 0.0325]	[-0.0541 - 0.0239]	[-0.0822 - 0.0226]	[-0.5409 - 0.4607]	[-0.4131 - 0.3533]	[-0.7150 - 0.3177]
Urban potential of full towns outside estate	0.0081	0.0059	0.0048	0.1934	0.1568	0.1565
	(0.0076)	(0.0078)	(0.0095)	(0.0953)	(0.0915)	(0.1146)
	[-0.0068 - 0.0230]	[-0.0095 - 0.0212]	[-0.0139 - 0.0234]	[0.0067 - 0.3801]	[-0.0224 - 0.3361]	[-0.0685 - 0.3815]
Urban potential of agro-towns outside estate	-0.0142	-0.0076	-0.0110	0.0696	0.0997	0.0885
	(0.0161)	(0.0134)	(0.0162)	(0.2070)	(0.1837)	(0.2028)
	[-0.0458 - 0.0174]	[-0.0339 - 0.0186]	[-0.0429 - 0.0208]	[-0.3361 - 0.4753]	[-0.2602 - 0.4597]	[-0.3097 - 0.4866]
Urban potential of royal towns	0.0082	0.0092	0.0080	0.1043	0.1089	0.0810
	(0.0055)	(0.0044)	(0.0037)	(0.0712)	(0.0549)	(0.0533)
	[-0.0026 - 0.0189]	[0.0006 - 0.0177]	[0.0008 - 0.0152]	[-0.0352 - 0.2439]	[0.0013 - 0.2164]	[-0.0236 - 0.1857]
Latitude	2.936	1.428	2.631	-32.13	-37.41	-36.00
	(2.806)	(2.164)	(2.626)	(33.92)	(28.67)	(32.15)
	[-2.564 - 8.436]	[-2.813 - 5.669]	[-2.524 - 7.785]	[-98.61 - 34.35]	[-93.60 - 18.77]	[-99.10 - 27.10]
Longitude	-4.235	-4.186	-3.090	12.94	3.477	33.51
	(3.872)	(3.498)	(3.595)	(56.21)	(47.86)	(53.41)
	[-11.82 - 3.353]	[-11.04 - 2.670]	[-10.15 - 3.965]	[-97.22 - 123.1]	[-90.33 - 97.28]	[-71.31 - 138.3]
Region dummy variables	YES	YES	NO	YES	YES	NO
Lordship type dummy variables	YES	YES	NO	YES	YES	NO
Estate-level mean values of regressors	YES	YES	NO	YES	YES	NO
R squared			0.399			0.396
Proportion correctly classified in first part	0.865			0.865		
R squared in second part	0.430			0.451		
Number of observations	11,342	11,349	10,886	11,342	11,349	10,886
Number of estates		1,357	894		1,357	894
Number of estates (first part)	1,355			1,355		
Number of estates (second part)	1,057			1,057		

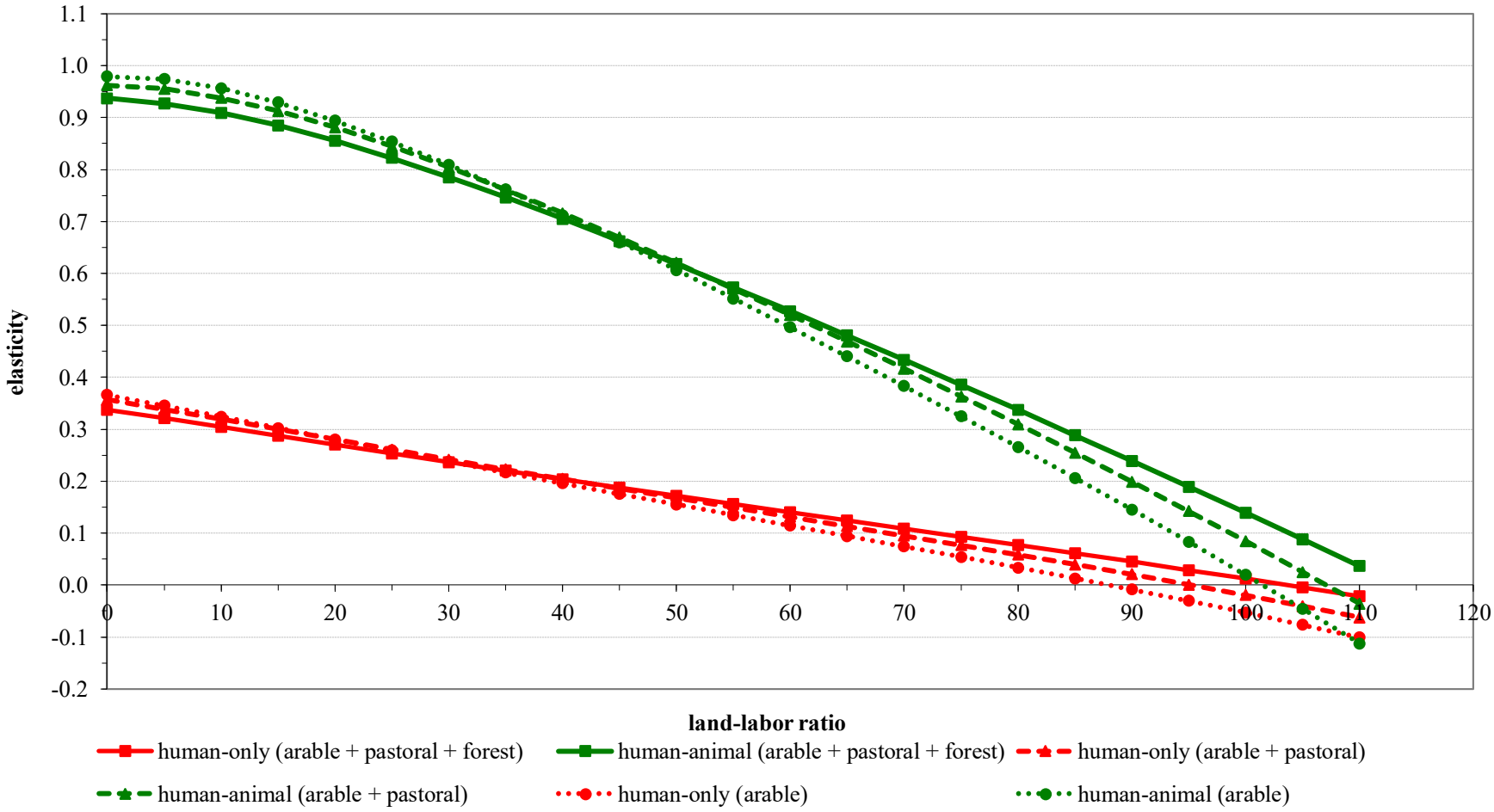
Notes: Standard errors are in parentheses, confidence intervals in brackets. For two-part and OLS within, standard errors are clustered at the estate level. For RE Tobit, standard errors are bootstrapped with resampling over estates. The two-part regression has only 11,342 observations because when it was estimated, the seven observations for one particular lordship type were dropped since these observations perfectly predicted zero coerced labor in the first-stage logit regression. The Tobit regression was estimated on 11,349 observations but marginal effects could only be calculated using 11,348 observations because for one observation a predicted value of the dependent variable required to calculate the marginal effects was missing. The OLS regression has just 10,886 observations because estates with only a single village cannot be included in the within transformation.

Table A2: Arable-Only Land-Labor Ratio (Excluding Pastoral and Forest)

Variables	Human-Only Labor			Human-Animal Labor		
	Two-part marginal effects	RE Tobit marginal effects	OLS within coefficients	Two-part marginal effects	RE Tobit marginal effects	OLS within coefficients
Land-labor ratio	0.3621	0.3481	0.1555	12.799	10.791	6.175
	(0.0421)	(0.0430)	(0.0196)	(0.704)	(0.755)	(0.552)
	[0.2796 - 0.4445]	[0.2638 - 0.4323]	[0.1170 - 0.1940]	[11.419 - 14.178]	[9.312 - 12.271]	[5.092 - 7.259]
Land-labor ratio squared	-0.0021	-0.0022	-0.0002	-0.0617	-0.0563	-0.0074
	(0.0004)	(0.0004)	(0.0001)	(0.0058)	(0.0068)	(0.0029)
	[-0.0028 - -0.0014]	[-0.0030 - -0.0014]	[-0.0004 - -0.00003]	[-0.0730 - -0.0504]	[-0.0695 - -0.0431]	[-0.0131 - -0.0016]
Village size	2.295	1.935	2.196	28.87	23.83	27.23
	(0.0841)	(0.0818)	(0.0871)	(1.090)	(1.003)	(1.151)
	[2.130 - 2.460]	[1.774 - 2.095]	[2.025 - 2.367]	[26.74 - 31.01]	[21.87 - 25.80]	[24.97 - 29.49]
Village size squared	-0.0090	-0.0080	-0.0088	-0.1318	-0.1123	-0.1245
	(0.0018)	(0.0016)	(0.0014)	(0.0160)	(0.0142)	(0.0143)
	[-0.0125 - -0.0054]	[-0.0111 - -0.0050]	[-0.0116 - -0.0061]	[-0.1632 - -0.1005]	[-0.1400 - -0.0845]	[-0.1525 - -0.0965]
Urban potential of full towns inside estate	0.0054	0.0100	0.0070	0.0112	0.0644	0.0203
	(0.0076)	(0.0062)	(0.0065)	(0.1032)	(0.0896)	(0.0984)
	[-0.0094 - 0.0202]	[-0.0021 - 0.0222]	[-0.0058 - 0.0197]	[-0.1910 - 0.2134]	[-0.1113 - 0.2401]	[-0.1728 - 0.2135]
Urban potential of agro-towns inside estate	-0.0171	-0.0149	-0.0302	-0.0283	-0.0255	-0.2104
	(0.0255)	(0.0200)	(0.0268)	(0.2552)	(0.2131)	(0.2628)
	[-0.0672 - 0.0330]	[-0.0541 - 0.0244]	[-0.0827 - 0.0224]	[-0.5284 - 0.4718]	[-0.4432 - 0.3922]	[-0.7261 - 0.3053]
Urban potential of full towns outside estate	0.0077	0.0054	0.0045	0.1771	0.1416	0.1432
	(0.0076)	(0.0078)	(0.0095)	(0.0918)	(0.0944)	(0.1131)
	[-0.0071 - 0.0225]	[-0.0099 - 0.0207]	[-0.0142 - 0.0231]	[-0.0027 - 0.3569]	[-0.0435 - 0.3267]	[-0.0788 - 0.3651]
Urban potential of agro-towns outside estate	-0.0156	-0.0090	-0.0118	0.0133	0.0535	0.0538
	(0.0160)	(0.0134)	(0.0162)	(0.2046)	(0.1797)	(0.2035)
	[-0.0471 - 0.0158]	[-0.0353 - 0.0173]	[-0.0437 - 0.0200]	[-0.3878 - 0.4144]	[-0.2987 - 0.4057]	[-0.3455 - 0.4531]
Urban potential of royal towns	0.0077	0.0088	0.0077	0.0869	0.0948	0.0685
	(0.0055)	(0.0043)	(0.0036)	(0.0699)	(0.0560)	(0.0529)
	[-0.0029 - 0.0184]	[0.0003 - 0.0173]	[0.0006 - 0.0149]	[-0.0501 - 0.2239]	[-0.0150 - 0.2046]	[-0.0354 - 0.1723]
Latitude	3.020	1.512	2.681	-30.03	-35.58	-34.92
	(2.814)	(2.178)	(2.629)	(33.52)	(31.39)	(31.77)
	[-2.496 - 8.536]	[-2.757 - 5.781]	[-2.480 - 7.841]	[-95.73 - 35.67]	[-97.10 - 25.93]	[-97.28 - 27.44]
Longitude	-4.192	-4.091	-3.020	13.26	6.501	36.15
	(3.854)	(3.485)	(3.579)	(55.52)	(49.95)	(53.20)
	[-11.75 - 3.362]	[-10.92 - 2.739]	[-10.04 - 4.005]	[-95.57 - 122.1]	[-91.39 - 104.39]	[-68.26 - 140.6]
Region dummy variables	YES	YES	NO	YES	YES	NO
Lordship type dummy variables	YES	YES	NO	YES	YES	NO
Estate-level mean values of regressors	YES	YES	NO	YES	YES	NO
R squared			0.399			0.398
Proportion correctly classified in first part	0.865			0.865		
R squared in second part	0.433			0.460		
Number of observations	11,342	11,349	10,886	11,342	11,349	10,886
Number of estates		1,357	894		1,357	894
Number of estates (first part)	1,355			1,355		
Number of estates (second part)	1,057			1,057		

