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The Economic Effects of the Abolition of Serfdom: Evidence from the Russian Empire[†]

By Andrei Markevich and Ekaterina Zhuravskaya*

We document substantial increases in agricultural productivity, industrial output, and peasants' nutrition in Imperial Russia as a result of the abolition of serfdom in 1861. Before the emancipation, provinces where serfs constituted the majority of agricultural laborers lagged behind provinces that primarily relied on free labor. The emancipation led to a significant but partial catch up. Better incentives of peasants resulting from the cessation of ratchet effect were a likely mechanism behind a relatively fast positive effect of reform on agricultural productivity. The land reform, which instituted communal land tenure after the emancipation, diminished growth in productivity in repartition communes. (*JEL* J47, N13, N33, N43, N53, Q11)

The effect of slavery and serfdom on economic efficiency and growth has been the subject of a long-lasting debate.[1] Despite many scholars who view slavery and serfdom as inefficient production systems with distorted incentives and suboptimal resource allocation (see, e.g., Cairnes 1862; Williams 1944; North and Thomas 1973; Anderson and Gallman 1977; Acemoglu and Robinson 2012; Ogilvie 2013), there is no clear theoretical argument for why slave- and land-owners failed to provide efficient incentives to their workers.

The literature provides many case studies of highly efficient slave systems. For example, the abolition of slavery in the US South saw a sharp decline in output per person and the stagnation of the Southern economy for generations (e.g., Fogel 1989;

*Markevich: New Economic School, Novaya Ulitsa, 100a, Scolkovo, Moscow 121353, Russia (email: amarkevich@nes.ru); Zhuravskaya: Paris School of Economics (EHESS), 48 Boulevard Jourdan 75014 Paris, France (email: ekaterina.zhuravskaya@psemail.eu). This paper was accepted to the *AER* under the guidance of Penny Goldberg, Coeditor. We thank Greg Clark, Paul Castañeda Dower, Amanda Gregg, Paul Gregory, Avner Greif, Irena Grosfeld, Sergei Guriev, Tim Guinnane, Mark Harrison, Peter Lindert, Steven Nafziger, Alan Olmstead, Gavin Wright, anonymous referees, and the seminar participants at Stanford University, University of Pennsylvania, Yale, Oxford, Northwestern, UC Davis, FIU, University of Nottingham, University of York, Higher School of Economics, New Economic School, Paris School of Economics, Namur University, University Paris-Dauphine and the NES CSDCI Conference "The Role of History and Diversity in Understanding Development," and SIOE Annual Conference for insightful comments. Andrei Markevich thanks the Hoover Institution for hospitality. We also thank Vladimir Avetian, Alexis Ghersengorin, Andrei Govorun, Guli Kholmatova, Olga Pavlenko, Vasilij Rusanov, and Sergei Steblev for excellent research assistance. The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

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[1] Serfdom is an institution of forced labor in agrarian economies; it was widespread in Europe in the Middle Ages. By the early modern period, it disappeared from most parts of Western Europe, while persisting in most parts of Eastern Europe and, in particular, in the Russian Empire, until the nineteenth century.

Atack and Passell 1994).[2] Slave labor in the United States around the mid-nineteenth century was more efficient at producing cotton than free labor in the West Indies, Brazil, India, and Egypt (e.g., Fogel and Engerman 1974; Olmstead and Rhode 2008).[3] Haiti of the eighteenth century, with production based predominantly on slave labor, was the most prosperous colony in the Americas; however, after the war of independence, it did not retain its prosperity (e.g., Girard 2005). Similarly, some recent studies (i.e., Cerman 2012 and Stanziani 2014a) present serfdom in Eastern Europe as a dynamic institution sustaining a considerable rate of economic growth. More prominently, the Russian Empire has been used as an example confirming the idea that serfdom must not be a crucial determinant of backwardness, as Russia remained a backward agrarian society right up to the Russian Revolution despite the abolition of serfdom in the 1860s (Gerschenkron 1962, 1965; Moon 1996). The arguments on both sides of this debate were mostly backed by case study evidence.

In this paper, we provide new systematic empirical evidence about the effect of the abolition of serfdom on development that sheds light on this debate. We document substantial positive effects of the abolition of serfdom on agricultural productivity, industrial development, and peasant nutrition in the nineteenth century Russian Empire. Our estimates imply that Russia's GDP increased by about 17.7 percent as a result of the reform in the second half of the nineteenth century.

During serfdom, Russia's serfs were the property of the gentry, who had formal usage and transfer rights over them. The abolition of serfdom, triggered by the exogenous shock of Russia's defeat in the Crimean War (1853–1856), involved two distinct stages: (i) the emancipation of serfs, which instantaneously granted personal freedom to all serfs; and (ii) the land reform, which defined the communal land property rights of the emancipated peasants. The emancipation occurred in 1861 throughout the European part of the empire.[4] At the time of emancipation, the obligations of former serfs to landlords were fixed as the institutionalized rent payment for land use. The subsequent land reform completely abolished any obligations of former serfs to landlords by transferring land rights to peasant communes in return for redemption payments. Land reform implementation took over 20 years following the emancipation.

After having stagnated at least since the beginning of the nineteenth century, agricultural productivity in Russia started to grow approximately at the time of the emancipation of serfs, as illustrated in Figure 1. Our goal is to test whether this change in the trend was causal and to measure the impact of the abolition of serfdom on agricultural productivity, industrial development, and peasant living standards.[5] We assembled unique province-level panel data on development outcomes for European

[2] In part, this effect was due to fewer hours of work per person.

[3] Olmstead and Rhode (2008, 2010) contested the causal interpretation of this fact showing that the biological innovations rather than the organization of production were at the core of the explanation for the relatively high productivity at Southern slave farms.

[4] Baltic provinces are the exception: serfs in the Baltics were emancipated between 1816 and 1819.

[5] Contemporaries did not agree on whether the change in the trends was a result of the reform. On the one hand, the special government commission in 1872 concluded that "the positive consequences of the reform are more or less clear"; on the other hand, intellectuals, such as Pyotr Struve, attributed the change in the trends to other factors, such as industrialization. Online Appendix Section A1 describes the sources of these contemporaries' quotes as well as the results of the survey of experts conducted in 1872 by the special government's commission evaluating the impact of the reform. The main results of this survey are summarized in online Appendix Figure A1.

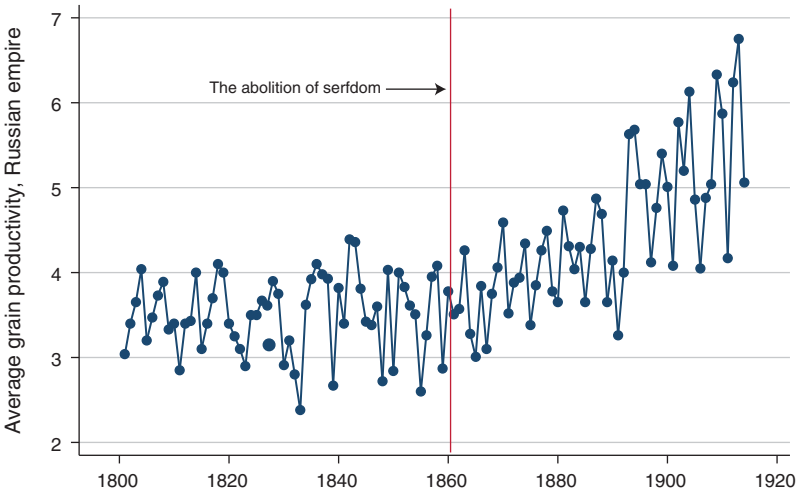

Figure 1. Dynamics of Agricultural Productivity in the Russian Empire

Note: The vertical line indicates the 1861 emancipation reform.

Source: Mikhajlovskij (1921, p. 50)

Russia between the end of the eighteenth and throughout the nineteenth centuries. Our empirical strategy is difference-in-differences with controls for province and time fixed effects. For the two main outcomes (agricultural productivity and industrial output), we also control for province-specific trends, as due to Russia's vast size, different provinces had different climatic and soil conditions and, therefore, different development trajectories. We estimate the change in the provincial development trends at the time of the emancipation of serfs depending on the pre-emancipation prevalence of serfdom (the share of serfs as compared to formally free rural residents) across Russian provinces. We also use cross-province and over-time variation in the rate with which the land reform was implemented. To address potential endogeneity and mismeasurement concerns, we rely on exogenous cross-province variation in the distribution of serfs driven by the nationalization of church lands and serfs on these lands by Catherine the Great and on exogenous cross-province and over-time variation in land reform driven by the differential incentives of landlords to push for land reform in collateralized and non-collateralized estates.

Serfs constituted 43 percent of all rural residents in European Russia in 1858. The formally free rural population consisted mainly of state peasants and free agricultural laborers. The composition of the rural population varied greatly across provinces: in 1858, the share of serfs ranged from 0.1 percent in Arkhangelsk to 83 percent in Mogilev; the share of serfs in the median province was 50 percent and in the mean province was 45 percent of the rural population.[6]

[6]The data on the composition of the rural population are from Bushen (1863). The sample is the European provinces of the Russian Empire, where emancipation took place in 1861, i.e., outside the Baltics.

We find that provinces where there were a lot of serfs before the emancipation lagged behind those provinces in which serfdom was not as prevalent, in terms of agricultural productivity, measured as the ratio of grain yield to seed (henceforth referred to as *grain productivity*). The abolition of serfdom caused a partial convergence in these two groups of provinces.

Difference-in-differences methodology allows estimating the difference in the effects between provinces with different initial levels of serfdom and not the level of the effect for any of the provinces. Theoretically, it is possible that the reform had a negative effect on provinces that relied on free labor under serfdom because of an increase in competition for nonlabor variable inputs after the emancipation from the provinces that initially relied on serf labor. Such a negative causal effect is, however, unlikely, because inputs, other than labor and land, played a relatively minor role in agricultural production in nineteenth century Russia.[7] Assuming that any observed fluctuations in agricultural productivity around the emancipation in provinces that relied on free labor were driven by a macro shock and were not related to the abolition of serfdom, we can interpret the difference-in-differences estimates as the effect on the level. Under this assumption, the abolition reform led to a 16 percent increase in grain productivity in the Russian Empire, above the overall development trend. The magnitude of this effect is comparable to 38 years of aggregate development as grain productivity on average increased by 4 percent per decade in nineteenth century Russia.

The quantity of the data on agricultural productivity allows us to disentangle the effects of the two components of the abolition of serfdom: the emancipation of serfs per se and the subsequent land reform. We find that the positive effect of the reform on agricultural productivity is entirely due to the emancipation. Obtaining personal freedom by serfs boosted growth in productivity, whereas the land reform significantly slowed it down, canceling out nearly one-half of the overall effect.

We examine the mechanism behind these effects. Consistent with Gerschenkron's (1965) arguments, we show that the inefficiency of land reform was associated with the repartition peasant commune, which undermined peasant incentives to invest in land. We also provide evidence consistent with the view that the change in peasants' incentives due to the loss of landlord's right to change the level of future obligations of peasants based on their previous performance, i.e., the cessation of the ratchet effect, was an important mechanism behind the relatively fast positive effect of the emancipation. The increase in agricultural productivity as a result of the emancipation occurred only in provinces (which constitute the majority) where landlords were unable to commit to long-term implicit contracts regulating the level of serfs' obligations, i.e., which suffered from the ratchet effect under serfdom. In addition, the production choices (i.e., which crops to seed, which to sell, and which to consume) became better adapted to climatic and market conditions following the emancipation in provinces with a larger share of serfs. These results are consistent with an increase in peasant effort post-emancipation and suggest that peasants' incentives

[7] We do observe a slight fall in productivity in the first two decades after the emancipation in provinces that were in the first tercile of the distribution of the share of serfs. This fall could be due to such a negative general equilibrium effect of the reform or it could be due to an unfavorable external macro shock.

played an important role in production and that the monitoring costs were too large for serf owners to ensure efficiency.

Further, we find a significant positive effect of the abolition of serfdom on industrial development. In all provinces, industrial output grew throughout the nineteenth century. Assuming that industry was not affected by the abolition of serfdom in provinces where labor was free to begin with, the difference-in-differences estimates yield that, in an average province where 45 percent of rural population was comprised of serfs, the abolition of serfdom led to an additional increase in industrial output of 39 percent throughout the second half of the nineteenth century. This is a large effect, especially in the face of the communal system of land titles and post-emancipation mobility restrictions set by the peasant commune, which reduced migration to urban areas (Gerschenkron 1965).

Finally, we find suggestive evidence that the abolition of serfdom also increased nutrition of former serfs. In particular, the emancipation was associated with a larger increase in the height of draftees under the universal conscription from areas with larger pre-emancipation share of serfs. There are many potential unobserved confounds in any analysis that uses the height of draftees as a proxy for nutrition, such as differences in local implementation of draft rules, local exogenous shocks to food supply, or local epidemics. Under the assumption that these confounds are not driving our results, the estimates imply that the height of draftees from private estates was 0.78 centimeters higher on average for cohorts born after the emancipation compared to cohorts born before the emancipation. The magnitude of this effect is comparable to an increase in the height of young adult men per decade in nineteenth century Western Europe (Hatton and Bray 2010).

The results proved to be robust to a battery of sensitivity tests. We test for and find no evidence of pre-trends in the analyses of grain productivity and industrial output. We also verify that our results are not driven by an underestimation of standard errors due to the presence of spatial and over-time correlation (Conley 1999, 2008). The results are also robust to controlling for several potentially confounding factors as well as an alternative data source for the prevalence of serfdom and using more granular district-level panel data for draftees' height.

Our paper relates to several strands of economic and historical literature. First, we contribute to the literature on institutions and economic development (e.g., Acemoglu and Johnson 2005; Banerjee and Iyer 2005; Nunn 2009; Acemoglu et al. 2011; Tabellini 2010; Bruhn and Gallego 2012; Michalopoulos and Papaioannou 2013; Ogilvie and Carus 2014). Our results are consistent with the view that the early disappearance of serfdom contributed to the rise of Western Europe and the Great Divergence between the West and East (e.g., North and Thomas 1973). Second, our work speaks to the literature on the efficiency of forced labor and its effects on economic development (e.g., Acemoglu, García-Jimeno, and Robinson 2012; Nunn 2008; Miller 2009; Dell 2010; Nunn and Wantchekon 2011; and Bertocchi and Dimicio 2014). More specifically, we contribute to the debate on the efficiency of serfdom in the Russian Empire, in which Gerschenkron (1962, 1965) and Koval'chenko (1967) argued that serfdom was inefficient, in contrast to Hoch (1986), Moon (1996), Mironov (2010), Dennison (2006, 2011), and Stanziani (2014a, b), who portray serfdom as a dynamic institution that sustained a considerable rate of economic development.