Notes: Standard errors are in parentheses, confidence intervals in brackets. For two-part and OLS within, standard errors are clustered at the estate level. For RE Tobit, standard errors are bootstrapped with resampling over estates. The two-part regression has only 11,342 observations because when it was estimated, the seven observations for one particular lordship type were dropped since these observations perfectly predicted zero coerced labor in the first-stage logit regression. The Tobit regression was estimated on 11,349 observations but marginal effects could only be calculated using 11,348 observations because for one observation a predicted value of the dependent variable required to calculate the marginal effects was missing. The OLS regression has just 10,886 observations because estates with only a single village cannot be included in the within transformation.

Figure A1: Elasticity of Coerced Labor with Respect to Different Measures of the Land-Labor Ratio, Two-Part Estimates, Bohemia 1757

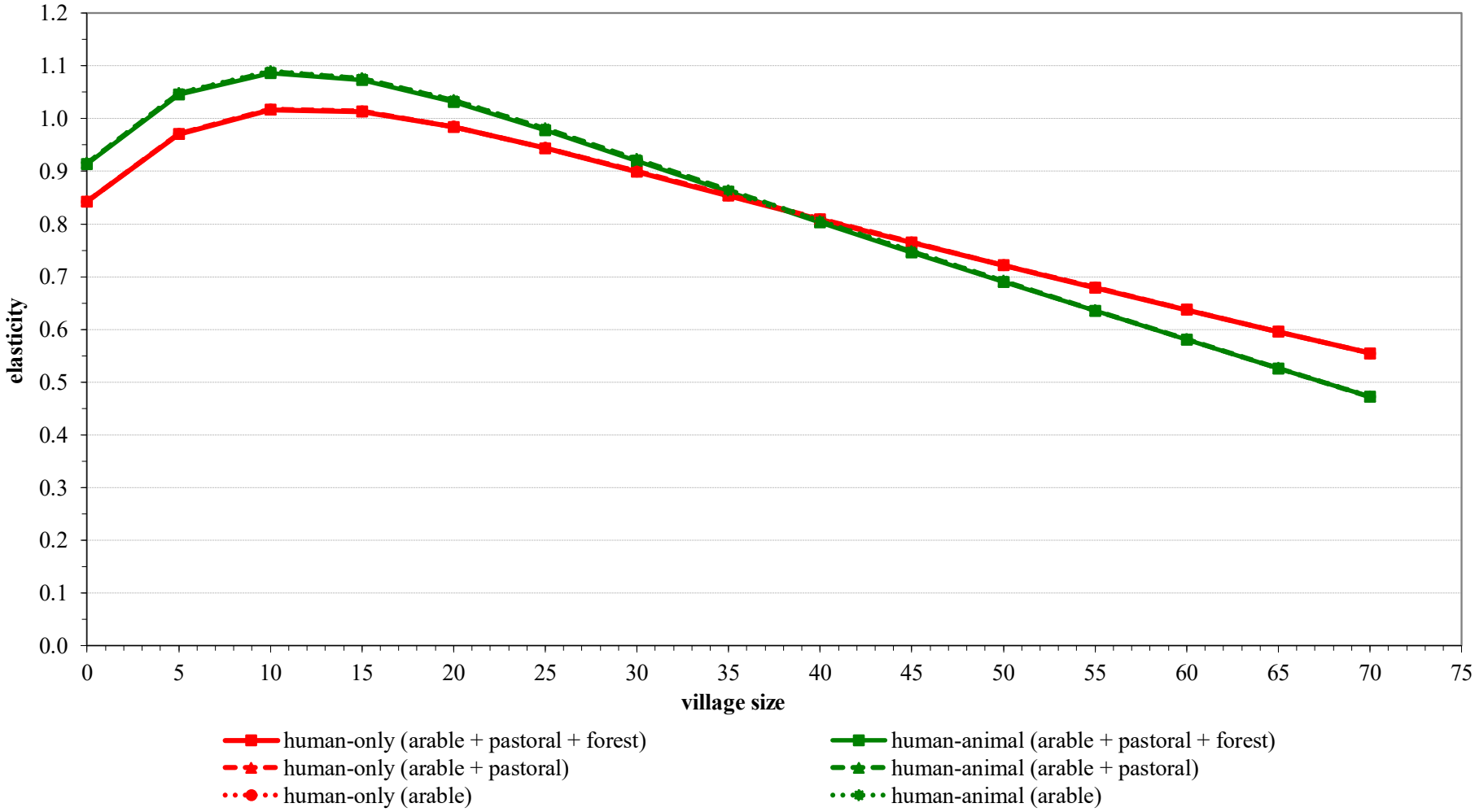


narrower definitions of the land-labor ratio in terms of solely agricultural or solely arable land give rise to slightly higher elasticities of both types of coerced labor with respect to the land-labor ratio in the four-fifths of villages with land-labor ratios below 40-45 *strych* per household. For most villages, defining the land-labor ratio in terms of purely agricultural (i.e. arable and pastoral) land slightly increases the estimated effect of the land-labor ratio on coerced labor, and defining it in terms of purely arable land increases it slightly more, but in both cases the increases are small. Our main result, that the land-labor ratio has a positive effect on coerced labor, which is statistically significant and economically non-trivial across most of the observed range of land-labor ratios, is robust to alternative definitions of the land-labor ratio. If anything, therefore, the results reported in the main text, based on Table 3, are a lower bound on the positive effect of the land-labor ratio on labor coercion for a majority of villages.

The effect of village size on coerced labor is virtually identical across all measures of the land-labor ratio, as is apparent from Figure A2. For all three measures, village size has a positive and statistically significant effect on labor coercion which follows a nearly identical inverted-U shape as village size increases. Our findings on the impact of village size are thus robust to alternative measures of the land-labor ratio.

The already weak effects of the urban potential variables on coerced labor are, if anything, even weaker with alternative measures of the land-labor ratio, as Tables A1 and A2 show. For human-only coerced labor, no towns have any statistically significant effects in the two-part estimation, regardless of how the land-labor ratio is measured. Royal towns have an effect that is statistically significant in the OLS and RE Tobit models, but the estimated sizes of these effects are even more economically insignificant in Tables A1 and A2 than in Table 3. Full towns inside the estate have an effect that is of borderline statistical significance in the RE Tobit model only in Tables 3 and A1, and loses statistical significance altogether in Table A2; in all three cases, the estimated effect is so small as to be of no conceivable economic significance. No other measure of urban potential has a statistically or economically significant effect on human-only coerced labor in Table 3, A1 or A2. Alternative measures of the land-labor ratio thus hardly alter

Figure A2: Elasticity of Coerced Labor with Respect to Village Size,
Controlling for Different Measures of the Land-Labor Ratio, Two-Part Estimates, Bohemia 1757



the effects of towns on human-only coerced labor, but such impact as they do have is to diminish the already minor statistical and economic significance of urban potential.

For human-animal coerced labor, too, alternative measures of the land-labor ratio weaken the already weak urban effects. Three types of town (full towns inside the estate, agro towns inside the estate, and agro towns outside the estate) have no statistically or economically significant effect on human-animal coerced labor under any definition of the land-labor ratio. For royal towns, the two-part marginal effect is of borderline statistical significance in our main specification in Table 4, and of none in Tables A1 and A2. The RE Tobit marginal effect of royal towns is statistically significant in Tables 4 and A1, but only of borderline statistical significance in Table A2; its estimated size is smaller in Table A1 than Table 4, and still smaller in Table A2. The OLS coefficient for royal towns is of borderline statistical significance in Table 4, but not statistically significant in Tables A1 or A2; its estimated size is also smaller under the narrower definitions of the land-labor ratio. For full towns outside the estate, the two-part marginal effect is statistically significant regardless of how the land-labor ratio is measured, but its estimated size is smaller under the narrower definitions. The RE Tobit marginal effect of full towns outside the estate is statistically significant in Table 4, of borderline significance in Table A1, and not significant in Table A2; its estimated size is also smaller under narrower definitions of the land-labor ratio. Adopting more restrictive definitions of the land-labor ratio thus consistently reduces the weak (and in most cases non-existent) effects of towns on human-animal labor coercion.

Exploring alternative measures of the land-labor ratio thus confirms the robustness of the results in our main specification. Defining the land-labor ratio more narrowly, in terms of only agricultural or only arable land, slightly increases the positive effect of the land-labor ratio on both measures of labor coercion, indicating that our estimates of this effect in Tables 3 and 4 is a lower bound. Narrower definitions of the land-labor ratio leave the effect of village size unchanged and further reduce the economic and statistical significance of urban potential, confirming our finding that towns exercise virtually no effect on labor coercion.

Appendix 3:
Fragmented Lordship

As discussed in the main text, almost one-quarter (24.5 percent) of villages in our dataset lie in settlements subject to fragmented lordship. This means that different parts of the settlement or subsets of inhabitants were the serfs of different landlords, who extracted different amounts of coerced labor from them.

To check the robustness of our results with respect to this feature of Bohemian serfdom, we first create a dummy variable for villages in such feudally fragmented settlements. Table A3 reports the results obtained when this dummy variable is added to the regressions.

Simply including a dummy variable for villages under fragmented lordship in this way, which allows the intercept of the regression equation to change, shows that labor coercion is lower in such villages. The coefficient on the fragmented lordship dummy is negative for both types of coerced labor in all regressions in Table A3. It is statistically significantly different from zero at conventional levels in all regressions except for the OLS regression for human-only coerced labor where it is significant only at a borderline level. The effect of fragmented lordship is non-trivial, as shown by the fact that the two-part marginal effect is -1.688 for human-only coerced labor (equivalent to 6.2 percent of the sample mean) and -27.970 for human-animal coerced labor (equivalent to 8.7 percent of the sample mean).

Allowing fragmented lordship to shift the intercept in the regression equation induces almost no change in the effect of the land-labor ratio on labor coercion, as emerges from comparing Table A3 with our main specifications in Tables 3 and 4. As Figure A3 shows, the two-part elasticities of both human-only and human-animal coerced labor with respect to the land-labor ratio are virtually identical whether or not the fragmented lordship dummy is included in the regressions. Likewise, as Figure A4 shows, controlling for fragmented lordship exercises a negligible impact on the effect of village size, with almost identical two-part elasticities across the whole range of values.

Table A3: Regressions Including Dummy for Fragmented Lordship

Variables	Human-Only Labor			Human-Animal Labor		
	Two-part marginal effects	RE Tobit marginal effects	OLS within coefficients	Two-part marginal effects	RE Tobit marginal effects	OLS within coefficients
Land-labor ratio	0.2975	0.2901	0.1418	10.700	8.935	5.405
	(0.0412)	(0.0430)	(0.0193)	(0.748)	(0.802)	(0.537)
	[0.2168 - 0.3783]	[0.2058 - 0.3743]	[0.1040 - 0.1797]	[9.233 - 12.167]	[7.363 - 10.508]	[4.351 - 6.460]
Land-labor ratio squared	-0.0015	-0.0016	-0.0002	-0.0467	-0.0420	-0.0069
	(0.0004)	(0.0004)	(0.0001)	(0.0062)	(0.0072)	(0.0029)
	[-0.0022 - -0.0008]	[-0.0024 - -0.0008]	[-0.0004 - -0.00002]	[-0.0587 - -0.0346]	[-0.0562 - -0.0278]	[-0.0127 - -0.0012]
Village size	2.260	1.918	2.181	28.30	23.44	26.88
	(0.0834)	(0.0822)	(0.0899)	(1.076)	(0.995)	(1.164)
	[2.096 - 2.423]	[1.757 - 2.079]	[2.004 - 2.357]	[26.19 - 30.41]	[21.491 - 25.392]	[24.60 - 29.16]
Village size squared	-0.0088	-0.0079	-0.0087	-0.1281	-0.1094	-0.1221
	(0.0018)	(0.0016)	(0.0014)	(0.0156)	(0.0140)	(0.0141)
	[-0.0122 - -0.0053]	[-0.0110 - -0.0048]	[-0.0114 - -0.0061]	[-0.1588 - -0.0975]	[-0.1368 - -0.0820]	[-0.1497 - -0.0944]
Urban potential of full towns inside estate	0.0046	0.0098	0.0068	0.0039	0.0685	0.0215
	(0.0075)	(0.0063)	(0.0065)	(0.103)	(0.0890)	(0.0971)
	[-0.0101 - 0.0193]	[-0.0026 - 0.0221]	[-0.0061 - 0.0196]	[-0.1972 - 0.2050]	[-0.1059 - 0.2430]	[-0.1690 - 0.2120]
Urban potential of agro-towns inside estate	-0.0231	-0.0204	-0.0328	-0.1625	-0.1388	-0.2798
	(0.0249)	(0.0197)	(0.0260)	(0.2368)	(0.1996)	(0.2444)
	[-0.0720 - 0.0257]	[-0.0590 - 0.0182]	[-0.0839 - 0.0183]	[-0.6266 - 0.3015]	[-0.5300 - 0.2524]	[-0.7594 - 0.1997]
Urban potential of full towns outside estate	0.0087	0.0061	0.0052	0.2187	0.1740	0.1753
	(0.0075)	(0.0078)	(0.0094)	(0.0892)	(0.0941)	(0.1111)
	[-0.0061 - 0.0234]	[-0.0091 - 0.0214]	[-0.0133 - 0.0237]	[0.0440 - 0.3935]	[-0.0104 - 0.3583]	[-0.0427 - 0.3933]
Urban potential of agro-towns outside estate	-0.0141	-0.0074	-0.0108	0.0779	0.1113	0.0973
	(0.0161)	(0.0135)	(0.0162)	(0.2078)	(0.1788)	(0.2032)
	[-0.0457 - 0.0176]	[-0.0339 - 0.0190]	[-0.0427 - 0.0210]	[-0.3294 - 0.4851]	[-0.2392 - 0.4617]	[-0.3015 - 0.4961]
Urban potential of royal towns	0.0083	0.0095	0.0083	0.1148	0.1210	0.0929
	(0.0055)	(0.0044)	(0.0037)	(0.0716)	(0.0579)	(0.0533)
	[-0.0025 - 0.0190]	[0.0009 - 0.0181]	[0.0011 - 0.0154]	[-0.0255 - 0.2551]	[0.0076 - 0.2345]	[-0.0116 - 0.1974]
Fragmented lordship dummy	-1.688	-2.161	-1.072	-27.97	-32.97	-20.91
	(0.574)	(0.491)	(0.583)	(7.143)	(6.085)	(6.945)
	[-2.812 - -0.564]	[-3.124 - -1.199]	[-2.216 - 0.0721]	[-41.97 - -13.97]	[-44.90 - -21.04]	[-34.54 - -7.276]
Latitude	3.399	1.787	2.911	-14.84	-25.93	-25.29
	(2.870)	(2.236)	(2.695)	(31.62)	(29.29)	(30.64)
	[-2.227 - 9.024]	[-2.595 - 6.168]	[-2.379 - 8.201]	[-76.82 - 47.14]	[-83.34 - 32.49]	[-85.43 - 34.85]
Longitude	-4.447	-4.466	-3.178	6.243	-2.592	30.55
	(3.925)	(3.591)	(3.617)	(61.15)	(54.31)	(55.72)
	[-12.14 - 3.246]	[-11.50 - 2.571]	[-10.28 - 3.920]	[-113.6 - 126.1]	[-109.04 - 103.86]	[-78.81 - 139.9]
Region dummy variables	YES	YES	NO	YES	YES	NO
Lordship type dummy variables	YES	YES	NO	YES	YES	NO
Estate-level mean values of regressors	YES	YES	NO	YES	YES	NO
R squared			0.399			0.394
Proportion correctly classified in first part	0.868			0.868		
R squared in second part	0.431			0.448		
Number of observations	11,342	11,349	10,886	11,342	11,349	10,886
Number of estates		1,357	894		1,357	894
Number of estates (first part)	1,355			1,355		
Number of estates (second part)	1,057			1,057		

Notes: Standard errors are in parentheses, confidence intervals in brackets. For two-part and OLS within, standard errors are clustered at the estate level. For RE Tobit, standard errors are bootstrapped with resampling over estates. The two-part regression has only 11,342 observations because when it was estimated, the seven observations for one particular lordship type were dropped since these observations perfectly predicted zero coerced labor in the first-stage logit regression. The Tobit regression was estimated on 11,349 observations but marginal effects could only be calculated using 11,348 observations because for one observation a predicted value of the dependent variable required to calculate the marginal effects was missing. The OLS regression has just 10,886 observations because estates with only a single village cannot be included in the within transformation.

Figure A3: Elasticity of Coerced Labor with Respect to the Land-Labor Ratio, With and Without Fragmented Lordship Dummy, Two-Part Estimates, Bohemia 1757