The literature, prior to our paper, was based primarily on sporadic anecdotal evidence with the important exception of Nafziger (2013) and Buggle and Nafziger (2016), who study the long-term effects of serfdom and document a negative cross-sectional relationship between the prevalence of serfdom and the long-term land inequality and well-being. The results of our paper combined with the findings of Buggle and Nafziger (2016) suggest that serfdom had a negative effect on development overall and that the emancipation reversed a substantial part of this influence.[8] Our work is also related to the literature on land reforms and land property rights (e.g., Besley and Ghatak 2010; Deininger and Feder 2001; and Fenske 2011). We show that the introduction of communal land titles had a negative effect in contrast to many examples of growth-promoting land reforms (Lipton 2009).

The paper proceeds as follows. In Section I, we present our hypotheses. Historical background is provided in Section II. In Section III, we describe the data. Section IV presents the empirical strategy. Section V reports the results. In Section VI, we describe a number of robustness checks. Section VII concludes.

I. Hypotheses

The effects of the abolition of serfdom on agricultural productivity, industrial development, and peasants' well-being are a priori ambiguous. On the one hand, it is reasonable to expect the emancipation to alleviate incentive problems in agricultural production. The serfs' effort and their proceeds were largely unobservable to the landlord due to monitoring costs, which led to severe distortions in the serfs' effort. The lack of credible commitment on the part of landlords not to revise the size of peasants' obligations in the future must have reduced peasant effort as a consequence of the ratchet effect. Anecdotal evidence suggests that some landlords were able to credibly commit to follow rules that fixed the amount of the obligations of peasants, maximizing the stream of payments over a longer-term horizon; however, this was not a common practice (Dennison 2011). Serfdom was also associated with adverse incentives for peasants to invest in their own human capital and land, both of which belonged to the landlord, in addition to the serfs' labor.

On the other hand, the use of coercion on forced labor under serfdom may have been a more effective enforcement mechanism to ensure effort on landlord farms compared to subsequent free labor relations (Acemoglu and Wolitzky 2011): the emancipation prohibited landlords to whip their former serfs, a common practice under serfdom, and that could have decreased productivity of landlord farms after the emancipation (e.g., Hoch 1986; Druzhinin 1966).[9]

[8] Buggle and Nafziger (2016) were the first to use an exogenous variation prevalence of serfdom coming from the nationalization of the monasterial lands a century before the emancipation of serfs. We also rely on the historical distribution of monasterial serfs for our instrumental variable strategy, but our identification assumptions are weaker due to the panel nature of the data we use, which allows controlling for province-specific trends. Other relevant contributions to the empirical literature on the history of the Russian Empire are Mironov and A'Hearn (2008); Nafziger (2012); Finkel, Gehlbach, and Olsen (2015); Castañeda Dower et al. (2018); Chernina, Castañeda Dower, and Markevich (2014); Castañeda Dower and Markevich (2017); and Nafziger (2016).

[9] We describe qualitative historical evidence of the changes in the Russian countryside after the emancipation in the online Appendix. It suggests that the emancipation brought about an increase in productivity on peasant farms and a decrease in productivity in landlord farms.

Thus, the extent to which the gentry could solve incentive problems by intense monitoring, commitment to long-term contracts, or coercion should determine how inefficient serfdom was. Many of these incentive problems are expected to have been alleviated with the emancipation. However, we expect most of these changes to be gradual. By contrast, the emancipation solved the ratchet effect problem right away by fixing the level of obligations for all (former) serfs. Yet, as the emancipation also prohibited landlords to whip their former serfs, the enforcement power of landlords on their own farms was also momently reduced.

Incentive problems are just a part of the story. Serfdom could have had efficiency advantages compared to post-emancipation production because of the economies of scale, access to finance and to new technologies, the enforcement of social order, and the ability to smooth consumption during shocks: all of which most probably were better realized in the large estates of gentry compared to the small entrepreneurial farms of emancipated peasants (Moon 1996; Mironov 2010; Dennison 2006, 2011; and Stanziani 2014a, b).

The expected effect of the land reform is also ambiguous. On the one hand, the land reform could have improved productivity by increasing peasants' incentives to invest in the land that they acquired. On the other hand, the land reform both de jure and de facto strengthened the institution of the commune, whose power was previously counterbalanced by the landlord's authority. Communes restricted the transfer rights over land and regulated major production decisions based on traditional practices, which could create distortions (Gerschenkron 1965, pp. 744–45). The so-called repartition communes that were the dominant form of land use in most parts of the empire periodically redistributed land among households despite the perverse effect on incentives to invest in land.[10]

One could expect a positive effect of the abolition of serfdom on the development of industry. First, under serfdom, the ratchet effect problem also applied to the artisan (industrial) activities of serfs, as these activities were also subject to arbitrary levels of quitrent from the lords. The emancipation eliminated this problem for the industrial production of serfs as much as for their agricultural production. Second, personal freedom given to serfs by the emancipation reform also could have increased mobility from rural to urban areas, where productivity and wages were higher. However, migration to cities was limited by the communal land titles, passport system regulated by the commune, and mutual tax responsibility within the commune, which should have slowed down industrial development (Gerschenkron 1965).

Similarly, it is also not clear whether one should expect peasant nutrition to be affected by the emancipation. If the emancipation led to higher productivity of former serfs, productivity improvements could translate into higher consumption. In addition, peasants may have also had lower incentives to feed children under serfdom, as peasants' children belonged to the gentry. Yet, serfs were the most valuable input into production for gentry and, therefore, rational landlords should have

[10] Gerschenkron (1965, p. 747) wrote about the repartition communes that "in Russian reality, the general shortcoming of the strip system were further aggravated by the temporary character of land use and the strong disincentive to improve a piece of land that sooner or later was to be transferred to another household." A recent literature studies the implementation of these legal constraints and their effect on Russian agriculture (Castaneda Dower and Markevich 2017; Chernina, Castañeda Dower, and Markevich 2014; Gregory 1980; Nafziger 2010, 2016).

made sure that their serfs were well fed. However, the asymmetry of information may have led to the malnutrition of serfs in equilibrium due to an excessively high level of peasant obligations arising from the concern of gentry that peasants hid the proceeds of their production.

II. The History and Geography of Russian Serfdom: A Short Overview

Serfdom was one of the key institutions in Russian history. It existed in its most severe form between 1649 and 1861 (i.e., 212 years). Originally, Russian peasants were free and could migrate across estates. The government began to limit the right of migration in the late fifteenth century. The 1649 Code of Law (*Sobornoye Ulozhenie*) proclaimed that peasants were the property of their estates and made migration a criminal offence. Peasants became attached to the land and had to obey the orders of their landlords. Serfs had to fulfill obligations in the form of in kind payment (quitrent) or labor (corvée) for their landlords. The landlords had (almost) full discretion over the amount and the form of these obligations. The landlords also had the right to sell, to buy, or to lease their serfs (*Russian Empire* 1857, vol. 9, articles 208, 1027, 1029, 1037, 1047, 1048, 1068).[11]

Our sample covers the European part of the Russian Empire (excluding the Kingdom of Poland and the Great Duchy of Finland), which was the home of about 80 percent of the total population of the empire (see Figure 2). In the middle of the nineteenth century, more than 90 percent of the population lived in rural areas (Bushen 1863). In 1858, 43.03 percent of all peasants were privately owned serfs. The rest of Russian peasantry could be classified into three large groups according to their legal status: the state peasants (40.4 percent of rural population); free agricultural workers (12.6 percent); and royal peasants (4 percent), all of which de facto can be considered (relative to serfs) as formally free peasants subjected to fixed taxation and land-lease rules.[12]

The composition of the rural population and, in particular, the shares of serfs versus all other groups of peasants who were formally free, substantially varied across provinces while being relatively stable over time in the last 60 years of serfdom.[13] Serfs were more prevalent in the "old" regions of the empire closer to Moscow, whereas state peasants and free agricultural workers were more numerous in the outskirts of the empire. The reasons for this spatial pattern were closely connected to the construction of the army and to the specificities of Russian conquest.[14] Figure 3

[11] The state sometimes intervened in cases of starvation and torture of serfs. The law also limited sales of serfs without land (*Russian Empire* 1857, vol. 9, articles 208, 1045, 1080–1084, 1102–1106, 1109–1113).

[12] We describe the legal status of each of the non-serf groups of peasants in more detail in online Appendix Section A2. In terms of the severity of the individual constraints on members of each of these groups, the free agricultural workers were the less constrained compared to state peasants and royal peasants, who, in turn, were much freer than serfs.

[13] There were no conversions of state or royal peasants or free rural population into serfs after the reign of Pavel I (1796–1801). The only major change in the respective shares of serfs and non-serf rural population took place in 1816–1819, when serfs in the Baltic provinces were emancipated and became free landless agricultural workers. The law of 1803 introduced a right of landlords to free their own serfs. Only about 100,000 male serfs (i.e., less than 0.5 percent of rural male population) were freed between 1803 and 1858 under this law (Veshnyakov 1858).

[14] See online Appendix Section A3 for details.

FIGURE 2. EUROPEAN PROVINCES OF THE RUSSIAN EMPIRE

Notes: Equirectangular projection used. Serfs in the Baltic provinces, Estlyndiya, Liflyandya, and Kurlyandia, were liberated 40 years before the emancipation of serfs in the rest of the empire. We run regressions both with and without the Baltic provinces in the sample. The baseline sample excludes them.

presents the spatial distribution of serfs across provinces in 1858.[15] An important determinant of the relative shares of serfs versus state peasants was the location of monasteries. In 1764, the lands and the serfs of the Orthodox Church, which was a major land-owner prior to that moment, were confiscated by the state and transferred to state ownership.[16]

A. *The Abolition of Serfdom: The Emancipation and the Land Reform*

Discussions of a potential emancipation reform within the Russian Empire began in the late eighteenth century (Dolgikh 2006). However, real steps toward enacting this reform were undertaken only following Russia's defeat in the Crimean War (1853–1856). The defeat against a coalition, which included Great Britain and France, demonstrated to the government that Russia had fallen behind other European countries and that liberalization was overdue.[17]

The Manifesto of February 1861 (and related laws) granted personal freedom to former serfs instantaneously and free of charge and outlined the rules of the subsequent land reform (*Russian Empire* 1863, vol. 36, part 1). Landlords lost the right to change the level of peasant obligations, to sell, buy, lease, punish, or

[15] Online Appendix Figure A2 confirms a strong negative correlation between the share of serfs and the proximity to Moscow (we account for this correlation in our empirical strategy). Figure A3 presents the spatial distributions of state peasants (panel A) and free agricultural workers (panel B).

[16] The nationalization reform of 1764 affected only monasteries in the central provinces of European Russia. In 1786 and 1788, monasterial lands were nationalized in Ukrainian and Southern provinces. The confiscations of the lands belonging to the Catholic Church took place in the late eighteenth to the first half of the nineteenth century. We describe the historical details of the nationalizations of the church lands in online Appendix Section A4.

[17] See online Appendix Section A5 for details.

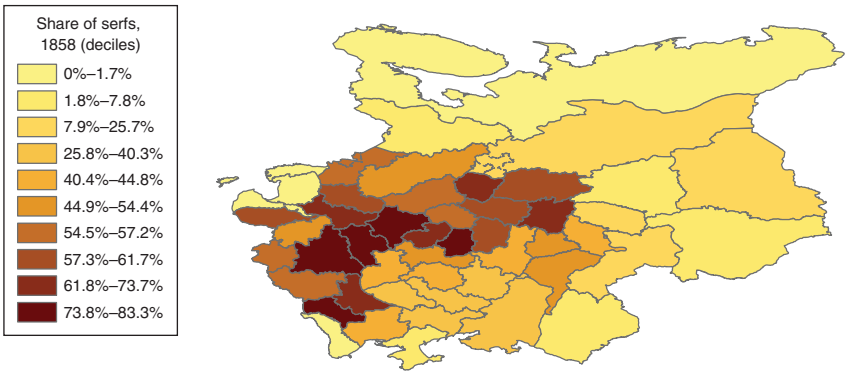

FIGURE 3. GEOGRAPHY OF SERFDOM: SERFS IN 1858 AS A SHARE OF RURAL POPULATION

Note: Equirectangular projection used.

imprison peasants.[18] Emancipated serfs had an obligation by law to buy out the land from the landlords. Peasants (as a commune) and their landlords had to negotiate the precise terms of this buyout, namely, the plots, the price, and the exact timing of the transaction.

The land reform was gradual and proceeded in two stages. The first stage regulated the peasant-landlord relationship in the form of a *regulatory charter* during the transition period, i.e., before the *buyout contract* was signed. The second stage marked the actual transfer of ownership over the land in exchange for an immediate payment, the terms of which were regulated by the buyout contract between the landlord, the peasant commune, and the state. The regulatory charters had to be signed by 1863; they fixed the amount of the lease payment (in the form of a quitrent or labor obligation of the landlord farm) for the use of land by peasants until the transfer of property rights and abolished all other peasant obligations to landlords. About 50 percent of the regulatory charters were signed as a result of a mutual agreement between peasants and landlords. In the absence of an agreement, local officials imposed the terms of a fallback regulatory charter. On estates where landlords did not change the level of peasant obligations during serfdom, i.e., where landlords were able to commit to an implicit long-term contract with peasants, these agreements were usually easier to reach, as they formalized the previously existing implicit contract. Peasants were less likely to agree with the terms of proposed regulatory charter if the terms were worse than those under serfdom (Litvak 1972; Zajonchkovskij 1968). Other factors, such as the attitude and skills of local officials, who arbitrated signing the regulatory charters, could also have affected chances to reach an agreement. However, in practice, the central government closely controlled the procedure and established an independent institution of local arbitrators, who secured relatively homogeneous implementation of the reform (we provide further details in the online Appendix).

[18] Former serfs were also granted a set of civil rights, including the right to marry without anybody's permission, to buy, sell, and lease property, to sign contracts, trade, launch businesses, and to represent themselves in court (*Russian Empire* 1863, vol. 36, part 1).

Eighty percent of the land value specified in the buyout contract was financed by the state in the form of a 49-year state loan to peasants, who had to pay back a fixed redemption amount annually. The time period for signature of the buyout contract ranged from 1862 to 1882. In western provinces, the land buyouts were completed by 1863 as a political measure following the Polish rebellion against the empire. In the eastern provinces, initially, the timing of the signature of the buyout contracts was not regulated; as a result, for 15 percent of former serfs, the contract negotiations lasted until 1881, when a new law prescribed an obligatory start of land buyouts (*Russian Empire* 1885, vol. 1; Zajonchkovskij 1968). An important determinant of the length of the transition period was landlord's indebtedness to the state. If the land was used as collateral, the buyout meant that the state deducted landlord's debts from the buyout amount and collected the payments, which the landlord otherwise would have gotten from the peasants for the land, leaving the landlord without money and land lease payments. Importantly, as a rule, lease payments were higher than the interest on the state loans. In contrast, landlords without debt got the full value of the land sold to former serfs at the signing of the buyout contract.[19]

III. Data

We combine various published and archival sources to construct a unique province-level panel dataset on the development of 46 European provinces of the Russian Empire in the nineteenth century.[20] Table 1 reports descriptive statistics and online Appendix Table A1 provides the data sources and lists the years for which the data are available for all variables used in the analysis.