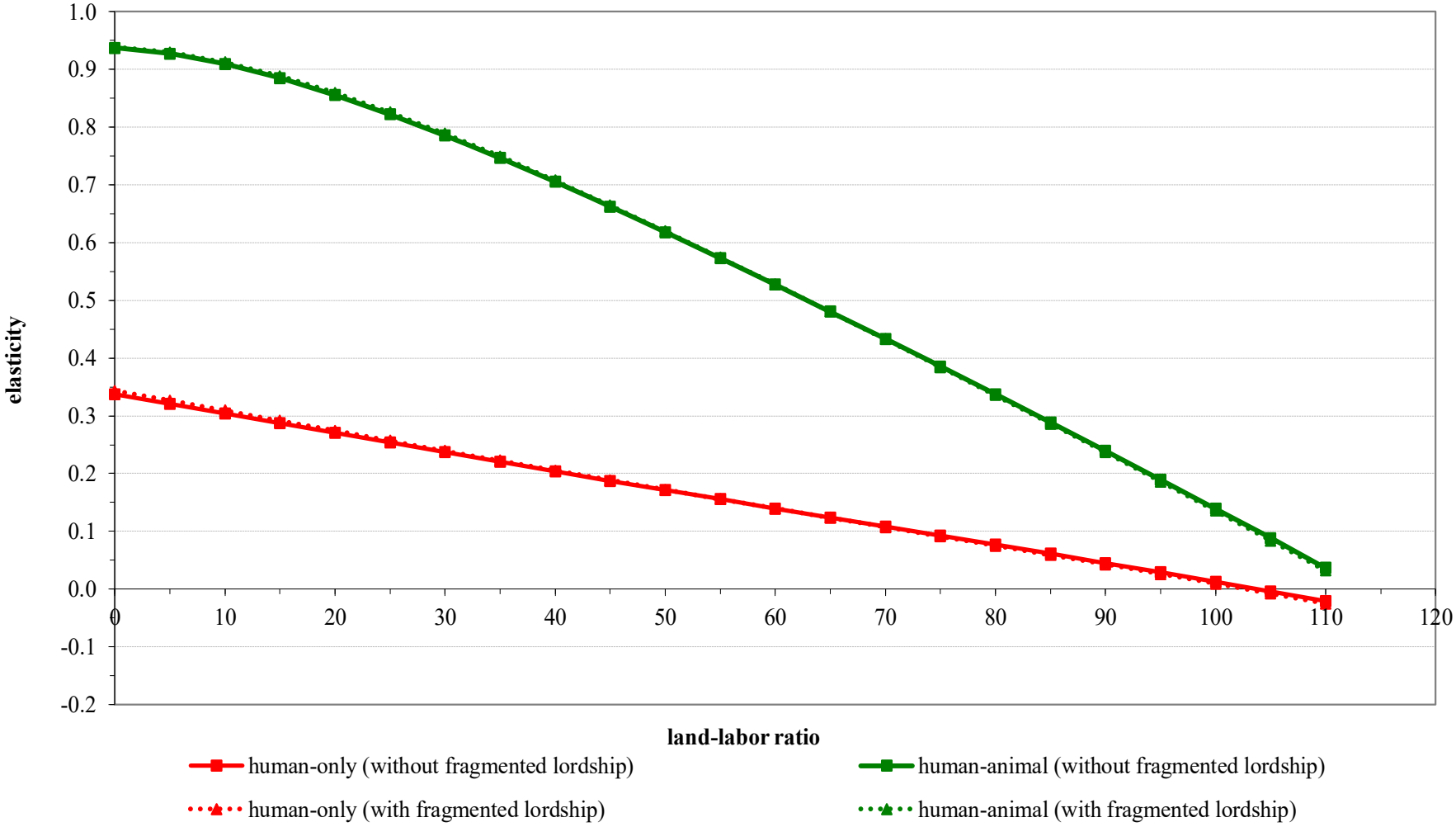
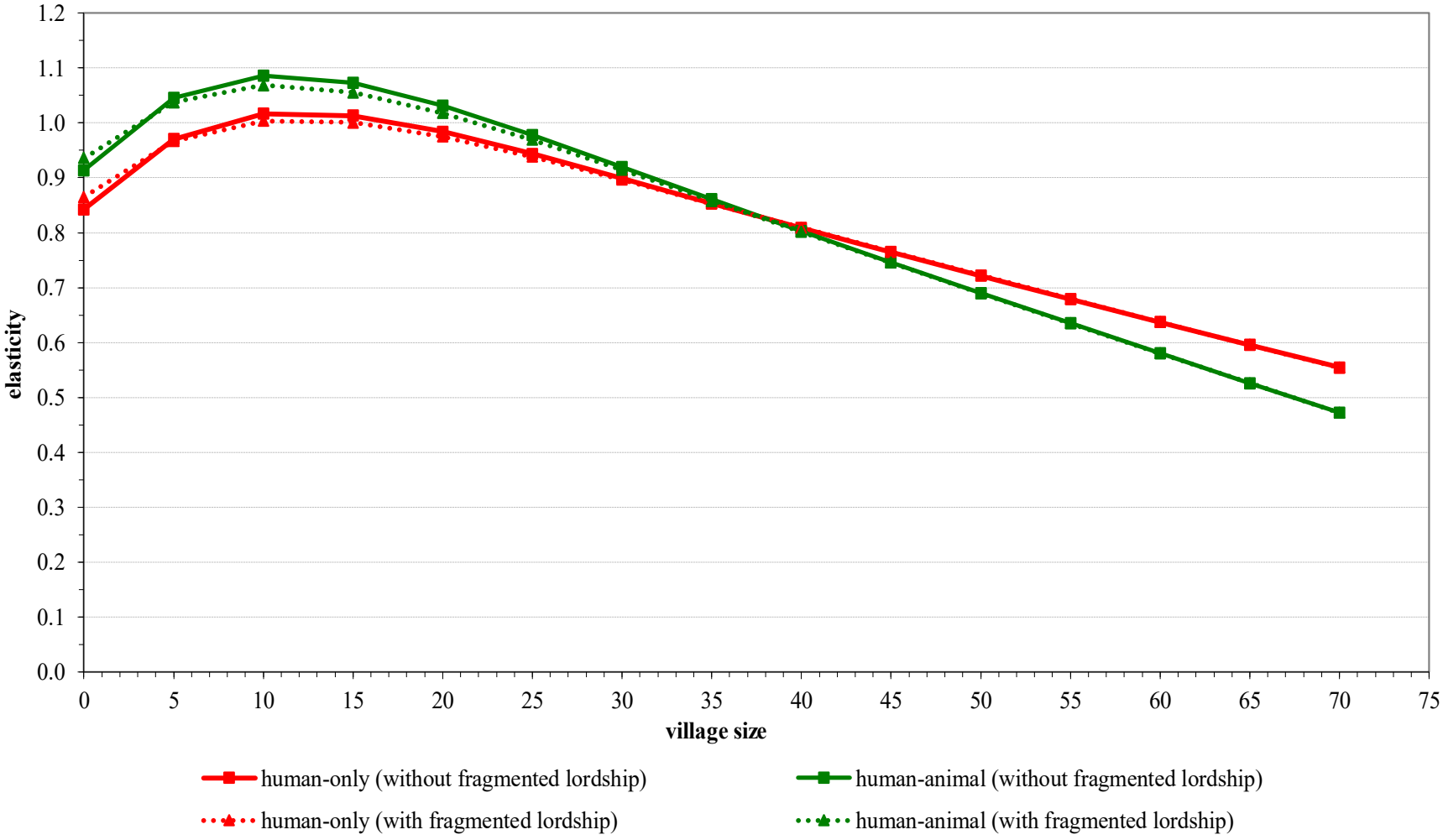


Figure A4: Elasticity of Coerced Labor with Respect to Village Size,
With and Without Fragmented Lordship Dummy, Two-Part Estimates, Bohemia 1757



Allowing fragmented lordship to shift the intercept exercises very slightly more impact on the effect of towns on coerced labor – mainly to weaken it. For human-only coerced labor, the two-part estimation shows no statistically significant effect of any type of town, whether or not fragmented lordship is controlled for. The RE Tobit marginal effect of full towns inside the estate is statistically significant at a borderline level in our main specification in Table 3, but is no longer significant in Table A3. We already noted that the effect of this variable is not statistically significant in Table A2, suggesting that little weight should be placed on its borderline statistical significance in Tables 3 and A1. For human-animal coerced labor, full towns outside the estate have statistically significant effects in the two-part regressions in both Table 4 and Table A3. But in the RE Tobit regression, the coefficient on this variable that is statistically significant at conventional levels in Table 4 is significant only at a borderline level in Table A3; it is also of borderline statistical significance in Table A1 and not statistically significant in Table A2, so its statistical significance in our main specification in Table 3 is not robust to these alternative specifications. All other urban effects on both types of coerced labor are nearly identical between Table 3 and Table A3; in all cases they are not economically significant.

To test whether fragmented lordship affected the coefficients of the regressors as well as the intercept of the regression equation, we estimate all regressions excluding villages in settlements under fragmented lordship. The results are reported in Table A4. Figures A5-A9 graph the elasticities implied by the results in Table A4 alongside those implied by our main specifications in Tables 3 and 4.

The elasticities of coerced labor with respect to the land-labor ratio are hardly affected by dropping the villages under fragmented lordship in the two-part and RE Tobit regressions, as Figures A5 and A6 show. The only difference for these two non-linear models is that the curve excluding villages under fragmented lordship slopes downwards slightly more steeply as the land-labor ratio increases than does the curve including such villages. The inverted-U shape of the RE Tobit elasticities curve in Figure A6 appears both with and without the villages under fragmented lordship and is thus robust to the

Table A4: Regressions Excluding Villages in Settlements under Fragmented Lordship

Variables	Human-Only Labor			Human-Animal Labor		
	Two-part marginal effects	RE Tobit marginal effects	OLS within coefficients	Two-part marginal effects	RE Tobit marginal effects	OLS within coefficients
Land-labor ratio	0.3738	0.3888	0.3129	13.238	11.799	10.896
	(0.0465)	(0.0472)	(0.0345)	(0.805)	(0.918)	(0.773)
	[0.2826 - 0.4651]	[0.2962 - 0.4813]	[0.2452 - 0.3806]	[11.660 - 14.816]	[9.998 - 13.599]	[9.377 - 12.414]
Land-labor ratio squared	-0.0020	-0.0023	-0.0014	-0.0638	-0.0606	-0.0433
	(0.0004)	(0.0005)	(0.0002)	(0.0064)	(0.0089)	(0.0056)
	[-0.0028 - -0.0013]	[-0.0032 --0.0014]	[-0.0019 - -0.0009]	[-0.0763 - -0.0512]	[-0.0781 - -0.0430]	[-0.0543 - -0.0324]
Village size	2.261	2.009	2.215	28.44	24.59	27.04
	(0.0959)	(0.1019)	(0.1020)	(1.219)	(1.195)	(1.276)
	[2.073 - 2.449]	[1.810 - 2.209]	[2.015 - 2.415]	[26.05 - 30.83]	[2.250 - 26.935]	[24.53 - 29.54]
Village size squared	-0.0076	-0.0078	-0.0085	-0.1166	-0.1076	-0.1160
	(0.0025)	(0.0019)	(0.0016)	(0.0212)	(0.0180)	(0.0160)
	[-0.0124 - -0.0028]	[-0.0115 - -0.0040]	[-0.0116 - -0.0053]	[-0.1583 - -0.0750]	[-0.1430 - -0.0723]	[-0.1475 - -0.0845]
Urban potential of full towns inside estate	0.0024	0.0096	0.0056	-0.0301	0.0600	-0.0051
	(0.0071)	(0.0076)	(0.0065)	(0.0991)	(0.0971)	(0.0952)
	[-0.0115 - 0.0164]	[-0.0053 - 0.0244]	[-0.0071 - 0.0182]	[-0.2244 - 0.1641]	[-0.1304 - 0.2503]	[-0.1920 - 0.1819]
Urban potential of agro-towns inside estate	-0.0235	-0.0222	-0.0346	-0.1139	-0.1109	-0.2281
	(0.0271)	(0.0229)	(0.0287)	(0.2586)	(0.2279)	(0.2606)
	[-0.0767 - 0.0296]	[-0.0670 - 0.0226]	[-0.0909 - 0.0216]	[-0.6207 - 0.3929]	[-0.5575 - 0.3358]	[-0.7396 - 0.2834]
Urban potential of full towns outside estate	0.0068	0.0045	0.0033	0.2076	0.1659	0.1695
	(0.0081)	(0.0089)	(0.0098)	(0.0984)	(0.1042)	(0.1141)
	[-0.0090 - 0.0226]	[-0.0129 - 0.0219]	[-0.0159 - 0.0225]	[0.0148 - 0.4004]	[-0.0383 - 0.3701]	[-0.0545 - 0.3935]
Urban potential of agro-towns outside estate	-0.0180	-0.0114	-0.0163	0.0801	0.1148	0.0829
	(0.0197)	(0.0169)	(0.0209)	(0.2527)	(0.2315)	(0.2594)
	[-0.0567 - 0.0206]	[-0.0445 - 0.0218]	[-0.0572 - 0.0247]	[-0.4152 - 0.5753]	[-0.3390 - 0.5686]	[-0.4263 - 0.5921]
Urban potential of royal towns	0.0107	0.0116	0.0106	0.1055	0.1183	0.0898
	(0.0070)	(0.0059)	(0.0054)	(0.0898)	(0.0715)	(0.0698)
	[-0.0029 - 0.0244]	[0.0001 - 0.0230]	[-0.0001 - 0.0212]	[-0.0704 - 0.2815]	[-0.0219 - 0.2585]	[-0.0472 - 0.2267]
Latitude	3.555	1.581	2.635	-2.982	-19.28	-15.94
	(3.699)	(3.080)	(3.734)	(34.78)	(34.72)	[37.10]
	[-3.696 - 10.80]	[-4.456 - 7.617]	[-4.696 - 9.965]	[-71.15 - 65.18]	[-87.34 - 48.78]	[-88.76 - 56.89]
Longitude	-5.460	-4.981	-4.235	-30.99	-25.97	-7.983
	(4.332)	(4.162)	(4.317)	(68.17)	(66.21)	[66.47]
	[-13.95 - 3.031]	[-13.137 - 3.176]	[-12.71 - 4.240]	[-164.6 - 102.6]	[-155.74 - 103.81]	[-138.5 - 122.5]
Region dummy variables	YES	YES	NO	YES	YES	NO
Lordship type dummy variables	YES	YES	NO	YES	YES	NO
Estate-level mean values of regressors	YES	YES	NO	YES	YES	NO
R squared			0.385			0.406
Proportion correctly classified in first part	0.893			0.893		
R squared in second part	0.412			0.442		
Number of observations	8,560	8,560	8,259	8,560	8,560	8,259
Number of estates		1,075	772		1,075	772
Number of estates (first part)	1,075			1,075		
Number of estates (second part)	976			976		

Notes: Standard errors are in parentheses, confidence intervals in brackets. For two-part and OLS within, standard errors are clustered at the estate level. For RE Tobit, standard errors are bootstrapped with resampling over estates. The OLS regression has just 8,259 observations because estates with only a single village cannot be included in the within transformation.

change in sample size. For the OLS regressions, the two curves follow a similar downward trajectory, but the curve excluding villages under fragmented lordship starts at a higher elasticity and falls more steeply. For the reasons discussed in the main text, the two-part model is our preferred one; the similarity of the two-part elasticity curves with and without the villages under fragmented lordship confirms that the results of our basic specification are robust to the fragmented lordship feature of Bohemian serfdom.