Outcome Variables.—Grain was the main output of the empire. We measure grain productivity as the grain yield to grain seed ratio because there are no panel data on labor and nonlabor inputs that would cover both pre- and post-emancipation periods. Grain productivity is widely used as a proxy for productivity in agriculture in Russia before the late nineteenth century as well as in medieval and early modern Europe. Data on grain productivity come from the annual governor reports for the years before 1883 and the official imperial statistics of the Central Statistical Committee for the later period.[21] The methodologies of the data collection were different before and after 1883, but the same within each of these periods irrespective of prevalence of serfdom in a province.[22] The quality of the late imperial statistics and governor reports is rather high (Koval'chenko 1979; Nifontov 1974, pp. 35–46).[23]

[19] We describe the details of the land reform and the determinants of gentry's indebtedness in online Appendix Sections A6 and A7, respectively.

[20] The baseline sample excludes Baltic provinces because these provinces differ from the rest in many respects. We discuss the robustness of the results to including the Baltics in the sensitivity section.

[21] For governor reports, we rely on the secondary published sources based on original archival documents.

[22] Governor reports provide only aggregated figures on all cereals before 1883. We aggregate data on rye, oat, wheat, barley, and buckwheat for the post-1883 period by summing up yields and seeds of these crops and taking a ratio of the sums to construct comparable measures. In Section VI, we verify that the change in the methodology of collection of grain data that occurred in 1883 does not drive our results for grain productivity. We provide further details in online Appendix Section A9 (on the methodology of data collection in the Russia Empire) and Section B (on the variables used in the paper).

[23] According to Nifontov (1974), the official procedure for data collection was very deliberate. It required a lot of cross checking by various local authorities. In addition, the central government carefully monitored implementation

TABLE 1—SUMMARY STATISTICS

	Obs.	Mean	SD	Min	Max
Panel A. Serfdom in 1858					
Share of serfs (by province)	46	0.45	0.24	0.001	0.83
Share of serfs (by district)	447	0.42	0.23	0	0.85
Share of state peasants (by province)	46	0.39	0.21	0	0.88
Share of formally free rural population (by province)	46	0.12	0.17	0.04	0.85
Panel B. Land reform during the years of its implementation (1862–1882)					
Land reform: Share of peasants with signed buyout contracts in 1862–1882 (by province and year)	877	0.32	0.24	0	0.83
Panel C. Development outcomes					
Grain productivity, yield-to-seed ratio (by province and year)	1,835	3.79	1.26	0.59	12.30
log industrial output, mln 1895 rubles (by province and year)	347	15.46	1.67	9.75	19.63
Height of draftees, centimeters (by province and birth cohort)	584	164.75	0.83	162.55	167.02
Height of draftees, centimeters (by district and birth cohort)	4,437	162.60	1.64	157.72	169.99
Panel D. Instruments					
Average share of monasterial serfs between 1796 and 1814 (by province)	46	0.09	0.08	0	0.39
Average share of monasterial serfs between 1796 and 1814 (by district)	439	0.09	0.11	0	0.64
Gentry indebtedness in 1858 (by province)	44	0.13	0.07	0.003	0.29
Interpolation between [1-indebtedness] and 1 in the interval 1862–1882 (by province and year)	877	0.95	0.06	0.71	1
Panel E. Other variables					
Implicit contracts: share of serfs with signed regulatory charters by 1863 (by province)	44	0.43	0.21	0.019	0.85
Repartition commune dummy (by province)	46	0.87	0.34	0	1
Share of winter crops seeded in total crops seeded (by province)	800	0.41	0.10	0.09	0.64
Distance to Moscow, km (by province)	46	666	323	24	1,307
Distance to Moscow, km (by district)	447	600	303	43	1,596
Crop suitability index (by province)	46	2.17	1.33	1	5
Crop suitability index (by district)	447	2.25	1.35	1	6
Rye-to-wheat world price ratio (by year, for the years with data on the composition of crops)	18	0.73	0.069	0.64	0.88
Rye-to-oat local price ratio (by mega-region and year, for the years with data on the composition of crops)	759	1.08	0.21	0.55	1.94

Note: The summary statistics are reported for the baseline samples, i.e., without Baltic provinces at the province level and and without Moscow at the district level.

Industrial development is measured by log industrial output in constant rubles of 1895.[24] These data also come from the governor reports and official statistical volumes published by the Central Statistical Committee.

The nutrition of peasants is proxied by the average height of 21-year-old draftees by birth cohort and province or subprovince (district) of residence; we calculate it from the data on the number of draftees in each of the nine height categories, reported by the Ministry of Defense of the Russian Empire. These measurements

of the data collection, as the data were used for potential tax redemption and state transfers. Nifontov (1974) verified that the time series of grain yields from the alternative sources, such as reports of the Ministry of State Property, are highly correlated with those based on the governors' reports. Fortunatov (1893) compared data on yields from governors' reports with the figures from individual estate archives and concluded that they are very similar (see online Appendix Section A9 for details).

[24] We use the Mironov (2010) index for Saint Petersburg to deflate industrial output reported in current rubles in the original sources. There are no deflation indexes for other regions; we check that our results are robust to deflation by regional rye prices taken from Mironov (1985). Online Appendix Table A16 presents the results.

were collected for conscripts drafted between 1874 and 1887 after the universal conscription was introduced by the 1874 military reform. Rural citizens were the main source of draftees for the army (Beskrovnii 1973). Draftees were chosen at random through an official lottery from the pool of all eligible 21-year-old men. This pool differed from the population of all 21-year-old men because of a number of exceptions based on family status, health, and anthropometric characteristics. Eligibility rules were the same across administrative units in each given year but varied slightly over time. The minimum height requirement of 153.35 centimeters was constant but the required minimum chest size (expressed in relative chest-to-height terms) changed in 1882, 1883, and 1885. We use available disaggregated data on height and chest size of draftees from Bobruisk district (available from Gorskij 1910) to estimate the share of men who got the exemption from service because of their chest size, in each height category by cohort and province (district). Then, we use these estimates to correct for a possible selection bias introduced by the changes in eligibility rules in each height category in the data reported by the Ministry of Defense. Importantly, as we show below, this correction did not have any impact on the estimates of the differential effect of the emancipation on provinces with high and low share of serfs, even though it did affect the average height of draftees. (Online Appendix Section A11 provides detailed description of military draft rules, the possible biases created by the eligibility rules, and the correction procedure that we employ.)

All three outcomes are available at the province level.[25] In addition, height data are available at district level with 447 districts in the baseline sample. Different numbers of snapshots over time are available for different outcomes. The largest number of over-time observations, 43, is available for grain productivity. The number of cross sections for the industrial output is 8. The number of cohorts with data on height is 14 at the province level; these are data covering all cohorts born between 1853 and 1866. At the district level, height data are available for 10 cohorts born between 1853 and 1862.

There are time gaps in the data for agricultural productivity and industrial output. We examined whether the years for which the data are available systematically differ from those years when the data are missing and find no systematic differences; we also found no change in this pattern between before and after the abolition of serfdom.[26] Occasionally, data on grain productivity and industrial output for some provinces are missing in the historical sources; thus, the resulting panels for these outcomes are unbalanced.

The Main Explanatory Variables.—We use cross-sectional data on the prevalence of serfdom across provinces and across districts before the emancipation. The data on the composition of the rural provincial population by status in 1858, i.e., the

[25] For height, our sample consists of 42 provinces because we exclude Don, Orenburg, Astrakhan, and Arkhangelsk for the following reasons. Data on height of draftees exclude Cossacks, who were a sizable share of the population in Don and Orenburg provinces. Local minorities present in Astrakhan and Arkhangelsk provinces in fairly large numbers were excluded from the military conscription. See online Appendix Section A11 for further details on the rules of draft and height data.

[26] Online Appendix Table A2 presents the results: in a time series setting, we regress dummies for whether our outcome variables are available for the detrended average of Russia's grain productivity by year and its interaction with the post-emancipation dummy for the entire nineteenth century.

shares of serfs, state peasants, free agricultural workers, and royal peasants, come from Bushen (1863).[27] The data on the number of serfs by district in 1858 come from Trojnitskij (1861); to get the share of serfs by district, we divide their number by district total population in 1858 from Bushen (1863).

We measure land reform implementation across provinces and over time with a proxy for the share of serfs who signed buyout contracts among the total rural population in each province and year. To construct this variable, we use two data sources: (i) the redemption payment statistics, which report the sums that peasants paid to the state for the loan annually by province. These data are available for all provinces and years up to (and including) 1876 and (ii) the 1877 cross section of the number of peasants who had signed buyout contracts by that time (Vilson 1878). First, we extrapolate the redemption payment statistics for each province for 1877, using a linear projection from 1870–1876 province-specific figures (after verifying that the redemption payments grew linearly in each province between 1870 and 1876). Then, we calculate the redemption payments per peasant in 1877 by dividing our estimate of redemption payments in 1877 by the number of peasants who signed buyout contracts in 1877. As a next step, we construct the share of serfs who signed buyout contracts each year in each province between 1862–1877 using redemption payment statistics and assuming constant redemption payments per peasant across estates and over time within each province. Finally, we extrapolate these numbers to the remaining four years of the land reform implementation, i.e., 1878–1881, using a linear projection from 1871–1877. As land reform was completed in 1882 by law (*Russian Empire* 1885, vol. 1), we set the share of serfs who signed buyout contracts among the total rural population to be equal to the total share of former serfs from 1882 onward.[28] In the nine westernmost provinces (Kovno, Vilno, Grodno, Minsk, Kiev, Mogilev, Podolsk, Vitebsk, and Volhyn), we set the proxy for the land reform implementation to be equal to the share of former serfs from 1863 onward due to the obligatory buyouts in these provinces in 1863 (in response to the Polish rebellion).

We use the share of serfs that belonged to monasteries and clergy before their nationalization (most of which took place in 1764) as an instrument for the prevalence of serfdom across provinces and districts in 1858. Henceforth, we refer to serfs that belonged to monasteries and clergy before the nationalization of church lands as monasterial serfs. These data come from Beskrovnii, Vodarskij, and Kabuzan (1972).[29] We also use data on the gentry's debts to a state bank

[27] We define the number of serfs in a province as the sum of two categories of peasants from Bushen (1863): *temporary obliged peasants* and *former serf-servants*. The number of state peasants in a province is the sum of *state peasants* and *military dwellers*. We consider the following groups as making up the rural population: *serfs*, *royal peasants*, *state peasants*, *military dwellers*, *soldiers in reserve*, *former soldiers*, *cantonists*, *citizens from irregular military regiments* (i.e., *Cossacks*), *colonists*, *peasants under supervision of various ministries*, *rural raznochintzi*, *foreigners in rural areas*, *non-Russians in rural areas*. Taken together, the latter nine groups comprise the free rural population in our classification. We verified that our results are robust to using 1857 tax census data (Kabuzan 1971) as a source of data for the composition of the peasantry by type instead of Bushen (1863). See Section VI for details.

[28] We provide the exact formula for the land reform implementation variable in online Appendix Section B. The actual figures for the implementation of the land reform are available for a number of provinces in 1870 (Obruchev 1871). The coefficient of correlation between our proxy for the land reform implementation and these numbers is 0.94.

[29] Beskrovnii, Vodarskij, and Kabuzan (1972) gives information about the number of (former) monasterial serfs per district at two points in time, 1796 and 1814. We take an average of the shares of monasterial serfs for the two periods. Details on the construction of this variable are presented in online Appendix Section B.

and other state financial institutions, which accepted deposits and provided credit, before the emancipation from Skrebitskij (1862–1866) to construct an instrument for the implementation of the land reform between 1862 and 1882 (we describe this instrument in the methodology section below). Henceforth, we refer to all the state financial institutions as state banks (historical details about gentry's debts are provided in online Appendix Section A7).

Additional Data.—We rely on FAO GAEZ data and the digital map of the Russian Empire to construct land suitability for grain cultivation by province and district using the median value for the respective polygon and the weather station data from the Global Land Surface Databank (Rennie et. al. 2014) to construct the series of annual mean temperatures by province and year. For these calculations as well as the distance to Moscow from the centroid of each province and each district, we use the digitized map of the Russian Empire (Kessler and Markevich 2015). To examine the mechanisms behind our main results, we use the following variables: a dummy for whether repartition communes were a prevalent form of communes in a province in 1905 comes from Dubrovskij (1963). These data are not available for earlier years, but we can use 1905 data in regression analysis because very few (if any) communes changed their status.[30] The share of serfs who agreed to sign regulatory charters in a province (our proxy for the prevalence of implicit contracts under serfdom) comes from Vilson (1878). The data on the composition of winter and spring grains are from the same sources as grain productivity, but they are available only for 18 points in time. To measure the relative price of winter to spring crops, we use: (i) the time series of the relative price of rye (the main winter crop) to wheat in the Netherlands goods exchange available from van Reil (2016); and (ii) the panel data on the regional relative price of rye to oats in Russia from Mironov (1985).

IV. Empirical Methodology

We use cross-province variation in the share of serfs to estimate the effect of the abolition of serfdom on the considered outcomes. For agricultural productivity and industrial development, our main specification is as follows:

$$(1) \qquad Y_{it} = \alpha\, ShareSerfs_i \times PostEmancipation_t + \mathbf{X}_{it}'\gamma + \psi_i + \mathfrak{G}_t + t\delta_i + \varepsilon_{it}.$$

Subscripts i and t index provinces and time periods. Time periods are either years or a series of consecutive years (e.g., decades), depending on data availability for a particular outcome. Y denotes grain productivity (yield/seed ratio) or log(industrial output) in province i at time t. *ShareSerfs* denotes the share of privately owned serfs in a province in 1858. *PostEmancipation* denotes a dummy indicating the time after

[30] Note that not all repartition communes, which had the legal right to redistribute the land across households, actually did this; and there are some anecdotes of redistribution of land across households in the hereditary communes (e.g., Dubrovskij 1963). As there are no systematic data on actual redistributions of land, we rely on the legal distinction between the repartition and hereditary communes, as the first approximation to the actual practices.

the emancipation of serfs, i.e., this dummy switches on in 1861 for the baseline sample.

The interaction between the share of serfs and the post-emancipation dummy is our main variable of interest. The coefficient on this interaction α is the difference-in-differences estimator of the effect of the abolition of serfdom on the considered outcome. In order to estimate this parameter consistently, we need to control for macroeconomic shocks, unobservable characteristics of provinces, as well as provincial trends. ψ_i and $б_t$ are the province and year fixed effects. As different provinces are expected to have different development trajectories, we control for 46 province-specific linear trends ($t\delta_i$) in the case of grain productivity and 14 region-specific trends in the case of industrial output. (The time dimension of the data for industrial output is insufficient to control for linear trends for each province.) To account for the correlations between the share of serfs with the distance from Moscow and soil quality, we control for the interactions between the post-emancipation dummy and log distance from Moscow and land suitability, minus their respective sample means; these variables are denoted by $\mathbf{X}_{it}$.[31]

To ensure that our results are not driven by the two main potentially confounding reforms, we adjust specification by including controls for the state peasants' and royal peasants' reforms into the ordinary least squares (OLS) panel regressions: we add the interactions of the shares of these groups in provincial rural populations with post-1866 and post-1859 dummies, respectively.[32]

The main identifying assumption in equation (1) is that there are no systematic differences in the trends of the outcomes of interest among provinces with different prevalence of serfdom before the emancipation (conditional on all other covariates, including province-specific trends). We test this for each outcome by replacing the interaction between the share of serfs ($ShareSerfs_i$) with the post-emancipation dummy by a series of interactions of $ShareSerfs_i$ with a number of dummies indicating different pre-reform and post-reform time periods.