Dropping the villages under fragmented lordship has little impact on the effects of other regression variables. As Figures A7 and A8 show, the elasticities of coerced labor with respect to village size are virtually identical regardless of whether the regressions include or exclude the villages under fragmented lordship. Excluding those villages also has virtually no impact on the urban variables; if anything, it weakens their already feeble statistical and economic significance.

These results offer further reassurance that the results in our main specification are unaffected by the fact that one-quarter of our villages are located in settlements under fragmented lordship.

Figure A5: Human-Only Coerced Labor, Elasticitiy with Respect to Land-Labor Ratio, With and Without Villages Under Fragmented Lordship, Bohemia 1757

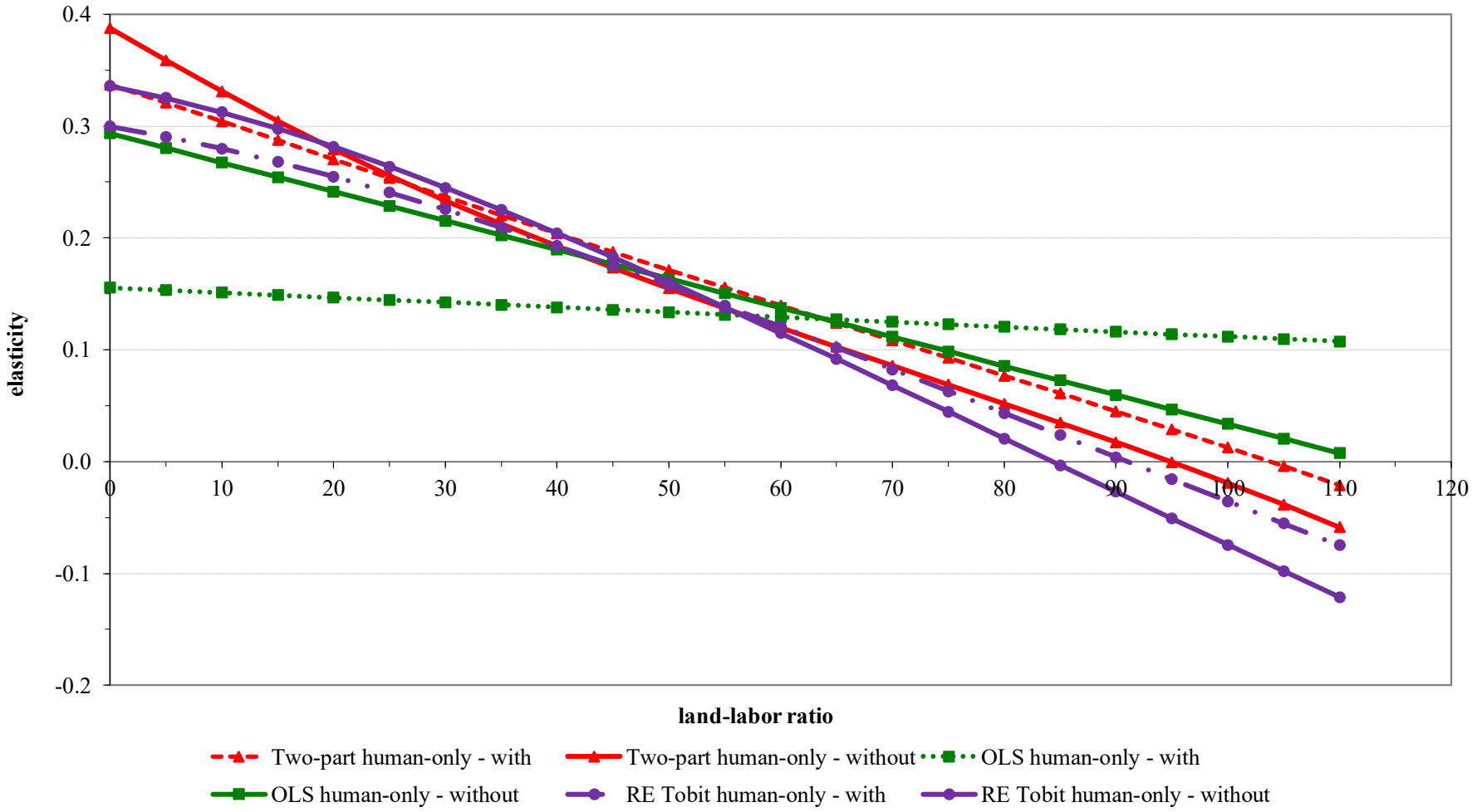


Figure A6: Human-Animal Coerced Labor, Elasticity with Respect to Land-Labor Ratio, With and Without Villages Under Fragmented Lordship, Bohemia 1757

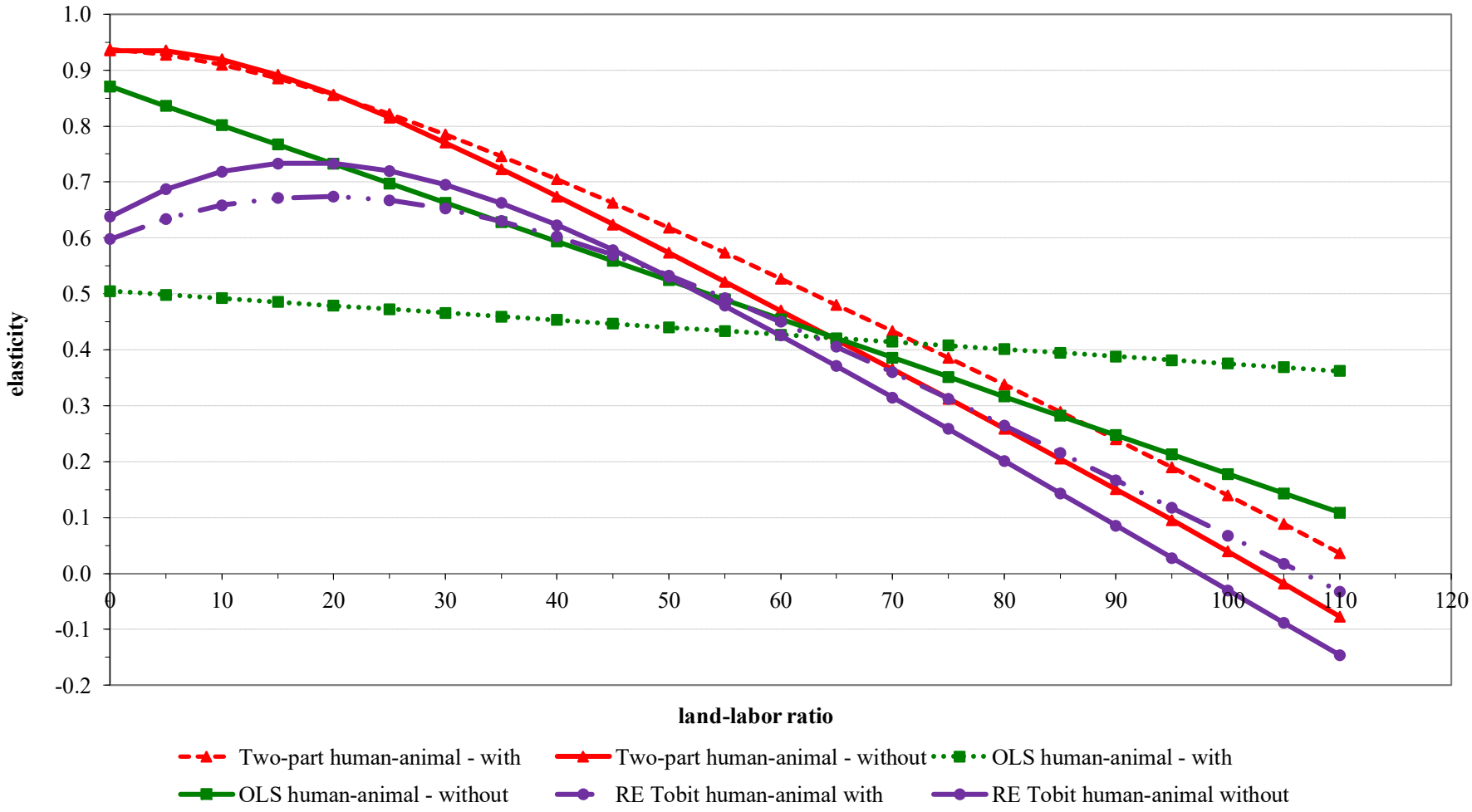


Figure A7: Human-Only Coerced Labor, Elasticity with Respect to Village Size, With and Without Villages Under Fragmented Lordship, Bohemia 1757