To study the effect of the emancipation on nutrition, we also use a difference-in-differences methodology, but with one important difference compared to the analysis which uses grain productivity or industrial output as outcomes: we do not observe height of draftees before the reform. All cohorts for which height data are available had been affected by the emancipation, but to a different extent. The draftees from the oldest cohort in our data were eight years old at the time of emancipation and draftees from the youngest cohorts were born five years after the emancipation. If emancipation affected nutrition and nutrition throughout childhood affected height of young adults, one should observe the differential trends in the average height of draftees in provinces with different shares of serfs, as younger cohorts spend a larger share of their childhood after the emancipation. Importantly, nutrition of the pregnant mother and of an infant in the first few years of life are particularly important for the height of an adult (Costa 2015), therefore, one should also expect the largest differences in height between cohorts born right before and right after the emancipation in provinces where serfdom was prevalent.

[31] The means are subtracted in order for α to estimate the effect of the abolition of serfdom at the mean levels for the distance from Moscow and land suitability.

[32] To be precise, the post-1866 dummy switches on in 1866 and post-1859 switches on in 1859.

To account for the fact that the differential trends are an important part of the main effect in the case of height, we estimate the following equation:

$$H_{ic} = \sum \alpha_c \times ShareSerfs_i \times D_c + \mathbf{X}'_{ic}\gamma + \psi_i + \text{б}_c + \varepsilon_{ic}, \tag{2}$$

where H stands for the average height of draftees, c indexes cohorts, and i indices either provinces or districts. D_c denotes dummies for the pairs of consecutive cohorts. A series of coefficients α_c estimates the dynamics of the effect of the emancipation on height. We expect a gradual increase in α_c coefficients both before and after the emancipation. We also estimate an additional specification, in which we replace the series of interactions between the share of serfs ($ShareSerfs_i$) and cohort dummies D_c with a single interaction between the share of serfs and a dummy for cohorts born after the emancipation. On average, we expect an increase in height to be bigger between cohorts born after the emancipation compared to cohorts born before the emancipation in provinces with a large pre-emancipation share of serfs than in provinces with a small share of serfs because in provinces with a larger share of serfs larger shares of draftees from cohorts born before the emancipation had a part of their childhood under serfdom.[33]

We follow Bertrand, Duflo, and Mullainathan (2004) and cluster error terms within each province separately before and after the emancipation of 1861 in panel specifications for all three outcomes. This system of clusters accounts for autocorrelation in residuals within each province. However, it does not account for spatial correlation. This is potentially problematic because the share of serfs is spatially correlated as can been seen in Figure 3. To verify that we do not underestimate standard errors due to the presence of both the spatial and over-time correlation in residuals, we collapse the panel data to a single cross section, in which we explicitly account for spatial correlation. In particular, we detrend each outcome of interest by taking residuals from regressing it on time dummies for all outcomes, and in the case of grain productivity on province-specific linear trends and in the case of industrial output on region-specific linear trends, and take the difference between the mean of detrended outcome before and after the emancipation separately in each province. As a result, we get the province-level cross sections of the average growth in each outcome between post- and pre-emancipation periods and regress these variables on the share of serfs controlling for log distance from Moscow and land suitability, correcting for spatial correlation of errors (Conley 1999, 2008). We allow for correlation across space within a radius of 900 kilometers, the distance equal to about one-third of the West-East and North-South dimensions of the territory for which we have data (it is sufficiently large to account for any existing spatial correlation).[34]

As the differences in the prevalence of serfdom are not random (and may be driven by some unobserved factors), we also use an instrumental variable strategy to

[33] We cannot control for the reforms affecting state and royal peasants in the case of height, because we do not have enough cross sections for the period after the state peasants' land reform (one in the province-level dataset and zero in district-level dataset). In addition, data on state and royal peasants are not available at district level.

[34] We verify that the results are robust to setting different thresholds for spatial correlation (unreported for conciseness). We also verify that our results are not driven by influential observations in this cross-sectional regression by calculating DFBeta coefficients for the main variable of interest, i.e., the share of serfs, for each observation and reporting results on the subsample excluding observations with the largest DFBetas.

estimate equation (1). It is important to note that only those unobserved factors that change the development trends in 1861 could potentially be driving the results of the OLS estimation of equation (1). Although we deem the existence of such factors to be unlikely, they are not impossible given the change in the geopolitical equilibrium following Russia's defeat in the Crimean War. To address potential endogeneity, we take the historical distribution of the share of serfs in the rural population that belonged to the church across provinces and districts before their nationalization as a source of exogenous variation in the share of serfs in 1858. In order to avoid a conflict between the crown and the church, monasterial lands nationalized by the state were less likely to be subsequently redistributed to gentry than other state lands (Semevskij 1906) and, therefore, peasants who lived on these lands were less likely to become private serfs after the nationalization of church property. Figure 4 illustrates that the prevalence of the monasterial serfs before their nationalization is a good predictor of the share of private serfs prior to the emancipation at the province level; it presents the conditional scatter plot between the share of former monasterial serfs (which is denoted by $MonastShare_i$) and the share of serfs in 1858 conditional on log distance from Moscow and land suitability across provinces.[35]

We instrument $ShareSerfs_i \times PostEmancipation_t$ with $MonastShare_i \times PostEmancipation_t$. This instrument is excludable because the distribution of church lands a century before the emancipation was orthogonal to the changes in economic fundamentals around emancipation conditional on the distance from Moscow. Monasteries accumulated the vast majority of their land before the institution of serfdom (Vodarskij 1988). With serfdom, peasants living on monasterial lands became monasterial serfs. As described in online Appendix Section A4, before their nationalization, monasterial serfs did not differ systematically from other private serfs (e.g., Zakharova 1982).

In order to disentangle the effect of the two components of the abolition of serfdom, namely, the emancipation, which gave personal freedom to serfs, and the land reform, which gave them communal land titles, we include in the list of covariates our proxy for the share of former serfs who signed buyout contracts in the rural population in a particular year. This exercise can only be done for grain productivity because of the high frequency of the data for this outcome. As the land reform implementation was endogenous, to estimate the causal effect of land reform we instrument the share of peasants (former serfs) who signed buyout contracts in this province up to this year among the provincial rural population with a synthetic variable which predicts the progress of land reform based on the pre-reform indebtedness of estates in a province. In particular, to construct the predicted land reform variable, we assume that landlords without debts initiated the signature of buyout contract immediately after the emancipation in 1862; whereas, the number of landlords with debts, who launched the land reform, grew linearly between 1862 and 1882. This instrument reflects the fact that the indebted landlords had incentives to postpone buyout operations because lease payments were higher than the interest on loans.[36]

[35] Similarly, online Appendix Figure A4 illustrates the negative relationship between the share of serfs in 1858 and the share of nationalized monasterial serfs across districts. Panel A presents the scatter plot on the full sample of districts and panel B shows that this relationship is not driven by outliers as it restricts the sample to districts with the share of monasterial serfs below 30 percent.

[36] See online Appendix Section A7 for details.

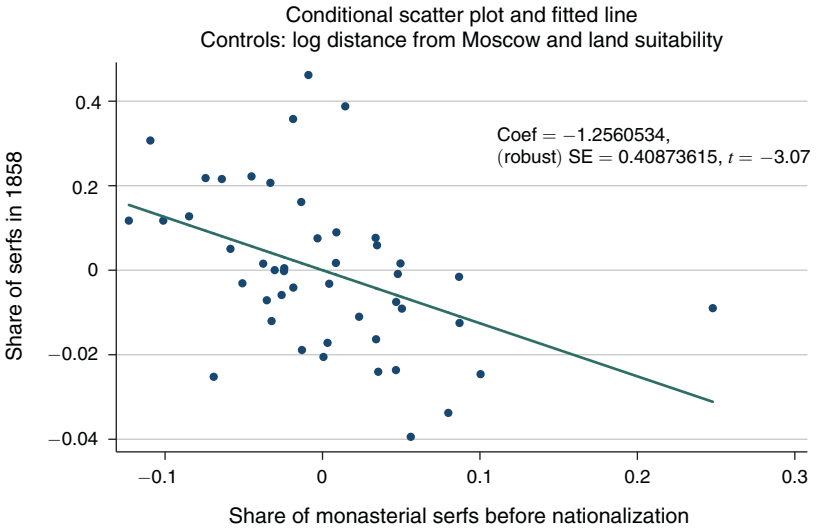

FIGURE 4. MONASTERIAL SERFS BEFORE NATIONALIZATION AND PRIVATE SERFS IN 1858 ACROSS PROVINCES

Thus, we construct the instrumental variables (IV) for the land reform as an interpolation between (1 − indebtedness) and 1 in the interval 1862–1882, 0 before 1862, 1 after 1882. We measure indebtedness as the ratio of serfs in the province used as collateral in landlords' debt contracts in 1858 to the total rural population in the province. For western provinces, the IV switches from 0 to 1 in 1863 because of changes in the land reform rules for these provinces as a result of the Polish revolt.

To illustrate how well this instrument predicts the progress of the reforms, we take a snapshot in 1872, i.e., halfway through the land reform implementation and plot in Figure 5 the cross-sectional association between the share of peasants who signed buyout contracts and the predicted land reform progress in 1872. The results of the first-stage estimations are presented in the next section alongside the results of the second stage. Historical sources suggest that this instrument is excludable because the primary reason to obtain loans for the gentry was status consumption rather than productive investments, and the primary reason for the state banks to grant loans was political (Gur'ev 1904; Korf 1906; Borovoj 1958). We describe in detail the historical evidence in favor of the excludability of this instrument in online Appendix Section A7. A quote from the government's committee on gentry's loans concluded in 1856 that "the amount of loans in a province did not depend on its economic prospects" (cited in Borovoj 1958, p. 204). The number of landlords who used loans to invest in their farms was negligible (Borovoj 1958). For example, only 1 percent of 8,500 landlords in Ryazan and Tambov provinces invested in "modernization" of their estates and only a small subset of them used loans (Koval'chenko 1959). These two provinces were from the region specializing in grain production, where landlords predominantly chose to run their own farms on corvée. To corroborate this anecdotal evidence, Figure A5 in the online Appendix shows that the gentry's indebtedness in 1858 was uncorrelated with either the level of grain productivity in 1858, or the change in grain productivity between 1858 and 1853, or the size of landlords' farms (measured by the number of peasants on corvée) in a province.

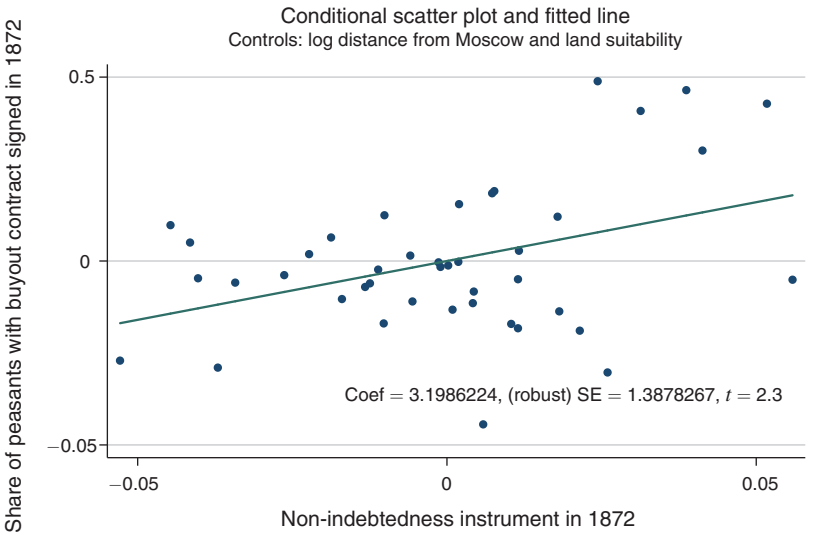

FIGURE 5. THE PROGRESS OF LAND REFORM AND THE LAND REFORM INSTRUMENT IN 1872, I.E., HALFWAY THROUGH LAND REFORM IMPLEMENTATION, ACROSS PROVINCES

V. Main Results: The Effects of the Abolition of Serfdom

A. *Productivity of Russian Agriculture*

Table 2 presents the estimated effect of the abolition of serfdom on the productivity of Russian agriculture. The results yield strong and robust evidence of a substantial positive effect of the abolition of serfdom on grain productivity. Panel A presents the results of the panel data estimation, panel B presents the corresponding first stages, and panel C presents the results of cross-sectional estimation. The first column of panel A presents the results of the most basic OLS specification with no additional covariates beyond province and year fixed effects. In column 2, we add controls for the (demeaned) distance from Moscow and crop suitability interacted with post-emancipation dummy and province-specific linear trends. In column 3, we instrument our main explanatory variable with the share of nationalized monasterial serfs interacted with post-emancipation dummy. The first stage of the two-stage least squares (2SLS) specification is presented in panel B of the table just below the second stage results. The instrument is a strong predictor of the interaction between the share of serfs and post-emancipation dummy with F-statistic above 18. In column 4, to the OLS specification we add controls for the reforms for state and royal peasants: the shares of state and royal peasants interacted with the onset of their respective reforms.[37] In column 5, we estimate the effect separately for the first decade after the 1861 reform and for the reminder of the nineteenth century.

[37] As the instrument predicts the variation in the prevalence of serfs versus state peasants across provinces, we cannot use IV once we control for the share of state peasants interacted with the post-1866 dummy because this control is highly correlated with the interaction of the share of state peasants with the post-1861 dummy, predicted by the instrument.

TABLE 2—THE EFFECT OF THE ABOLITION OF SERFDOM ON PRODUCTIVITY IN AGRICULTURE

Panel A. Panel data estimation

	Grain productivity						
Model:	OLS (1)	OLS (2)	IV, second stage (3)	OLS (4)	OLS (5)	OLS (6)	IV, second stage (7)
Share of serfs × Post-emancipation	0.81 [0.23]	0.80 [0.25]	1.29 [0.46]	1.04 [0.25]		1.03 [0.34]	2.76 [0.60]
Share of serfs × 1861–1870					0.75 [0.24]		
Share of serfs × post-1871					0.98 [0.38]		
Share of peasants with signed buyout contracts						−0.40 [0.25]	−1.20 [0.32]
Demeaned log distance to Moscow × Post-emancipation		−0.93 [0.36]	−0.58 [0.42]	−0.86 [0.36]	−0.93 [0.36]	−0.63 [0.42]	0.61 [0.47]
Demeaned crop suitability × Post-emancipation		0.07 [0.04]	0.06 [0.04]	0.06 [0.04]	0.07 [0.04]	0.06 [0.04]	0.06 [0.05]
Year and province fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province-specific trends	No	Yes	Yes	Yes	Yes	Yes	Yes
State and royal peasant reforms	No	No	No	Yes	No	No	No
Observations	1,835	1,835	1,835	1,835	1,835	1,780	1,780
R^2	0.368	0.403	0.533	0.404	0.403	0.402	0.539

Panel B. First stages of the corresponding 2SLS panel regressions

	Share of serfs × Post-emancipation	Share of serfs × Post-emancipation	Share of peasants with signed buyout contracts
Model:	IV, first stage (3)	IV, first stage (7.1)	IV, first stage (7.2)
Share of nationalized monasterial serfs × Post-emancipation	−1.25 [0.30]	−1.29 [0.30]	−1.34 [0.28]
Interpolation between (1-indebtedness) and 1 in the interval 1862–1882		0.12 [0.17]	2.70 [0.26]
Controls as in respective column of panel A	Yes	Yes	Yes
Observations	1,835	1,780	1,780
F, monasterial serfs instrument	18.15	18.87	23.90
F, indebtedness instrument		0.512	111.6

Panel C. Cross-sectional estimation robust to spatial correlation

	The change in detrended grain productivity between pre- and post-emancipation	
Model:	OLS spatial HAC	OLS spatial HAC
Sample:	full (1)	$\|DFBeta\| < 0.3$ (2)
Share of serfs	0.90 [0.26]	0.76 [0.23]
log distance to Moscow, crop suitability	Yes	Yes
Observations	46	43
Adj. R^2	0.257	0.332

Notes: In panels A and B, standard errors are clustered by province separately before and after the 1861 emancipation reform. In panel C, standard errors are adjusted to spatial correlation within 900 km. Post-emancipation is a dummy, which is switched on in 1861. (1861–1870) and post-1871 are dummies equal to 1 in the corresponding years and 0 otherwise. Share of peasants with signed buyout contracts equals 0 in all provinces for the years before 1862 and then gradually reaches the share of serfs in the corresponding province. In the non-western provinces this happened by 1882, and in western provinces there is a discrete jump in this variable to the share of serfs in 1863. Indebtedness is the ratio of serfs in the province used as collateral in landlords' debt contracts in 1858 to the total rural population in the province.

In all specifications, we find positive and statistically significant estimates of the effect of the abolition of serfdom, estimated by the coefficient on the interaction term between the share of serfs and post-emancipation dummies. The magnitude of the coefficient of interest is somewhat larger in the IV specification compared to the OLS specifications, although the point estimates of the OLS specifications are well within the confidence interval for the IV point estimate. This difference in magnitude is probably due to an inherent measurement error bias in OLS estimates, as we measure the prevalence of serfdom at one point in time, in 1858, whereas the share of (former) serfs year-to-year differs, for example, as a result of idiosyncratic shocks to mortality due to infectious diseases. The difference in the magnitudes of the coefficients presented in column 5 suggests that three-quarters of the overall effect on grain productivity for the nineteenth century was realized in the first decade after the emancipation. These results provide only a partial support for the claims of historians that the realization of the positive effects of the emancipation was very slow because of the slow institutional adjustments and associated transaction costs (Gerschenkron 1965; Nifontov 1974). In panel C of Table 2, we report cross-sectional results with standard errors corrected for spatial correlation. Column 1 presents results for the full sample of 46 provinces and column 2 for a subsample excluding the most influential observations. Again, we find a strong and significant correlation between the change in detrended grain productivity between the pre- and post-emancipation periods and the share of serfs by province, suggesting that the presence of spatial correlation in residuals is not driving our results.[38]

The difference-in-differences estimates show the average change in the difference in trends between provinces with large and small pre-emancipation share of serfs as a result of the emancipation and are not informative of the absolute level of the change in the trend in either group of provinces. In Figure 6, we present the summary of raw data around emancipation to shed light on the changes in absolute levels of grain productivity that gave rise to our estimates. The figure portrays the level of grain productivity around the emancipation (smoothed by taking averages by decade) separately in three subsamples defined by terciles of the pre-emancipation shares of serfs. In the first tercile, the share of serfs ranges from 0.08 percent to 40.3 percent with the mean of 17 percent; in the second tercile, it is between 41.2 percent and 57.2 percent with the mean of 50 percent; and in the third tercile, the range of the share of serfs is from 59.5 percent to 83.3 percent with the mean of 69 percent.

Several facts, important for the interpretation of the results, become apparent from this figure. First, before the emancipation, provinces with a larger share of serfs lagged behind provinces with a smaller share of serfs in terms of grain productivity. This gap partially closed with the emancipation. Second, productivity rose in provinces in the second and third terciles after the emancipation. In contrast, the productivity in provinces of the first tercile fell during the first two decades after the emancipation and recovered in the third decade. This fall could be a result of confounding negative macro shocks (for instance, climate shocks), which, in provinces that had a lot of serfs, were offset by the positive dynamics as a result of the reform.

[38] Influential observations are defined as having an absolute value of DFBeta greater or equal to 0.3. Online Appendix Figure A6 illustrates the cross-sectional relationship by presenting a conditional scatter plot on the full sample with an indication of DFBeta for each observation.

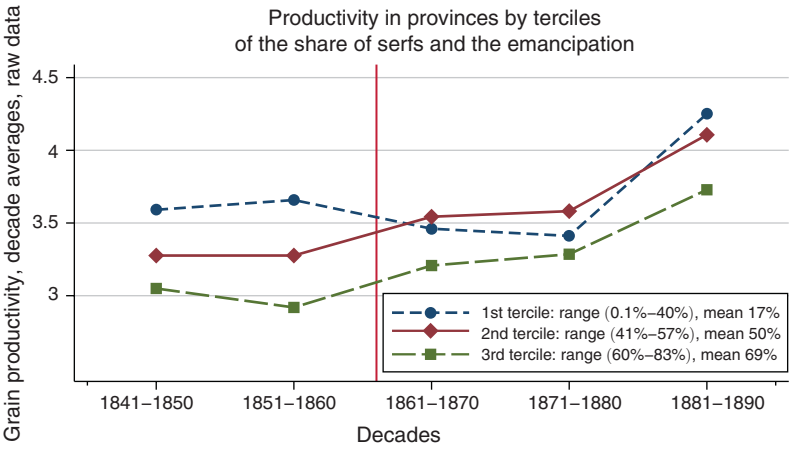

FIGURE 6. GRAIN PRODUCTIVITY IN THE THREE TERCILES OF THE SHARE OF SERFS IN 1800–1900

Notes: The series represent decade averages separately for 16 provinces in the first tercile of the pre-emancipation share of serfs, 15 provinces in the second tercile, and 15 provinces in the third tercile of the share of serfs. The vertical line indicates the 1861 emancipation reform.

It also could be due to a general-equilibrium negative causal effect of the emancipation on provinces that relied on free labor in agriculture before the emancipation. It is possible that an increase in productivity in the high-serfdom provinces as a result of the emancipation led to an increase in the demand for variable nonlabor production inputs, such as fertilizers or agricultural machines, which could have led to an increase in the price of these inputs. Such an increase in input prices, in turn, could have had a negative effect on the productivity in the provinces, which were more productive to start with. One should note, however, that nonlabor nonland inputs played a minor role in agricultural production at that time, so it is unlikely that the changes in prices of these inputs had a big effect on productivity.

We proceed to testing the main identifying assumption of the difference-in-differences approach, i.e., whether there are diverging pre-trends in agricultural productivity among provinces with high and low prevalence of serfdom. We estimate the coefficients of 11 interaction terms of the share of serfs in 1858 with dummies indicating five-year intervals, including 3 before the emancipation (leaving 1795–1829 period as the comparison group).[39] In this specification, we include the same controls as in column 4 of Table 2 with one important difference: instead of 46 province-specific trends, we control for 14 region-specific trends, each of which groups together several provinces that are commonly considered to have similar development trajectories.[40] This change is necessary, as the addition of 11 interaction terms into this specification makes the use of 46 province-specific trends too demanding.

[39] We use a decade dummy for the 1840s rather than two five-years dummies because of data availability.

[40] We provide the precise lists of the regions and of the provinces they are comprised of in online Appendix Section B.

Figure 7 visually represents the results by plotting the coefficients on these interactions along with their 90 percent confidence intervals by time period.[41] The results indicate the absence of pre-trends, as there are no significant effects before the emancipation reform.[42] The figure also illustrates how the magnitude of the effect evolved over time. The effect during the immediate aftermath of the emancipation is positive, but not statistically significant. Grain productivity in the provinces with emancipated serfs rose continuously (relative to the dynamics of grain productivity in provinces with free labor) throughout the 1870s. After 1881, the difference between provinces with high and low share of serfs became substantially smaller (but coefficients remain positive and jointly statistically significant). Below, after the discussion of the magnitude of the effect, we investigate the reason for this partial setback.

To present the magnitude, we rely on the estimate from column 3 of panel A, which, unlike the raw data presented in Figure 6, factors out province-specific trends and differences in productivity due to distance from Moscow and land suitability and uses IV. An increase in the share of serfs from 17 percent to 69 percent (i.e., the mean values in the bottom and top terciles, equivalent to an increase of two standard deviations of the share of serfs) led to an additional increase in grain productivity after the emancipation of 0.67 above the trend or an increase of 19.2 percent from the mean 1858 level. [43]

These are large effects, as compared to the aggregate trend in grain productivity, which, on average, increased by 4 percent per decade in the nineteenth century. However, these effects are relatively small compared to the level of volatility in the economy.[44]

In columns 6 and 7 of Table 2, we attempt to disentangle the effects of the two components of the abolition of serfdom on agricultural productivity: the emancipation

[41] Column 1 of Table A3 in the online Appendix presents the entire regression output.

[42] Grain productivity insignificantly decreased in provinces with a large share of serfs right before the emancipation compared to 1840s or the comparison years, 1795–1829. This could possibly be due to disorganization in an anticipation of the reform.

[43] The coefficient on the distance from Moscow interacted with post-emancipation dummy is negative and in some specifications significant, thus the described magnitudes refer to provinces with the mean log distance from Moscow. Online Appendix Table A4 reports the results of a regression in which we replace the distance from Moscow interacted with the post-emancipation dummy by the triple interaction between the share of serfs, the distance from Moscow, and the post-emancipation dummy. (The interaction of the distance from Moscow with the post-emancipation dummy is excluded from this specification because of multicollinearity with this triple interaction term due to high correlation between the share of serfs and distance from Moscow.) The point estimate of the coefficient on the triple interaction term is negative and statistically significant at the 10 percent level, suggesting that the closer the province to Moscow, the larger the effect of the abolition of serfdom. This is not surprising, as the proximity to Moscow also meant proximity to the largest markets and to market infrastructure. The magnitude of the estimated coefficients implies that in the most remote provinces of our sample, the effect of the abolition of serfdom was positive, but much smaller than the average. We also check that our results are not driven by productivity in regions where agriculture played a relatively limited role by reproducing panel regressions on grain productivity with weights by the logarithm of grain output. Online Appendix Table A5 presents the results. The point estimates are very similar to unweighted regressions. A smaller number of observations in Table A5 is due to the fact that for several years we have data only on the yield-to-seed ratio, and not on the yield output.

[44] As shown in Figure 1, productivity jumped up and down by about 1 point from one year to the next. This volatility may explain why there is no vivid jump in productivity in the first decade following the emancipation in Figure 1, which is evident from Figure 7. As we argue above, difference-in-differences methodology allows drawing inference only about the difference in the effects of the reform in provinces with high and low prevalence of serfdom and not about the level of the effect in either of these groups of provinces, which theoretically could be negative in provinces that always relied on free labor. Such possible negative effect may also be a reason why one does not see a jump on Figure 1 during the first decade after the emancipation.

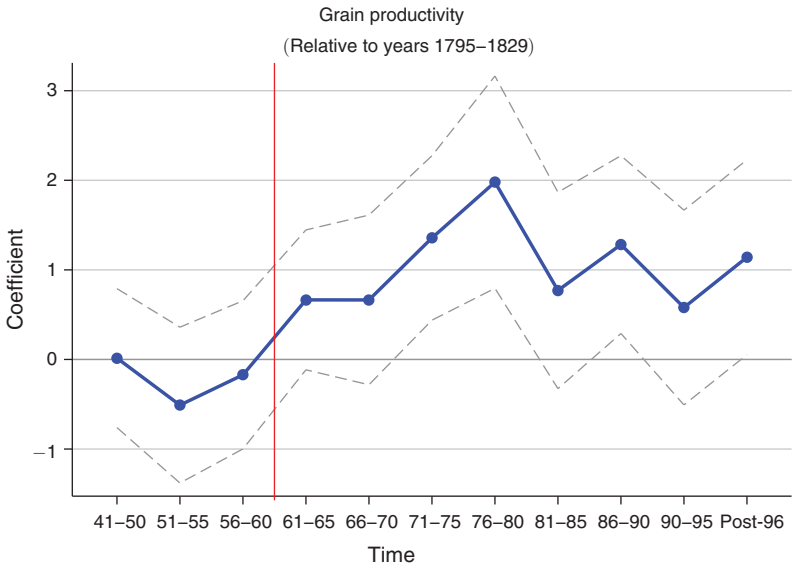

FIGURE 7. THE TIME-VARYING EFFECT OF EMANCIPATION: GRAIN PRODUCTIVITY

Notes: The number of cross sections within five-year intervals varies because of missing data for 1867–1869 and 1877–1882. The figure presents coefficients (along with their 90 percent confidence interval) in the regression of grain productivity on five-year interval dummies interacted with the share of serfs in a province, province and year fixed effects, region-specific linear trends, and controls for demeaned suitability interacted with the post-emancipation dummy, and demeaned distance from Moscow interacted with the post-emancipation dummy, the share of state peasants interacted with the post-1866 dummy, and the share of royal peasants interacted with the post-1859 dummy. Four cross sections between 1795 and 1829 are held as the comparison group. The vertical line marks the timing of the emancipation. The table-form representation of the results of this estimation is presented in column 1 of Table A2 in the online Appendix.

itself and the subsequent land reform. In particular, we add our proxy for the share of peasants (former serfs) who had signed buyout contracts in this province up to this year among the provincial rural population to the list of covariates. In this specification, the coefficient on the interaction between the share of serfs and the post-emancipation dummy estimates the effect of the emancipation, and the coefficient on the share of peasants who signed buyouts contracts estimates the effect of the land reform. Column 6 presents OLS estimates and column 7 presents IV estimates. In the 2SLS estimation, we instrument both the emancipation (as above, with the share of nationalized monasterial serfs) and the land reform. The instrument for the land reform, as described in the methodology section, is the linear interpolation between (1 − indebtedness) at the beginning of the land reform (in 1862) and one at the end of the land reform (in 1882). Panel B presents the results of the first-stage regressions below the second stage: both instruments are strong predictors of the respective endogenous regressors (F-statistics for the excluded instruments are reported at the bottom of the table). Both in OLS and IV specifications, we find that the effect of the emancipation on productivity in agriculture is positive and statistically significant. The effect of the land reform is negative in OLS and IV specifications, but it is statistically significant only in IV. The IV point estimates are much larger in magnitude, which points to the a priori plausible endogeneity of the implementation of the land reform. According to IV estimates, the effect of the

emancipation per se is 2.1 times as large as the total overall effect of the abolition of serfdom (column 7 versus column 3).