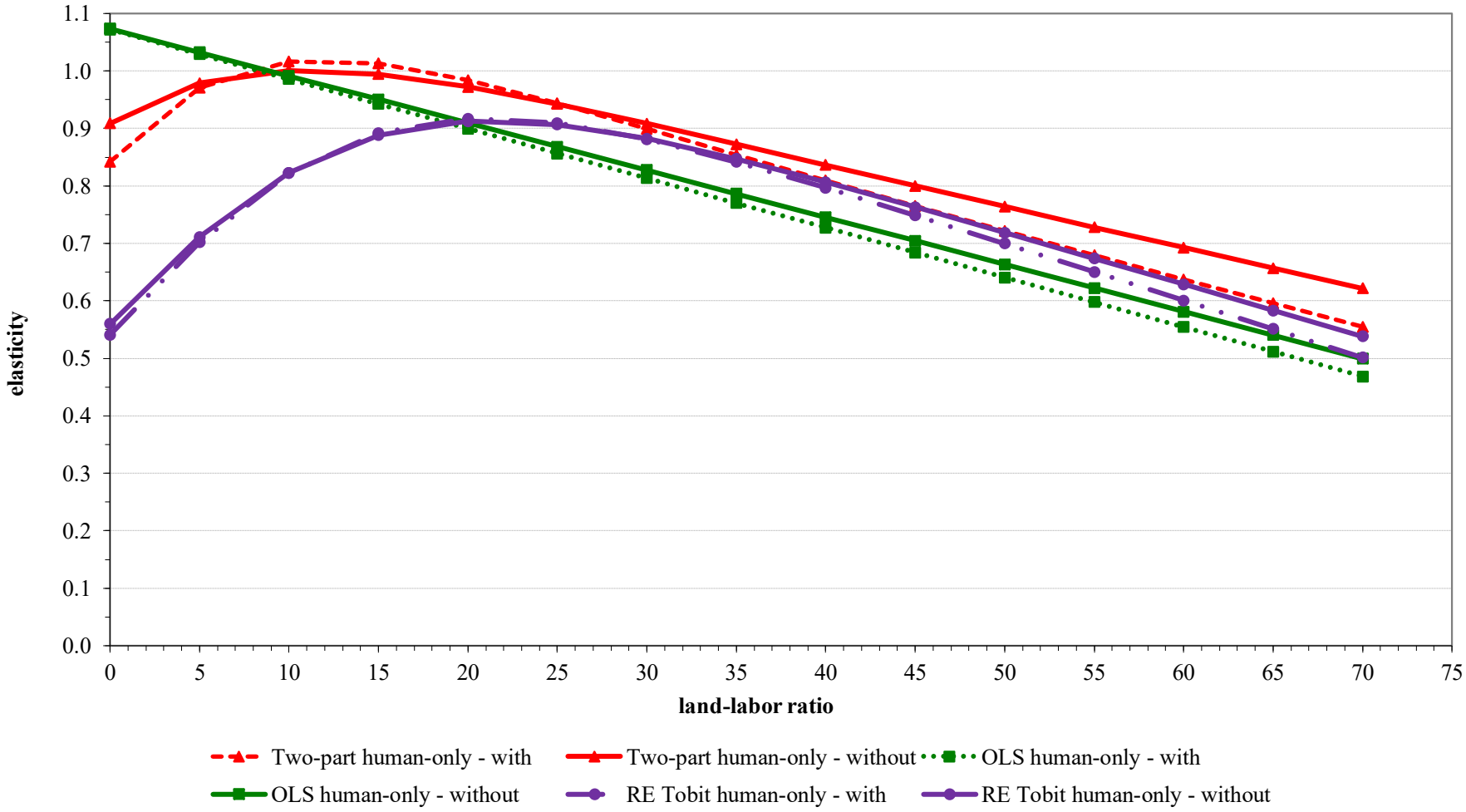
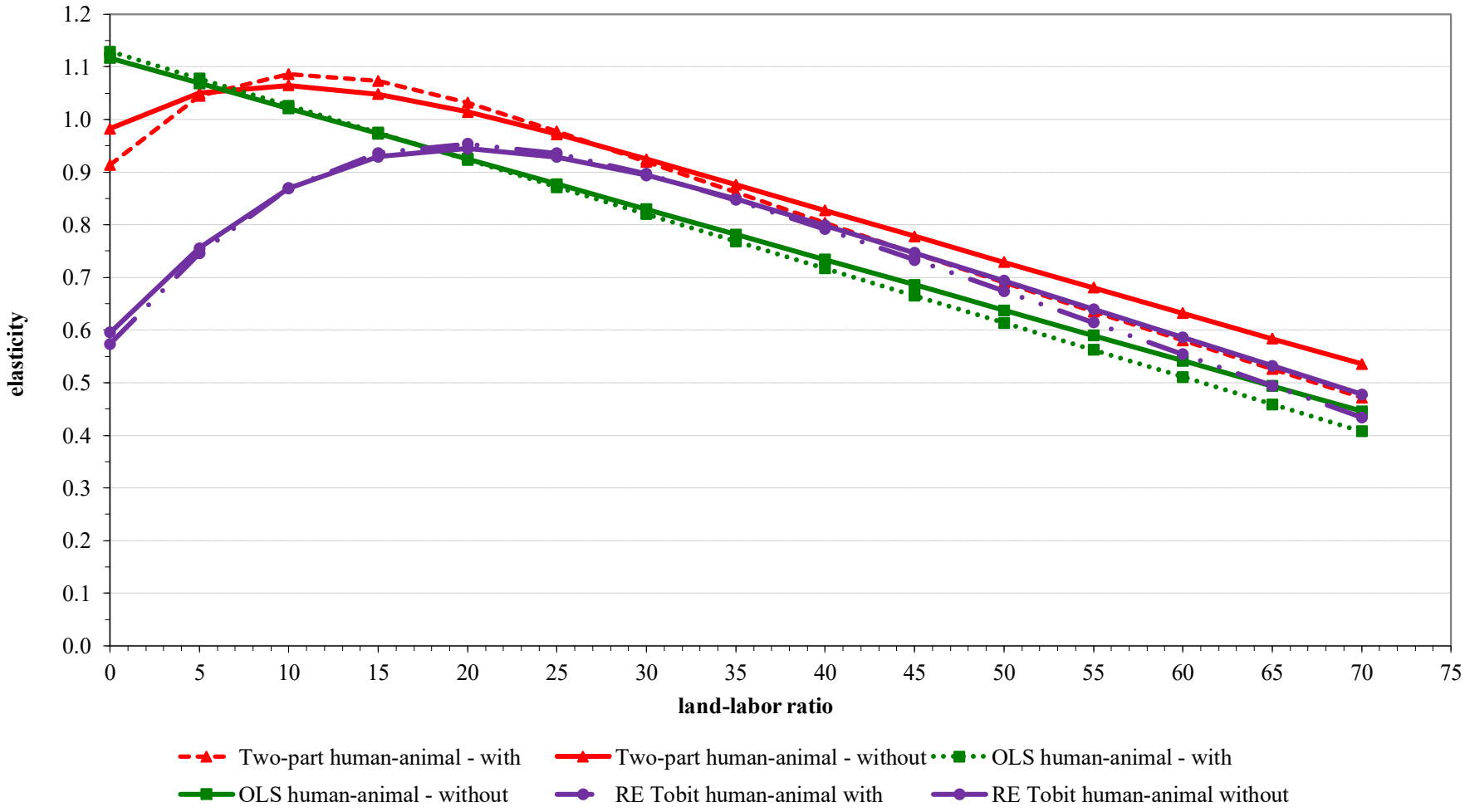


Figure A8: Human-Animal Coerced Labor, Elasticity with Respect to Village Size, With and Without Villages Under Fragmented Lordship, Bohemia 1757



Appendix 4:
Estimating the Two-Part and Tobit Regressions on the OLS Sample

The sample of villages that can be analyzed differs between the OLS regressions (which use only 10,886 observations) and the non-linear (two-part and RE Tobit) regressions (which use 11,342 and 11,349 observations respectively). The difference in sample size arises from the fact that the within transformation used in the OLS regressions cannot be applied to estates containing just a single village, resulting in the loss of 463 villages in single-village estates from the total of 11,349 villages whose coerced labor is recorded. When the two-part model is estimated, the 7 observations for one particular lordship type are dropped because these observations perfectly predict zero coerced labour in the first-stage logit regression, with the result that 11,342 observations are used in the two-part model.

To check for the effect of the difference in sample size on the difference between the non-linear and the OLS analyses, we estimate the two-part and the RE Tobit regressions on the sample of 10,886 observations used for the OLS regressions. Table A5 reports the results, and Figures A9 and A10 graph the elasticities with regard to the land-labor ratio and village size respectively.

When the two-part and RE Tobit regressions are estimated on the smaller OLS sample, they generate estimates of the elasticity of labor coercion with respect to the land-labor ratio which are even more different from the OLS estimates than when the non-linear regressions are estimated on the larger sample. As can be seen by comparing Figure 1 with Figure A9, this applies both to human-only and to human-animal coerced labor. That is, the difference between the non-linear and the OLS estimates is accentuated when the non-linear models are estimated on the OLS sample. Figure A9 also shows that the elasticities of human-animal coerced labor with the land-labor ratio follow an inverted-U shape according to the RE Tobit analyses on both the larger and the smaller sample, confirming that the shape of this curve is not caused by the difference in the samples analyzed.

For the other regression variables, by contrast, there is virtually no difference between the two-part and RE Tobit estimates generated on the full sample and those generated on the OLS sample. Analyzing the smaller sample does not accentuate the gap between the non-linear and the OLS estimates of the elasticity of coerced labor with respect to village size, as Figure A10 shows. Similarly, estimating the two-part and RE Tobit models on the smaller sample does not give rise to any differences worth noting in the estimates of any of the urban effects on coerced labor, as can be seen by comparing Table A5 with the main specification in Table 3. Most of the already feeble urban effects are even weaker when the non-linear models are estimated on the smaller OLS sample.

Table A5: The Two-Part and Tobit Regressions Estimated on the OLS Sample

Variables	Human-Only Labor			Human-Animal Labor		
	Two-part marginal effects	RE Tobit marginal effects	OLS within coefficients	Two-part marginal effects	RE Tobit marginal effects	OLS within coefficients
Land-labor ratio	0.3645	0.3631	0.1415	11.984	10.493	5.398
	(0.0395)	(0.0403)	(0.0193)	(0.738)	(0.772)	(0.536)
	[0.2871 - 0.4420]	[0.2841 - 0.4422]	[0.1037 - 0.1793]	[10.538 - 13.430]	[8.981 - 12.006]	[4.347 - 6.450]
Land-labor ratio squared	-0.0022	-0.0023	-0.0002	-0.0606	-0.0565	-0.0069
	(0.0003)	(0.0004)	(0.0001)	(0.0062)	(0.0070)	(0.0029)
	[-0.0029 - -0.0016]	[-0.0031 - -0.0016]	[-0.0004 - -0.00002]	[-0.0727 - -0.0484]	[-0.0702 - -0.0428]	[-0.0127 - -0.0012]
Village size	2.315	2.003	2.195	29.05	24.85	27.16
	(0.0825)	(0.0900)	(0.0871)	(1.075)	(1.094)	(1.148)
	[2.154 - 2.477]	[1.827 - 2.180]	[2.024 - 2.366]	[26.95 - 31.16]	[22.71 - 26.10]	[24.91 - 29.41]
Village size squared	-0.0090	-0.0084	-0.0088	-0.1321	-0.1191	-0.1240
	(0.0018)	(0.0016)	(0.0014)	(0.0159)	(0.0150)	(0.0142)
	[-0.0126 - -0.0055]	[-0.0115 - -0.0053]	[-0.0115 - -0.0061]	[-0.1632 - -0.1010]	[-0.1485 - -0.0897]	[-0.1519 - -0.0961]
Urban potential of full towns inside estate	0.0049	0.0108	0.0072	0.0099	0.0823	0.0306
	(0.0075)	(0.0069)	(0.0065)	(0.1022)	(0.1077)	(0.0973)
	[-0.0097 - 0.0196]	[-0.0028 - 0.0243]	[-0.0055 - 0.0200]	[-0.1903 - 0.2102]	[-0.1288 - 0.2933]	[-0.1603 - 0.2215]
Urban potential of agro-towns inside estate	-0.0199	-0.0167	-0.031	-0.1015	-0.0751	-0.2440
	(0.0254)	(0.0224)	(0.0265)	(0.2384)	(0.2074)	(0.2464)
	[-0.0697 - 0.0300]	[-0.0606 - 0.0272]	[-0.0831 - 0.0211]	[-0.5687 - 0.3656]	[-0.4816 - 0.3314]	[-0.7277 - 0.2396]
Urban potential of full towns outside estate	0.0084	0.0064	0.0053	0.2162	0.1786	0.1782
	(0.0075)	(0.0086)	(0.0094)	(0.0886)	(0.0874)	(0.1117)
	[-0.0062 - 0.0230]	[-0.0105 - 0.0233]	[-0.0132 - 0.0239]	[0.0426 - 0.3898]	[0.0074 - 0.3499]	[-0.0410 - 0.3973]
Urban potential of agro-towns outside estate	-0.0151	-0.0080	-0.0108	0.0727	0.1105	0.0982
	(0.0163)	(0.0136)	(0.0162)	(0.2089)	(0.1878)	(0.2033)
	[-0.0470 - 0.0169]	[-0.0347 - 0.0187]	[-0.0426 - 0.0210]	[-0.3367 - 0.4820]	[-0.2576 - 0.4786]	[-0.3008 - 0.4972]
Urban potential of royal towns	0.0086	0.0100	0.0083	0.1195	0.1293	0.0945
	(0.0055)	(0.0048)	(0.0037)	(0.0720)	(0.0550)	(0.0530)
	[-0.0023 - 0.0194]	[0.0006 - 0.0193]	[0.0012 - 0.0155]	[-0.0216 - 0.2605]	[0.0215 - 0.2372]	[-0.0096 - 0.1986]
Latitude	3.599	1.818	2.904	-13.37	-27.03	-25.43
	(2.890)	(2.186)	(2.686)	(31.57)	(27.70)	(30.54)
	[-2.064 - 9.263]	[-2.266 - 6.102]	[-2.368 - 8.176]	[-75.24 - 48.51]	[-81.32 - 27.27]	[-85.36 - 34.50]
Longitude	-4.146	-4.425	-3.158	11.52	0.180	30.93
	(3.910)	(3.397)	(3.605)	(61.93)	(52.31)	(55.76)
	[-11.809 - 3.516]	[-11.084 - 2.234]	[-10.23 - 3.918]	[-109.9 - 132.9]	[-102.34 - 102.70]	[-78.50 - 140.37]
Region dummy variables	YES	YES	NO	YES	YES	NO
Lordship type dummy variables	YES	YES	NO	YES	YES	NO
Estate-level mean values of regressors	YES	YES	NO	YES	YES	NO
R squared			0.399			0.393
Proportion correctly classified in first part	0.873			0.873		
R squared in second part	0.438			0.462		
Number of observations	10,840	10,886	10,886	10,840	10,886	10,886
Number of estates		894	894		894	894
Number of estates (first part)	886			886		
Number of estates (second part)	851			851		