IV estimates imply that the emancipation without the inefficiencies of the land reform would have led to an additional increase in grain productivity in the second half of the nineteenth century of 1.44 points (41 percent of mean 1858 level) in the provinces in the bottom compared to the top tercile of the share of serfs. However, the land reform reduced this difference by 0.62 points (column 7). Thus, IV estimates imply that the land reform substantially slowed down post-emancipation growth in agricultural productivity among provinces that relied on serfs' labor pre-emancipation: only 56.5 percent of the emancipation reform's potential was realized due to inefficiencies of the land reform.[45] Importantly, these IV estimates are valid only if the assumption of the excludability of the synthetic indebtedness instrument for the land reform is valid. We provide anecdotal evidence in the online Appendix in support of this assumption, but it cannot be verified directly.

The Mechanisms.—Gerschenkron (1965) has argued that the land reform negatively affected Russian agricultural development by empowering the peasant repartition commune, where land was redistributed among households, in contrast to the hereditary commune in which households had perpetual usage rights of specific land plots. In column 1 of Table 3, we test this conjecture and find empirical support for it. We include the interaction between the land reform proxy and the dummy for the repartition commune to the specification, presented in column 6 of panel A in Table 2. We find that the average negative effect of the land reform is entirely due to the negative effect of land reform under the repartition commune. The effect of the land reform in the hereditary commune is positive albeit not statistically significant and the effect of the land reform in the repartition commune is negative and significant. The difference between the effects of the land reform in provinces with the two types of communes is estimated by the coefficient on the interaction between the share of peasants with signed buyout contracts and the repartition commune dummy. We run an OLS specification only because we do have a credible instrument for the repartition commune dummy. Judging by the analysis presented above, the OLS estimates underestimate the negative effect of the land reform. An additional source of bias could arise from the endogeneity of the repartition communes if it had a direct nonlinear effect on dynamics of agricultural productivity at the time of the reform, which is unlikely, but not impossible. Thus, these results should be interpreted with caution. If taken at face value, these results suggest that the inefficient repartition commune was the reason for the setback in the reform progress after 1882, the year when the land reform was completed.

What was the mechanism behind the positive effect of the emancipation reform? As we have shown above, three-quarters of the overall effect of the abolition of serfdom on grain productivity were realized during the first decade after the emancipation. This may seem puzzling because the reform implementation took time and many aspects of the reform, once implemented, arguably are expected to have a sluggish effect on agricultural productivity. For example, an increase in human

[45] The 56.5 percent figure comes from the following calculation using point estimates presented in column 7 of Table 2: (2.76–1.20)/2.76.

Table 3—The Mechanisms behind the Effects of the Land Reform and the Emancipation

Dependent variable:	Grain productivity		Share of winter crops seeded at $t-1$ in total winter and spring crops seeded at $[t-1; t]$ production cycle				
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)	OLS (7)
Share of serfs × Post-emancipation	0.833	1.734	−0.064	−0.050	−0.072	−0.042	−0.076
	[0.331]	[0.429]	[0.016]	[0.016]	[0.017]	[0.021]	[0.019]
Share of peasants with signed buyout contracts	0.109	−0.499					
	[0.267]	[0.254]					
Share of peasants with signed buyout contract × repartition commune	−0.697						
	[0.337]						
Share of serfs × Post-emancipation × Implicit contracts		−1.561					
		[0.532]					
Demeaned temperature $(t-1)$			0.005		0.003		0.005
			[0.003]		[0.003]		[0.003]
Share of serfs × Post-emancipation × Demeaned temperature $(t-1)$			0.010		0.008		0.013
			[0.004]		[0.004]		[0.005]
Share of serfs × Post-emancipation × Demeaned rye-to-wheat world price ratio $(t-1)$				−0.495	−0.441		
				[0.120]	[0.119]		
Share of serfs × Post-emancipation × Demeaned rye-to-oat local price ratio $(t-1)$						−0.100	−0.114
						[0.042]	[0.042]
Share of serfs × Demeaned rye-to-oat local price ratio $(t-1)$						−0.012	0.009
						[0.047]	[0.043]
Demeaned rye-to-oat local price ratio $(t-1)$						0.030	0.021
						[0.022]	[0.019]
Demeaned log distance to Moscow × Post-emancipation	−0.920	−0.788	−0.030	0.024	−0.020	0.020	−0.044
	[0.438]	[0.420]	[0.020]	[0.017]	[0.020]	[0.020]	[0.023]
Demeaned crop suitability × Post-emancipation	0.045	0.037	0.001	−0.001	0.000	−0.001	0.002
	[0.039]	[0.035]	[0.002]	[0.002]	[0.002]	[0.003]	[0.002]
Year and province fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,780	1,726	792	796	792	755	751
R^2	0.403	0.420	0.792	0.794	0.802	0.783	0.795

Notes: Standard errors are clustered by province separately before and after the 1861 emancipation reform. Post-emancipation is a dummy, which is switched on in 1861.

capital investment (as a result of granting personal freedom to serfs) and an increase in investment in land in hereditary communes (as a result of the change in property rights) could have had an effect on productivity only with a considerable lag. It is also possible, although historians argue against it, that productivity increased because of an increase in capital inputs after the reform (as a result of the acquisition of agricultural machinery) and because of new technologies (i.e., a shift to more productive seed varieties).[46] These changes also could not have happened fast.

[46] There is a consensus in the historical literature that no improvements in agricultural capital, i.e., tools and machines, occurred until the end of the nineteenth century (e.g., Nifontov 1974). Mironov (2010, p. 557) shows that the number of working days in an average peasant household decreased after the abolition of serfdom because of the improved efficiency. Strumilin (1966) shows that the time (in working days) needed to cultivate a unit of land was approximately constant between the 1850s and 1890s (see online Appendix Section A10 for details). It is also theoretically possible that land input increased because of virgin lands exploration after the emancipation, which could have had an effect on productivity if the new lands were more productive. We test and reject this mechanism. We have collected data on cultivated land for four cross sections, two before and two after the emancipation, and used the logarithm of cultivated land as an outcome variable estimating equation (1) with region-specific trends. We find that the abolition of serfdom did not affect cultivated areas. The results of the OLS and IV regressions are presented in online Appendix Table A6. The coefficients on the interaction of the share of serfs with the post-emancipation dummy are not significantly different from zero irrespective of specification.

One important change that did occur right after the emancipation was the cessation of the ratchet effect, as the law fixed the level of peasants' obligations for all (former) serfs (*Russian Empire* 1863, vol. 36, part 1). This change could have had a positive effect on peasants' incentives right at the time when they became residual claimants of the proceeds of their labor, provided that serf owners were not able to commit to a fixed level of serfs' obligations under serfdom. Importantly, many contemporaries believed that an increase in peasant effort and care was what was needed to boost agricultural productivity.[47] Therefore, one could expect a relatively fast positive effect of the emancipation on peasants' effort and, consequently, productivity, if serfdom was subject to a ratchet effect. This hypothesis is not testable directly because there are no data on peasant effort. We test it indirectly.

First, if peasant incentives were the main driver of the productivity improvements following the emancipation, in estates where serfs faced high-powered incentive schemes designed by landlords under serfdom, we expect to see no gains in productivity after the emancipation. As described above, it was easier to reach an agreement about the level of former serfs' obligations during the transition period on estates where the obligations were fixed de facto before emancipation by an implicit long-term contract. We use the share of serfs who agreed to sign regulatory charters as a proxy for the presence of such implicit contracts. Column 2 of Table 3 presents the results of the estimation of the differential effect of emancipation on productivity, depending on the share of serfs with long-term implicit contracts by province. We operationalize this test by adding an interaction of the share of serfs with signed regulatory charters (i.e., agreed to the proposed terms of the fixed land lease payments in the interim period before the signature of buyout contract) with the share of serfs post-emancipation to our main specification. As above, we run only OLS regressions because we do not have a credible instrument for the use of implicit contracts under serfdom, which potentially could be a problem if there is a reason unrelated to serfdom for a change in productivity trends in 1861 in provinces where landlords committed to a long-term implicit contract vis-à-vis their serfs. As expected, we find that implicit contracts under serfdom significantly decreased productivity improvements as a result of the emancipation, as can be seen from the negative significant coefficient on the triple interaction between the share of serfs with implicit contracts, the share of serfs, and the post-emancipation dummy. The share of serfs under implicit long-term contracts varies across provinces from 2 percent to 85 percent, with the median province at 43 percent. A one standard deviation increase in the share of serfs with implicit contracts (equal to 21 percentage points) increased the effect of the reform on grain productivity by 26.5 percent. The total effect of the abolition of serfdom (taking into account the countervailing effects of the emancipation and the land reform) was positive and

[47] Agricultural handbooks from the first half of the nineteenth century (e.g., Mordvin 1839; Usov 1840; Dmitriev 1844; Ungern-Shterenberg 1848) discuss ways of increasing agricultural productivity readily available at that time. Some of these improvements were as sophisticated as new seed varieties and the introduction of multiple-field crop rotation, others as simple as a change in the timing and the order of existing agricultural operations. These handbooks explicitly name the lack of incentives to exert effort on the part of serfs and landlords' monitoring problems as the main explanations for low agricultural productivity. Mordvin (1839) singled out 15 reasons for poor harvests; 6 of them were directly related to serfs' low effort. Ignatovich (1925, p. 160) studied the contemporaries' assessment of productivity under serfdom and concluded that serfs did not exert effort without constant monitoring, and with monitoring they spent 25 to 30 percent more time to carry out any particular task compared to free labor.

statistically significant in provinces where the share of serfs subject to implicit long-term contracts with landlords was below 50.5 percent, in other provinces, it was positive and insignificant with one exception: in one province, where the share of serfs with implicit contracts attained its maximum, the overall effect of the reform was negative and insignificant.

Second, we can observe whether peasants made adjustments to the choice of which crops to seed, which to sell, and which to consume depending on the climatic and market conditions. As effort and care are needed to make such adjustments, we expect peasants to choose more appropriate crops for cultivation with regard to climatic and market conditions after the emancipation. Due to the technology that prevailed at the time, each plot was divided roughly into three parts: for winter grains, spring grains, and fallow. The peasants could change the relative sizes of the three parts depending on what made more sense in terms of climatic and price shocks. In particular, colder temperatures were associated with higher failure of winter crops relative to spring crops and, therefore, warmer years, on average, were associated with higher shares of winter grains in total amount of crops seeded. To harvest in the summer of year t, the winter crops were seeded in the fall of year $t-1$ and the spring crops in the spring of year t. The decision of how much to allocate to winter versus spring grains was taken in the fall of the year $t-1$ (when the winter crops were seeded).

Market conditions also mattered for the choice of what shares of each type of crop to seed. Since price fluctuations allowed at most an imperfect forecast of the relative price of winter to spring crops for the next season, it was rational to sell a larger share of more expensive crops after the harvest (in the summer and fall) and allocate to seeds and to private consumption a larger share of the less expensive crops. These choices started to have an effect on peasants' well-being only after the emancipation, when they became residual claimants on their harvest. Thus, we should expect the share of winter crops in the total of crops seeded to be more sensitive to climatic and market conditions after the emancipation if increased effort is the mechanism behind the effect of emancipation.

In column 3 of Table 3, we regress the share of winter crops seeded in total seed on the last year's temperature and its interaction with the share of serfs post-emancipation. We find that, on average, the share of winter crops was lower during cold shocks and that this relationship became significantly stronger for the emancipated serfs after 1861. In columns 4 and 6, we explore the choice between the winter and spring crops to be seeded depending on their relative price. In column 4, we use time series of the price of rye relative to wheat in Dutch commodity exchange in Amsterdam, which we interact with the share of serfs and post-emancipation dummy. In column 6, we use the price of rye relative to oats in Russian regions, again interacted with the share of serfs and the post-emancipation dummy. Price of rye relative to oats varies both across space and over time and, therefore, in addition to the triple interaction term, we include the relative price itself and its double interaction with the share of serfs in the list of covariates in column 6. As expected, we find that the emancipated serfs sold a larger share of the more expensive crops (leaving cheaper crops for seeds and own consumption): an increase in the price of rye, the main winter crop, relative to spring crops made peasants seed a relatively lower share of winter rye, because they sold a larger share

of it on the market.[48] In columns 5 and 7, we combine the climate and market conditions and get the same result.[49]

To sum up, we find suggestive evidence that an increase in peasants' incentives was an important mechanism through which the emancipation boosted agricultural productivity.

B. *Industrial Development*

In Table 4, we estimate the effect of the abolition of serfdom on log industrial output. The table presents the results of estimating the same specifications as the first four columns of Table 2 but with log industrial output as dependent variable. The only difference between this analysis and the analysis of grain productivity presented above is that the time dimension of the data for industrial output is substantially shorter (eight snapshots) and, as a consequence, we do not have enough statistical power to control for trends specific to each province; thus, instead we control for the 14 region-specific trends. We find a positive and statistically significant effect of the abolition of serfdom on industrial output in all specifications (i.e., in OLS with and without controls for region-specific trends and with and without controls for state and royal peasants, as presented in columns 1, 2, and 4 of panel A of Table 4 and in IV, as presented in column 3 of the same panel). Panel B presents the first stage, which is sufficiently strong. Panel C presents the cross-sectional relationship between the change in detrended log industrial output between the pre- and post-emancipation periods and the prevalence of serfdom across provinces with an adjustment for spatial correlation on the full sample and excluding the most influential observations. Again, we find that the results are robust.[50] As we illustrate in online Appendix Figure A7, which plots raw data, industrial output was uncorrelated with the share of serfs before the emancipation; and at the end of the nineteenth century it became positively correlated with the share of serfs across provinces. Output grew in all provinces throughout the nineteenth century, but after the emancipation it started growing faster in provinces where serfdom was more prevalent before.

As far as the magnitude of the estimated effect is concerned, in contrast to the results for grain productivity, there is a substantial difference in the size of point estimates of the effect of the abolition of serfdom on industrial output between OLS and IV specifications: 0.73 versus 2.6 (columns 2 and 3). This implies the following magnitudes: a one standard deviation increase in the share of serfs before the emancipation led to an additional increase in industrial output of 19 percent over the course of the second half of the nineteenth century according to the OLS specification and of 86 percent according to the IV specification. In an average province,

[48] In the second half of the eighteenth century, Russia became an important supplier of grain to European markets. Russia's export of grain was negligible in 1700. However, already by 1800, about one-quarter of all Russia's "marketed" grain (i.e., excluding grain consumed by producers) was sold abroad (Mironov 1985).

[49] Note that we do not combine these specifications with our measure of implicit contracts because these contracts could also regulate directly the shares of winter and spring crops. In addition, it is worth noting that the interaction between the temperature and the post-1861 dummy has zero effect on productivity and just adds noise to the estimation.

[50] Figure A8 in the online Appendix illustrates the cross-sectional relationship presented in Panel C of Table 4 with an indication of DFBeta for each observation.

Table 4—The Abolition of Serfdom and Industrial Development

Panel A. Panel data estimation

	ln(industrial output)			
	OLS (1)	OLS (2)	IV, second stage (3)	OLS (4)
Share of serfs	0.78	0.73	2.60	1.38
× Post-emancipation	[0.31]	[0.38]	[1.23]	[0.57]
Demeaned log distance from Moscow		0.36	1.70	0.52
× Post-emancipation		[0.44]	[1.01]	[0.44]
Demeaned crop suitability		0.13	0.13	0.12
× Post-emancipation		[0.06]	[0.07]	[0.06]
Year and province fixed effects	Yes	Yes	Yes	Yes
Region-specific trends	No	Yes	Yes	Yes
State and royal peasant reforms	No	No	No	Yes
Observations	347	347	347	347
R^2	0.800	0.885	0.934	0.887

Panel B. First stages of the corresponding 2SLS panel regressions

	Share of serfs × Post-emancipation
Model:	IV, first stage (3)
Share of nationalized monasterial serfs × Post-emancipation	−1.02
	[0.26]
Controls as in respective column of panel A	Yes
Observations	347
F, excluded instrument	15.42

Panel C. Cross-sectional estimation robust to spatial correlation

	The change in detrended log industrial output b/w pre- and post-emancipation	
Model:	OLS spatial HAC	
Sample:	Full (1)	|DFBeta| < 0.3 (2)
Share of serfs	1.90	2.02
	[0.38]	[0.40]
log distance from Moscow, crop suitability	Yes	Yes
Observations	45	41
Adj. R^2	0.273	0.349

Notes: In panels A and B, standard errors are clustered by province separately before and after the 1861 emancipation reform. In panel C, standard errors are adjusted to spatial correlation within 900 km. Post-emancipation is a dummy, which is switched on in 1861.

with 45 percent of serfs in rural population prior to the emancipation, industrial output increased by 39 percent above the trend according to the OLS specification and by a factor of 3.2 according to the IV specification. It is implausible that measurement error is the only explanation. The most likely reason for such a large difference between OLS and IV is the heterogeneous effect of the abolition of serfdom on industrial development. It is possible that the abolition of serfdom had different effects on industrial output in those provinces, where, in the absence of monasteries, the lands would have been transferred into private ownership, i.e., because of a high demand for land ("compliers"), and those provinces, where in the absence

of monasteries, the lands would have stayed in state ownership anyway because the gentry was not interested in owning land in these provinces ("always takers"). In that case, the OLS estimates the average treatment effect across all provinces, whereas IV estimates the local average treatment effect (LATE) across provinces where the instrument made a difference, i.e., compliers (Imbens and Angrist 1994). A possible reason for why the abolition of serfdom had differential effects across provinces on industrial output is that the reform affected industry mostly through labor market spillovers, which could only occur in places where peasants were tied to large landlords' farms.

The large magnitude of the effect on industrial development that we find is in line with findings on the substantial level of labor migration within provinces from villages into the provincial industrial sector in the late nineteenth century in spite of the constraints erected by the peasant commune (Borodkin, Granville, and Leonard 2008; Burds 1998; Crisp 1976; and Nafziger 2010). Figure 8 presents the estimates of the dynamics of the effect of the abolition of serfdom on industrial output (similarly to Figure 7); it confirms the absence of pre-trends, as the estimates for the years before the emancipation are small and statistically insignificant. Data limitations do not allow us to make any conclusions about the dynamics of the effect of the reform on industrial output during the first two decades after the emancipation.

A Back-of-the-Envelope Calculation of the Effect of the Reform on the Value Added.—If we assume that there was no negative general-equilibrium effect of the abolition of serfdom on provinces that relied on free labor before 1861 (which means that the decline of grain productivity observed in the 1860s and 1870s in provinces in the first tercile of share of serfs was due to an unfavorable external macro shock, such as a weather shock), we can use our estimates to calculate the effect of the reform on the total value added. Under this assumption, our estimates imply that the value added in agriculture increased by 16 percent and in industry by 37 percent.[51] The composition of the value added across sectors in the Russian Empire in 1860 was as follows: agriculture constituted 59.3 percent; industry, 5.1 percent; and the rest of the economy was comprised of services broadly defined (Goldsmith 1961). We have no data to calculate the effect of the abolition of serfdom on the service sector. We consider a market-based scenario, in which the service sector grew at the same rate as the rest of the economy on average, which is reasonable because the main driving force for the service sector growth was the increase in demand as the main contributors to this sector were trade and transport. Applying this sectoral composition, we get that an increase in Russia's GDP as a result of the abolition of serfdom in the second half of the nineteenth century amounted to 17.7 percent.[52]

[51] These numbers are calculated using the estimates from column 3 of Table 2 and column 2 of Table 4 and applying these effects to the average share of serfs in the European Russia, which amounted to 43 percent of rural population. $16 \approx 0.43 \times 1.29/3.5$, where 3.5 is the mean productivity in 1858. $37 \approx \exp(0.73 \times 0.43)$.

[52] We can calculate the lower bound for the effect of the reform on the value added by making the extremely conservative and most probably unreasonable assumption that the service sector was not affected by the abolition of serfdom. In that case, the increase in the total value added as a result of the reform would amount to 11.4 percent.

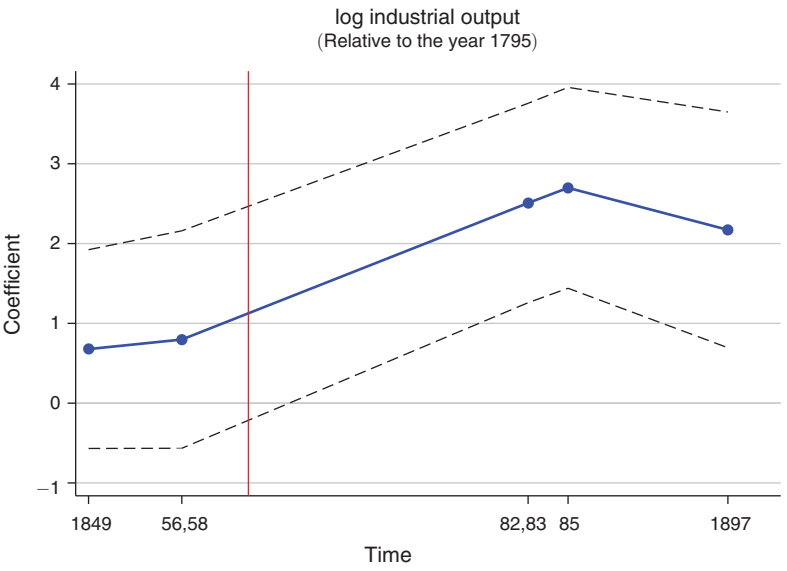

FIGURE 8. THE TIME-VARYING EFFECT OF EMANCIPATION: INDUSTRIAL OUTPUT

Notes: The figure presents coefficients (along with their 90 percent confidence interval) in the regression of log industrial output on interactions of the share of serfs in a province with 4 dummies for: 1849, 1856 and 1858, 1882 and 1883, and for 1885 and 1897, province and year fixed effects, region-specific linear trends, and controls for demeaned suitability interacted with the post-emancipation dummy, demeaned distance from Moscow interacted with the post-emancipation dummy, the share of state peasants interacted with the post-1866 dummy, and the share of royal peasants interacted with the post-1859 dummy. The year 1795 is held as the comparison group. The vertical line marks the timing of the emancipation. The table-form representation of the results of this estimation is presented in column 2 of Table A2 in the online Appendix.

C. *Peasants' Nutrition*

We proceed by estimating the effect of the abolition of serfdom on draftees' height as a measure of nutrition. As all cohorts of the 21-year-old draftees for whom we have the data have lived at least a part of their childhood after the emancipation, we start with estimating an event-study regression (equation (2)) at the province level, interacting the share of serfs separately with the dummies for each pair of two consecutive cohorts between 1855–1866, leaving cohorts born in 1853 and 1854 as the comparison group. As we discussed in the methodology section, one should expect to find an increasing trend in the estimated coefficients before the emancipation because older cohorts born before the emancipation spent more time during their childhood under serfdom. In addition, the positive trend could continue after the emancipation because the effects of the reform on nutrition are not immediate. This is exactly what we find. Figure 9 reports the results in a graphic form and column 3 of online Appendix Table A3 reports the full regression output. The coefficients on interactions with cohort dummies are all positive and increasing in magnitude. The largest increase in the coefficients is between the period immediately before and immediately after the emancipation. This is consistent with the findings of the health literature that nutrition in the early infancy has the most important effect on height during adulthood (Costa 2015). The continuing rising trend in coefficients for cohorts born after the emancipation is consistent with our findings on

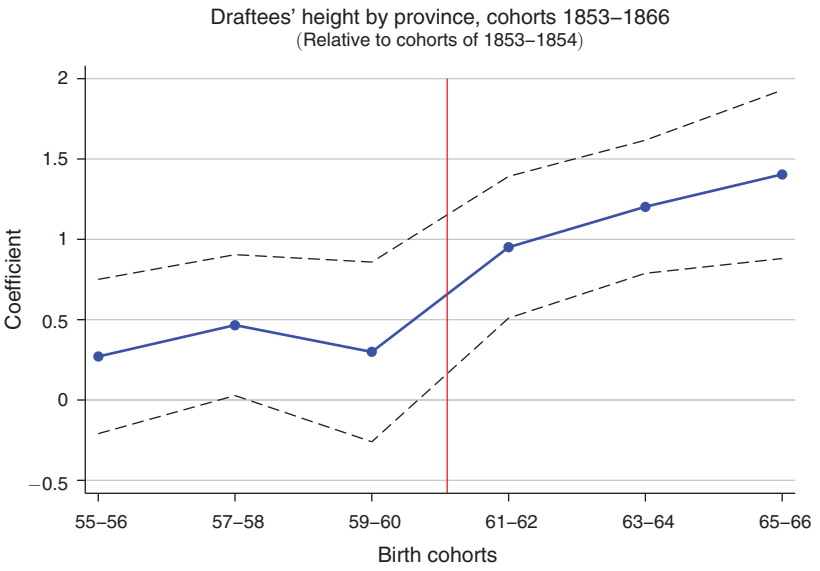

FIGURE 9. THE TIME-VARYING EFFECT OF EMANCIPATION: DRAFTEES' HEIGHT

Notes: The figure presents coefficients (along with their 90 percent confidence interval) in the regression of the height of draftees on two-year interval dummies for birth cohorts born around the emancipation interacted with the share of serfs in a province, province and birth-cohort fixed effects, and controls for demeaned suitability interacted with the post-emancipation dummy, and demeaned distance from Moscow interacted with the post-emancipation dummy. Two cohorts of 1853 and 1854 are held as the comparison group. The vertical line marks the timing of the emancipation. The table-form representation of the results of this estimation is presented in column 3 of Table A3 in the online Appendix.

the gradual effect of the abolition of serfdom on grain productivity during the first 20 years after the reform.[53]

Table 5 estimates the average difference in increases in height between cohorts that were born before and after the emancipation for provinces with high and low prevalence of serfdom. We find that this average difference is statistically significant. The structure of the table is similar to that of Tables 2 and 4. In particular, in panel A, we present the results of the panel estimations; panel B presents the first stages for the corresponding 2SLS regressions; and panel C presents the cross-sectional results for first differences with standard errors adjusted for spatial correlation in error terms with a cutoff at 900 km. In the table, we present results using both province- and district-level data.

Columns 1 to 3 of panel A present the province-level panel results and columns 4 to 6 the district-level results. In each sample, we present three specifications: the most basic one with district and birth-cohort fixed effects and without any additional controls and the baseline OLS and IV specifications with controls for the (demeaned) distance from Moscow and land suitability interacted with post-emancipation

[53] We also study the dynamics of the effect of the emancipation on height using district level data by estimating the same equation at district level. The results are similar as they also reveal an increasing pre-trend but are slightly less strong. We illustrate these results in online Appendix Figure A9 and present the full regression output in column 4 of Table A3.

Table 5—The Abolition of Serfdom and Peasant Living Standards: Draftees' Height

Panel A. Panel data estimation

	Draftees' height (cohorts 1853–1866) Province-level data			Draftees' height (cohorts 1853–1862) District-level data		
	OLS		IV, second stage	OLS		IV, second stage
	(1)	(2)	(3)	(4)	(5)	(6)
Share of serfs × Post-emancipation cohorts	0.75	0.92	0.78	0.41	0.65	0.82
	[0.16]	[0.13]	[0.32]	[0.14]	[0.16]	[0.49]
Demeaned log distance from Moscow		0.73	0.65		0.18	0.21
× Post-emancipation		[0.18]	[0.24]		[0.05]	[0.096]
Demeaned crop suitability × Post-emancipation		0.15	0.16		0.08	0.08
		[0.02]	[0.02]		[0.02]	[0.03]
Birth cohort and province or district fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Province-specific trends	No	No	No	No	No	No
Observations	584	584	584	4,437	4,437	4,357
R^2	0.114	0.217	0.853	0.559	0.561	0.730

Panel B. First stages of the corresponding 2SLS panel regressions

	Share of serfs × Post-emancipation cohorts IV, first stage (3)	Share of serfs × Post-emancipation cohorts IV, first stage (6)
Share of nationalized monasterial serfs	−1.29	−0.63
× Post-emancipation cohorts	[0.31]	[0.08]
Controls as in respective column of panel A	Yes	Yes
Observations	584	4,357
F, excluded instrument	17.32	72.12

Panel C. Cross-sectional estimation robust to spatial correlation

	The change in detrended height by province b/w pre- and post-emancipation cohorts OLS spatial HAC		The change in detrended height by district b/w pre- and post-emancipation cohorts OLS spatial HAC	
	full	$\lvert DFBeta \rvert < 0.3$	full	$\lvert DFBeta \rvert < 0.15$
	(1)	(2)	(3)	(4)
Share of serfs	0.90	0.73	0.65	0.46
	[0.153]	[0.164]	[0.198]	[0.141]
log distance from Moscow, crop suitability	Yes	Yes	Yes	Yes
Observations	42	38	447	438
Adj. R^2	0.554	0.541	0.043	0.040

Notes: In panels A and B, standard errors are clustered by province separately before and after the 1861 emancipation reform. In panel C, standard errors are adjusted to spatial correlation within 900 km. Post-emancipation is a dummy, which is switched on in 1861.

dummy.[54] The coefficient on the main variable of interest is positive and statistically significant irrespective of specification, and the first stages are sufficiently strong not to worry about a weak instrument problem. IV and OLS point estimates are within the confidence interval range of the other estimate and IV estimates in the province-level and district-level samples are close in magnitude. The precision of district-level estimates is somewhat smaller most likely because there are only two post-emancipation cross sections.