Notes: Standard errors are in parentheses, confidence intervals in brackets. For two-part and OLS within, standard errors are clustered at the estate level. For RE Tobit, standard errors are bootstrapped with resampling over estates. The two-part regression has only 10,840 observations because when it was estimated, 46 observations for several lordship types were dropped since they perfectly predicted the outcome in the first-stage logit regression.

Figure A9: Elasticity of Coerced Labor with Respect to the Land-Labor Ratio for the OLS Sample, Bohemia 1757

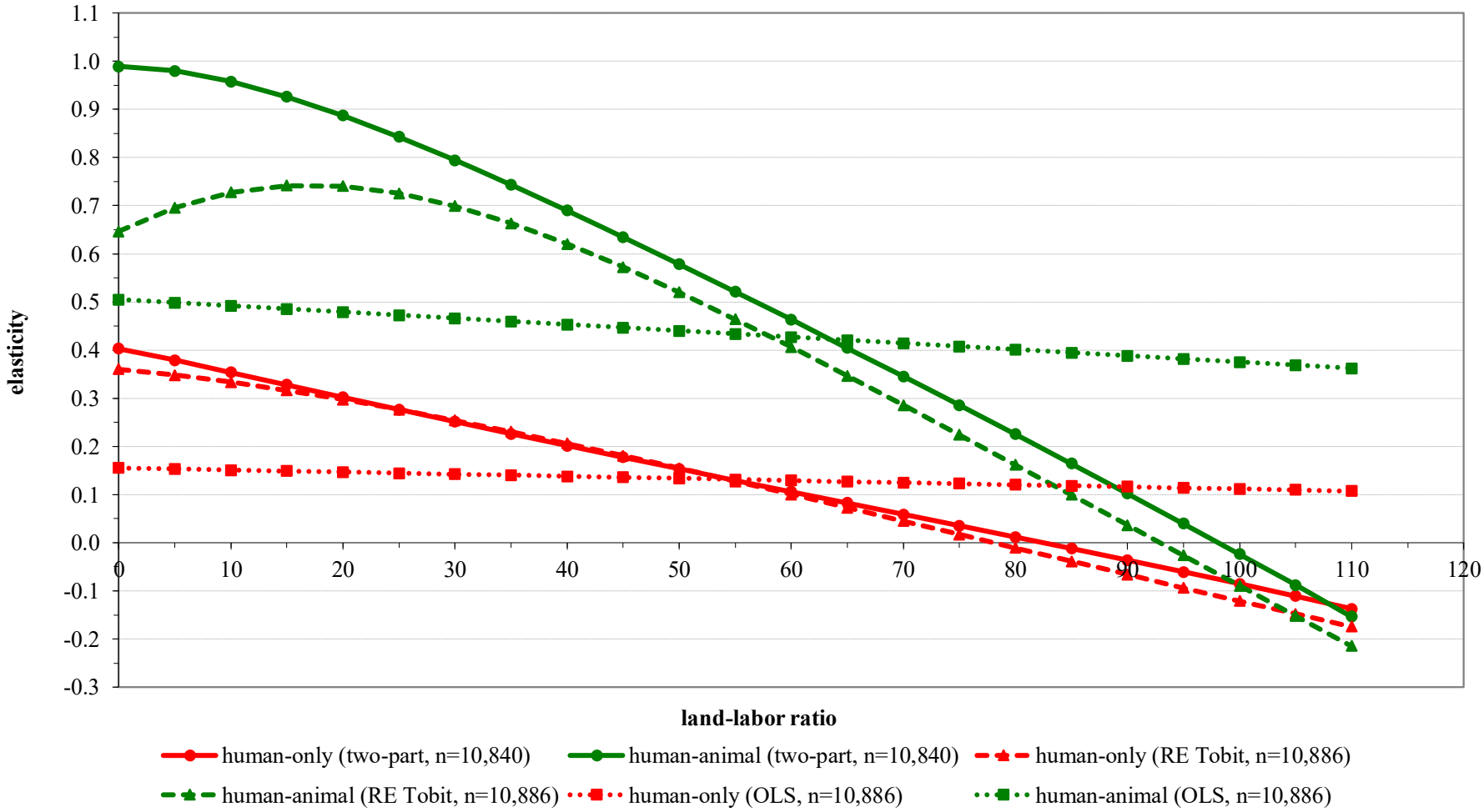
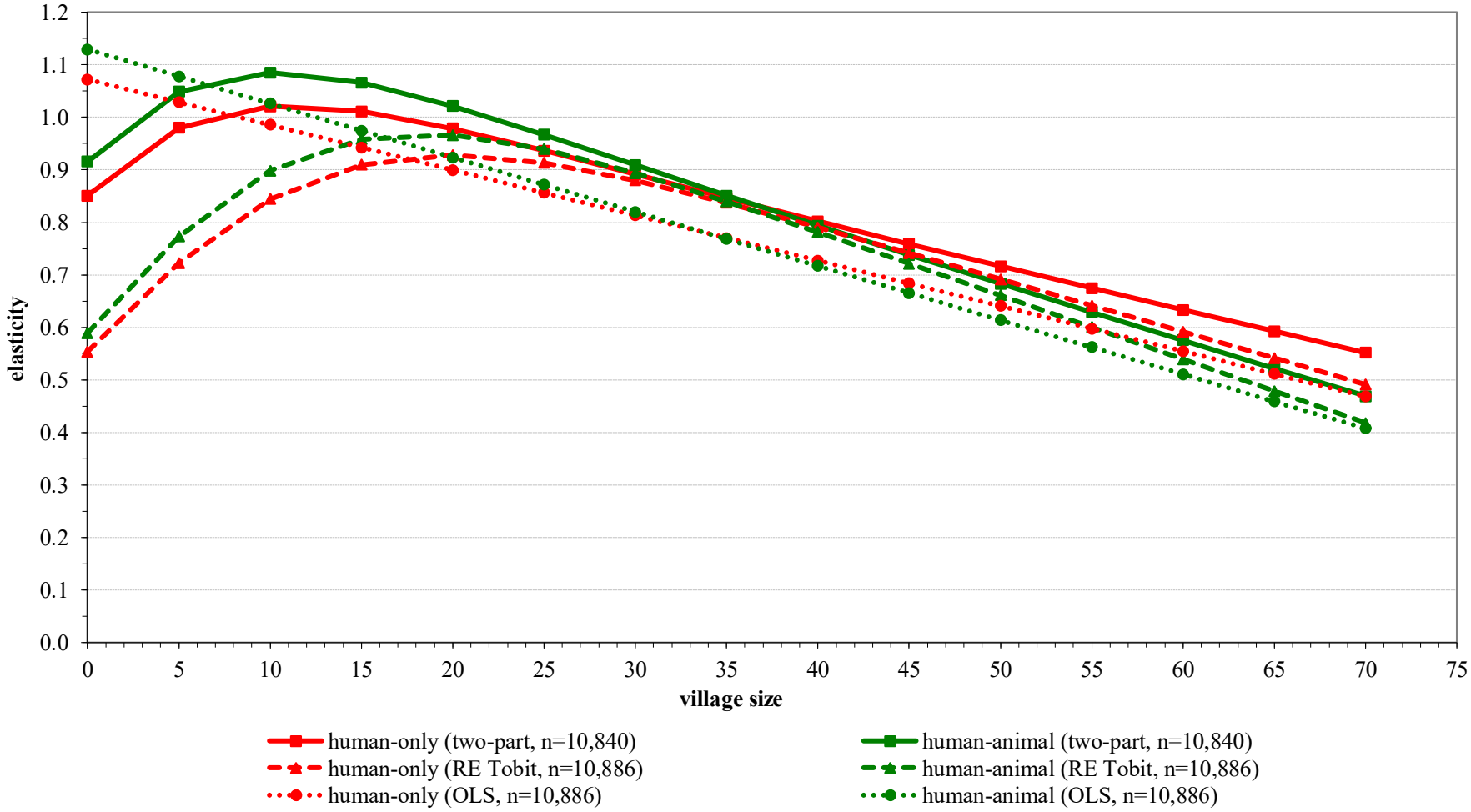


Figure A10: Elasticity of Coerced Labor with Respect to Village Size for the OLS Sample, Bohemia 1757



Appendix 5:
Interactions between the Land-Labor Ratio and Urban Potential

As discussed in the main text, our initial regressions included interaction terms between the land-labor ratio and the urban potential variables, in order to explore whether the impact of urban potential on labor coercion depends on the land-labor ratio in the village or the impact of the land-labor ratio in the village depends on the urban potential available to serfs in that village.