[54] In the district-level regressions, we exclude districts with less than 30 draftees to be able to estimate precisely the average height of draftees. The results are robust to using all districts. The baseline district sample also excludes the Moscow district because this observation is an outlier in the cross-sectional regression. We also verify that the results are robust to excluding the Saint Petersburg district (we report these results in online Appendix Table A7).

These regressions can be interpreted as estimates of the effect of the abolition of serfdom on height of draftees under the following strong assumptions. We need to assume that: first, there were no confounding epidemics; second, the draft rules, i.e., the universality of conscription and the random nature of the lottery, were enforced; and third, the chest-to-height restrictions that were introduced in 1882 and almost completely undone in 1883 and 1884 were implemented as designed (in Section VB, we address the question of how these reforms could have affected our estimates if they were properly implemented). It is important to note that we cannot fully verify these assumptions and that the enforcement of rules was not perfect. Thus, one should interpret the estimates with caution. In addition, to interpret the estimates directly even though they represent the differences between the change in height of draftees in provinces with high and low level of serfdom, we need to assume that the emancipation had no effect on nutrition in provinces that relied on free labor.

Under these assumptions, the IV estimates imply that the abolition of serfdom in an average province led to an increase in the height of draftees by 0.35 centimeters ($0.35 = 0.78 \times 0.45$) as both serfs and free peasants had the same chance to be drafted and serfs constituted 45 percent of the total population in an average province. As the draftee's height is an individual characteristic rather than a characteristic of the economy, one could also interpret the results at the individual level: those born after the emancipation in (former) serf families were 0.78 centimeters taller on average than they would have been without the abolition of serfdom. Using the relationship between height and per capita incomes of European males in the second half of the nineteenth century presented by Floud (1990), we can calibrate the increase in incomes with which these gains in height were typically associated. In particular, the average height of draftees born in 1858 (164.82 cm) implied an income per capita of $664.52 (in 1970 US$) and the average height of emancipated peasants, according to our estimates (165.6 cm $= 164.82 + 0.78$) implied an income per capita of $790.32: i.e., the emancipation was associated with an increase in well-being comparable to a 18.9 percent increase in per capita income at that level of development.[55] These improvements in well-being could be driven by a combination of two factors: the boost of productivity as a result of the abolition of serfdom which we find in Table 2 and the redistribution from landlords to peasants which may have occurred as a result of the emancipation, when the peasants' obligations to landlords were fixed.[56]

In panel C of Table 4, as is the case for the other two outcomes, we present cross-sectional regressions with standard errors corrected for spatial correlation for both province- and district-level data. We detrend the average height by regressing it on birth-cohort dummies, take averages of the detrended height for each province

[55] The first two rows of Table 5 in Steckel (1995) and column 3 in Table 3 in Floud (1990) suggest that, for heights between 163.8 and 166.9 for European adult males in the second half of the nineteenth century, the relationship between per capita income (Y) and height (H) can be approximated by the following equation: $Y = (H - 160.7) \times 5000/31$. It is important to note that the relationships between height and per capita income could be different for European and Russian males, in which case one cannot apply this formula. There are no similar studies for the Russian Empire.

[56] A potential alternative mechanism is that the differential access to health care for serfs and free peasants was affected by the emancipation. However, there is no historical evidence that serfs and other rural citizens suffered differentially from the pandemic diseases that had an effect on the biometrics in adulthood, such as cholera or typhus (e.g., Brokgauz, F. A., and I. A. Efron. 1903. Vol. 37, article "Cholera"; Arkhangelskii 1874).

and district, respectively, separately for those born before and after 1861, take a difference and regress it on the pre-emancipation share of serfs. We also repeat this exercise, excluding the most influential observations.[57] The results are robust to accounting for a spatial correlation of errors and to excluding outliers.

The Confounding Reforms of Draft Rules.—An important potential concern with our analysis of height is the confounding reforms of draft rules that occurred in 1882, 1883, and 1885, affecting cohorts born in 1861 (the year of the emancipation), 1862–1863, and 1864 and after, respectively. The law expressed minimum requirement for chest in terms of height. The minimum required chest size was increased in 1882 and decreased in 1883 and then again in 1885 (almost to the level before the 1882 draft reform). For example, for the height of 164.82cm (equal to the average height of 1858 cohort drafted in 1879), draft rules required the minimum chest size of 80.1875cm before 1882; the required chest size was increased by 5.5 percent in 1882 and decreased by 1.3 percent in 1883 and then decreased further by 3.3 percent in 1885; overall, the minimum chest size requirement after 1885 was only 0.7 percent higher than before 1881. In percentage terms, the magnitude of the changes in minimum chest size requirements was almost the same for height of the range between $+/-$ three standard deviations around the average height of the 1858 cohort. Potentially, the 1882 reform of draft rules could lead to a bias in favor of finding a positive effect of the emancipation on height because of a negative relationship between height and the ratio of chest to height. One should note, however, that our results on the increasing trend in the difference in height between provinces with high and low prevalence of serfdom for cohorts born before the emancipation presented at Figure 9 cannot be driven by these reforms of draft rules.

We have used disaggregated data on height and chest of Russian Orthodox draftees in Bobruisk district from Gorskij (1910) to correct for the potential selection bias due to changes in draft rules.[58] If, however, the draftees from Bobruisk were not representative of draftees in the rest of Russia in terms of their anthropometric characteristics, this correction is not enough to eliminate the potential bias in our estimates. To address this concern, we use 1883 and 1885 changes in minimum chest-to-height requirement as a placebo and estimate the difference-in-differences effect of placebo emancipations that took place in these years. The prediction is that if our estimate of the effect of the emancipation on height is driven by the confounding change in draft rules in 1882, the estimated placebo effects for years 1883 and 1885 should be of the opposite sign and their sum should be of approximately the same magnitude as the effect of the reform, as the changes made in 1883 and 1885 practically reversed the change introduced in 1882.

Online Appendix Table A9 presents the results of the placebo tests for height adjusted for possible selection using disaggregated data from Bobruisk and

[57] At the province level, as above, we set the cutoff for influential observations at $|\text{DFBeta}| = 0.3$; at the district level, the cutoff is set for 0.15, as the highest value for $|\text{DFBeta}|$ is 0.22. Due to a larger number of observations at the district level, each individual observation has a smaller effect on the estimated coefficient. The cross-sectional relationships are illustrated by conditional scatter plots in the two panels of online Appendix Figure A10, in which we indicate DFBeta for each observation.

[58] Online Appendix Table A8 reports the minimum and maximum chest sizes of Orthodox men in Bobruisk by height categories from Gorskij (1910).

unadjusted raw height data. We run specifications analogous to the one presented in column 1 of Table 5 on the sample of cohorts born after the emancipation (i.e., starting with the draft year of 1882) to estimate the differential effect of the 1883 and 1885 changes in the draft rules for provinces with high and low shares of serfs. We estimate the effect of the 1883 reform in columns 1 and 4 by focusing on cohorts born after the emancipation but drafted before the 1885 reform, the effect of the 1885 reform in columns 2 and 5 by focusing on cohorts drafted after the 1883 reform, and on the cumulative effect of the two reforms in columns 3 and 6 by comparing cohorts drafted in 1882 with cohorts drafted after 1885. In all specifications, we find positive insignificant coefficients on the interactions between the pre-emancipation share of serfs and post-1862 or post-1864 birth-cohort dummies. The positive sign of the coefficients is inconsistent with the hypothesis that the change in minimum chest-size requirement that occurred in 1882 had a positive differential effect on provinces with different pre-emancipation shares of serfdom (as that reform went in the opposite direction to the reforms of 1883 and 1885). Thus, we can attribute the differential increase in height between cohorts born before and after 1861, presented in Figure 9 and Table 5, to the emancipation. Consistent with Figure 9, these positive placebo coefficients provide additional evidence that the emancipation reform had a gradual impact, as the effect continued to increase with time after the emancipation.

Finally, whether we correct the average height figure for the selection due to changes in chest-to-height requirements does not affect any of our estimates of the differential effect (even though they do change the average height figures). This can be seen from virtually identical estimates in Table 5 in the main text, which presents results for the adjusted height, and online Appendix Table A10, which presents results for unadjusted height figures, as well as in columns 1–3 as compared to columns 4–6 in Table A9. Overall, we find no effect of the confounding reforms of draft rules.

VI. Additional Sensitivity Tests

This section briefly describes a multitude of sensitivity tests that we conducted to verify the robustness of our findings to controlling for potentially confounding factors and using alternative data sources, various sample restrictions, and different specifications.

First, we verify that our results are not driven by the following potentially confounding factors: the length of the railway network in a province and year (in log kilometers), historical yearly temperature, and measures of court reform, which started in 1864 and was implemented in different provinces at different rates, and of the so-called *zemstvo* reform, which introduced elected local self-government bodies in 34 out of 46 provinces in our baseline sample in 1864.[59]

[59] To account for the court reform, we construct a dummy variable, which switches on when the court reform was launched in a particular province. To account for the *zemstvo* reform, we interact the annual *zemstvo* expenditure in each province (averaged across years for which the data are available: 1868, 1871, 1876, 1880, 1885, 1890, 1895, and 1903) normalized by rural provincial population in 1858 with the post-1864 period dummy. Online Appendix Tables A11, A12, and A13 report the results for each of our outcomes controlling for each of these potentially confounding factors separately and together. Our main coefficients of interest, estimating the effect of the abolition of serfdom, remain positive and statistically significant in 13 out of 15 regressions. In 2 regressions with industrial output as the outcome variable and *zemstvo* expenditures as an additional control, statistical significance is lost. This

Second, we verify that the results concerning the land reform are robust to excluding observations for the provinces of the former Polish-Lithuanian Commonwealth in the years before 1843 (see online Appendix Table A14). We do this to rule out a concern of possible endogeneity of the share of monasterial serfs in these provinces before 1843, as there, in contrast to the rest of the empire, the nationalization of monasterial lands continued until 1842. We also verify that the results are robust to restricting the sample to only the core provinces of the empire, i.e., Great Russia, New Russia, and the Eastern part of Belorussia, consisting of 35 out of 46 provinces, where the land reform was regulated by a single statute.[60] We further verify that the redistribution of land between peasants and landlords, which was decided at the signature of the 1863 regulatory charters, did not drive the main effects of the abolition of serfdom on agricultural productivity: we add a measure of how much land peasants "lost" as a result of the reform to the list of covariates and find similar results.[61] We also show that the effect on productivity does not depend on the size of the estate or the prevalence of small peasant farms as opposed to large private landlord farms, measured as a share of serfs on quitrent in a province. Both of these measures can be viewed as proxies for the access to capital and technologies.[62]

Online Appendix Tables A17 to A20 show that the results are robust to using 1857 tax census data on the share of serfs across provinces (Kabuzan 1971) instead of the 1858 data from Bushen (1863).[63] Further, in regressions for grain productivity, we restrict the sample to years before 1883, as for this subsample the data came from a single source, governor reports.[64]

We study the robustness of our results to the inclusion of Baltic provinces into the sample; the results are presented in online Appendix Table A22.[65] We also verify the

is not surprising, as these expenditures were channeled to the least industrially developed provinces as reflected in the negative and significant coefficient on this control, which makes them highly endogenous.

[60] In the empire, there were four different laws (charters) that regulated the rules of the land reform throughout the empire. They differed in terms of the size of the minimum and the maximum plots that former serfs' households could get as a result of the land reform (*Russian Empire* 1863, vol. 36, part 1). See columns 1 to 4 of online Appendix Table A15, which replicate columns 6 and 7 of Table 2 on the restricted sample.

[61] On average, peasants got less land in ownership than they cultivated under serfdom (Zajonchkovskij 1968). These results are presented in the columns 5 and 6 of Table A15. Column 5 presents the results for the subsample of the Great Russia provinces, which experienced the biggest land "cuts," and column 6 for the baseline sample.

[62] See columns 7 and 8 of online Appendix Table A15. We do not have instruments for either the size of the estates or the prevalence of private landlord farms, both of which could be endogenous. Therefore, one should be cautious about the interpretation of these results.

[63] The point estimates have similar magnitude to the baseline. The precision of estimates, however, goes down. In 2 out of 21 regressions, the coefficient of interest loses statistical significance at the conventional level. The decrease in the precision of estimates is to be expected because the 1857 data are much noisier.

[64] Online Appendix Table A21 reports specifications presented in the two top panels of Table 2 for this subsample. The main result on the overall effect of the abolition of serfdom holds (columns 1–5). In columns 6 and 7, we present the regressions that aim at disentangling the effects of the emancipation per se and of the land reform for this reduced sample. The results hold only in the IV specification (column 7). In OLS, the coefficients of interest are insignificant, and the coefficient on the land reform has a wrong sign. This might be because in this subsample there are no observations for the years after the end of the land reform. Importantly, as the land reform is endogenous, only IV regressions are valid (provided that the IV is excludable).

[65] In the sample including Baltic provinces, the post-emancipation dummy varies both over time and across provinces: it switches on in 1819 in the three Baltic provinces and in 1861 in all other provinces. The $ShareSerfs_i$ for the Baltic provinces is equal to the share of former serfs in 1858 according to Bushen (1863). As the Baltic provinces are special in many ways, we also include the interaction of control variables with the Baltic provinces dummy. The first two columns of Table A22 present the results. We find a positive and significant effect of the emancipation on grain productivity. In columns 3 and 4 of Table A22, we allow the effect of the emancipation to differ between Baltic provinces and the rest of the sample. The effect in the Baltics is positive but imprecisely estimated, so we cannot reject the hypothesis that the effects are the same in the two groups of provinces. Point estimates for Baltic provinces and for the provinces from our baseline sample are similar in magnitude.

robustness of the results to weighting observations by log provincial population as reported in online Appendix Tables A23 to A26.

Finally, we run a series of placebo tests on the sample before 1861, in which we replace our main explanatory variable of interest, i.e., the interaction between the share of serfs and the post-1861 dummy with the interaction between the share of serfs and dummies, which switch on in different consecutive years. We present the estimated coefficients along with their confidence intervals graphically on online Appendix Figure A11. All of them are fairly precisely estimated zeros.

VII. Conclusions

The abolition of serfdom had a substantial positive effect on agricultural productivity, industrial development, and peasants' nutrition in nineteenth century Russia. The improvements amounted to about a 17.7 percent increase in Russia's GDP in the second half of the nineteenth century. The evidence suggests that a primary reason for the large effect of the abolition of serfdom on agricultural productivity was a sharp change in the incentive structure of 43 percent of Russia's rural population, which was transformed by the 1861 emancipation from serfs with no rights over their own labor or human capital into free small-scale farmers. This change led to a greater effort, better use of local conditions, and better use of available agricultural knowledge and technologies.

The abolition of serfdom would have contributed to even faster development if the land reform had transferred ownership rights over land to peasant households rather than the commune, or at the very least to hereditary rather than the repartition communes. The increase in the power of the repartition peasant commune (designed by the land reform) was the most likely mechanism behind the negative effect of the land reform.

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