For human-only coerced labor, none of the coefficients on the interaction terms is significantly different from zero at conventional levels. For human-animal coerced labor, however, two of the interaction terms are significantly different from zero in the two-part regressions and one in the RE Tobit regressions.

Table A6 reports these results alongside the regressions without the interaction terms from our main specification in Table 4. In the two-part regression (but not the RE Tobit one), the interaction term with full towns outside the estate is statistically significant; in both the two-part and the RE Tobit regressions, the interaction term with royal towns is statistically significant. However, in all cases the size of the marginal effects is very small. Furthermore, the inclusion of the interaction terms has virtually no impact on the elasticities of human-animal coerced labor with respect to the land-labor ratio in Figure A11 or with respect to village size in Figure A12. Even the interaction terms that are statistically significant, therefore, are not economically significant. Including the interaction terms does not give rise to any differences worth noting in the elasticity of coerced labor with respect to urban potential, except for the case of full towns outside the estate, where it increases the two-part elasticity from 0.2259 to 0.2825 and the RE Tobit elasticity from 0.1917 to 0.2216. As already noted, the effect of this variable is always very sensitive to alternative specifications, so it is not surprising that it is also sensitive to inclusion of the interaction terms. In all other cases, alternative specifications weaken the statistical and economic significance of this urban variable, and this is the only specification which strengthens it slightly.

Overall, the effects of the interaction terms are very small and their inclusion exercises virtually no impact on the estimated effects of the other regression variables. The results in our main specification in Table 4 are thus robust to non-inclusion of the interaction terms.

Table A6: Regressions for Human-Animal Coerced Labor
With and Without Interaction Terms between the Land-Labor Ratio and Urban Potential

Variables	Two-part marginal effects		RE tobit marginal effects	
	Without interaction terms	With interaction terms	Without interaction terms	With interaction terms
Land-labor ratio	10.633	5.941	8.788	6.248
	(0.761)	(1.917)	(0.817)	(1.773)
	[9.141 - 12.125]	[2.184 - 9.698]	[7.187 -10.389]	[2.773 - 9.723]
Land-labor ratio squared	-0.0462	-0.0493	-0.0412	-0.0446
	(0.0064)	(0.0061)	(0.0074)	(0.0075)
	[-0.0587 - -0.0336]	[-0.0613 - -0.0373]	[-0.0556 - -0.0267]	[-0.0592 - -0.0300]
Village size	28.82	28.72	23.66	23.69
	(1.185)	(1.189)	(1.009)	(1.043)
	[26.50 - 31.15]	[26.39 - 31.05]	[21.69 - 25.64]	[21.65 - 25.74]
Village size squared	-0.1313	(0.1301)	-0.1115	-0.1113
	(0.0177)	(0.0177)	(0.0137)	(0.0138)
	[-0.1660 - -0.0967]	[-0.1648 - -0.0954]	[-0.1383 - 0.0847]	[-0.1383 - -0.0842]
Urban potential of full towns inside estate	0.0192	0.0772	0.0811	0.1446
	(0.1035)	(0.1968)	(0.0835)	(0.1635)
	[-0.1836 - 0.2221]	[-0.3084 - 0.4629]	[-0.0825 - 0.2447]	[-0.1759 - 0.4650]
Urban potential of full towns inside estate * Land-labor ratio		-0.0020		-0.0029
		(0.0064)		(0.0061)
		[-0.0145 - 0.0106]		[-0.0148 - 0.0091]
Urban potential of agro-towns inside estate	-0.1020	-0.1249	-0.0811	-0.0812
	(0.2390)	(0.3270)	(0.1839)	(0.2639)
	[-0.5704 - 0.3664]	[-0.7657 - 0.5159]	[-0.4414 - 0.2793]	[-0.5984 - 0.4360]
Urban potential of agro-towns inside estate * Land-labor ratio		0.0023		0.0006
		(0.0068)		(0.0066)
		[-0.0110 - 0.0157]		[-0.0124 - 0.0136]
Urban potential of full towns outside estate	0.2229	-0.0240	0.1779	0.0341
	(0.0893)	(0.1424)	(0.0888)	(0.1377)
	[0.0479 - 0.3979]	[-0.3031 - 0.2551]	[0.0038 - 0.3520]	[-0.2358 - 0.3040]
Urban potential of full towns outside estate * Land-labor ratio		0.0095		0.0055
		(0.0039)		(0.0036)
		[0.0018 - 0.0171]		[-0.0016 - 0.0125]
Urban potential of agro-towns outside estate	0.0781	0.0072	0.1110	0.1072
	(0.2076)	(0.3551)	(0.1828)	(0.3079)
	[-0.3289 - 0.4850]	[-0.6888 - 0.7032]	[-0.2472 - 0.4692]	[-0.4962 - 0.7106]
Urban potential of agro-towns outside estate * Land-labor ratio		0.0022		0.0001
		(0.0060)		(0.0051)
		[-0.0096 - 0.0140]		[-0.0099 - 0.0101]
Urban potential of royal towns	0.1186	-0.1088	0.1214	-0.0742
	(0.0717)	(0.1241)	(0.0549)	(0.0933)
	[-0.0218 - 0.2591]	[-0.3521 - 0.1345]	[0.0137 - 0.2290]	[-0.2572 - 0.1087]
Urban potential of royal towns * Land-labor ratio		0.0074		0.0061
		(0.0030)		(0.0021)
		[0.0017 - 0.0132]		[0.0020 - 0.0102]
Latitude	-15.34	-20.57	-25.94	-28.46
	(31.79)	(31.22)	(26.13)	(26.39)
	[-77.64 - 46.95]	[-81.75 - 40.61]	[-77.15 - 25.28]	[-80.18 - 23.25]
Longitude	7.165	5.124	-1.368	-3.590
	(61.08)	(60.22)	(51.12)	(51.14)
	[-112.56 - 126.89]	[-112.9 - 123.2]	[-101.56 - 98.82]	[-103.8 - 96.64]
Region dummy variables	YES	YES	YES	YES
Lordship type dummy variables	YES	YES	YES	YES
Estate-level mean values of regressors	YES	YES	YES	YES
Proportion correctly classified in first part	0.865	0.866		
R squared in second part	0.445	0.451		
Number of observations	11,342	11,342	11,349	11,349
Number of estates			1,357	1,357
Number of estates (first part)	1,355	1,355		
Number of estates (second part)	1,057	1,057		

Notes: We report marginal effects for two-part and Tobit regressions for ease of comparison with OLS coefficients. Standard errors are in parentheses, confidence intervals in brackets. For two-part, standard errors are clustered at the estate level. For RE Tobit, standard errors are bootstrapped with resampling over estates. The two-part regression has only 11,342 observations because when it was estimated, the seven observations for one particular lordship type were dropped since these observations perfectly predicted zero coerced labour in the first-stage logit regression. The Tobit regression was estimated on 11,349 observations but marginal effects could only be calculated using 11,348 observations because for one observation a predicted value of the dependent variable required to calculate the marginal effects was missing.

Figure A11: Elasticity of Human-Animal Coerced Labor with Respect to the Land-Labor Ratio, With and Without Land/Labor-Urban Interactions, Bohemia 1757

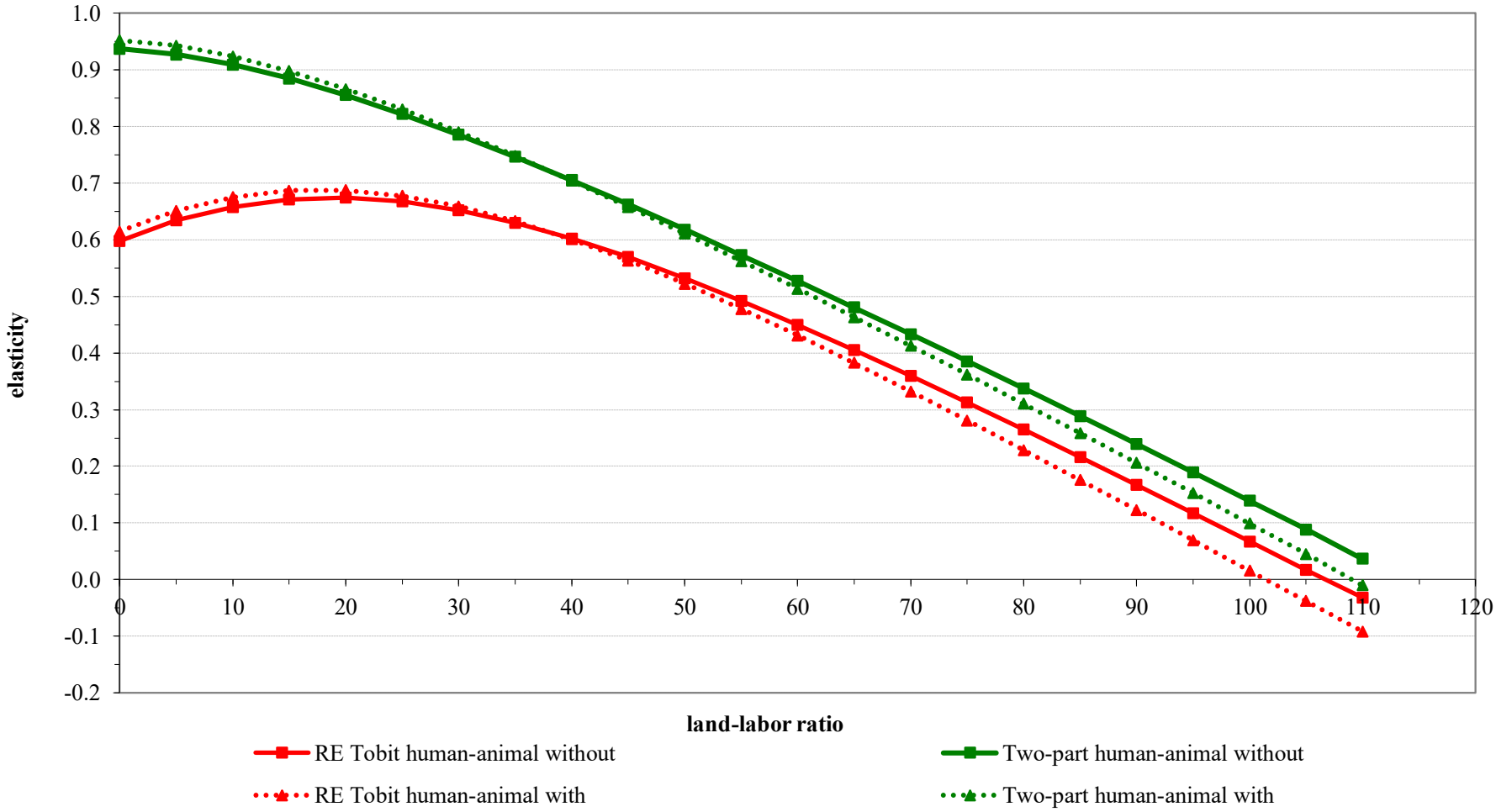


Figure A12: Elasticity of Human-Animal Coerced Labor with Respect to Village Size, With and Without Land/Labor-Urban Interactions, Bohemia 1757