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## Property rights, land liquidity, and internal migration

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## ABSTRACT

In the early twentieth century, a large number of households resettled from the European to the Asian part of the Russian Empire. We propose that this dramatic migration was rooted in institutional changes initiated by the 1906 Stolypin land titling reform. One might expect better property rights to decrease the propensity to migrate by improving economic conditions in the reform area. However, this titling reform increased land liquidity and actually promoted migration by easing financial constraints and decreasing opportunity costs. Treating the reform as a quasi-natural experiment, we employ difference-in-differences analysis on a panel of province-level data that describe migration and economic conditions. We find that the reform had a sizeable effect on migration. To verify the land liquidity effect, we exploit variation in the number of households participating in the reform. This direct measure of the reform mechanism estimates that land liquidity explains approximately 18% of migration during this period.

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## 1. Introduction

Internal migration is a central feature of economic development. An important factor for potential migrants that the vast literature on the causes of migration has rarely emphasized is land liquidity. Potential migrant households usually hold assets in both labor and land. If land is illiquid, a household will lose all or most of its land assets when it migrates. Historically, restrictive land rights have limited the liquidity of land and have consequently inhibited migration. Using a Russian land reform from the early twentieth century as a quasi-natural experiment, we confirm that the liquidity of land does matter for migration, showing that improvements in property rights, and the resulting increase in land liquidity, have had the laudable effect of encouraging migration.

Specifically, we investigate the impact of the 1906 land titling reform in the Russian Empire—a core part of the broader agrarian reform, known as the Stolypin reform after its key promoter, tsarist prime minister, Petr Stolypin—on internal migration patterns during

the period of 1901–1914. In late Imperial Russia, internal migrants were mainly peasants who resettled from densely populated European provinces to the sparsely populated Siberian and steppe provinces in the Asian part of the empire. The state would allocate plots of land for permanent usage free of charge to newcomers and the abundant virgin land beyond the Ural Mountains yielded high returns to migration. Fig. 1 demonstrates that the annual number of migrating households was about 15,000 before the reform, never exceeding 30,000 in a single year. After the reform, migration quickly reached a new yearly level of 40,000 households, peaking at 80,000 to 100,000 in the first three years (1907–1909). We show that increases in land liquidity due to better property rights brought about by the reform explain at least 18.1% of the postreform Europe–Asia migration.

The Stolypin reform that attempted to address the backwardness of Russian agriculture and land titles were a means to unlock the full benefits of the reform. The land titling component of the reform specifically undermined the power of the repartition commune (*peredel'naya obshchina*). The repartition commune, an outcome of the 1861 emancipation of the serfs, was a mandatory association for peasant households in about eighty percent of rural settlements in European Russia. Formally, the commune collectively owned peasants' land and could periodically reallocate usage rights to land plots on the basis of family size and composition or failure to meet obligations to the commune. The reform allowed

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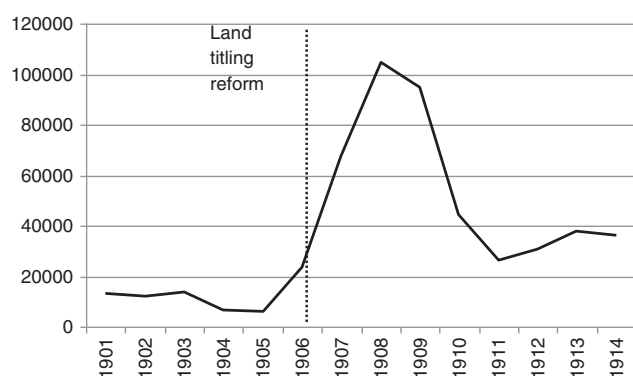


Fig. 1. Migration dynamics 1901–1914: the annual number of households that migrated to the Asian part of the empire. Source: Turchaninov (1910, 1915).

households to exit the commune with a land title that granted greater individual rights for plots currently in the household's possession. This switch from communal to individual ownership promoted peasant migration by improving the liquidity of land through both economic and institutional means. The economic effect is straightforward: households can more easily sell their land allotments and finance migration, something particularly important for migrants planning to permanently resettle. The institutional effect is indirect. Here, the opportunity cost of migration decreases because owners are able to better extract land rent without being physically present. We interpret both effects broadly as functioning as an increase in the liquidity of land because, in both cases, the capacity to convert land into cash improves. These arguments apply to emigration as well, but the empirical focus on internal migration allows us to hold a host of other institutional factors fixed.

To obtain our empirical results, we construct a panel of province-level data describing migration to the Asian part of the empire, before and after the 1906 land titling reform. We argue that the reform crucially provides a source of exogenous variation in land liquidity since only repartition communes experienced significant changes in individual transfer rights. Peasant households not located in repartition communes were either organized into hereditary (*podvornaya*) communes or no commune at all; in both cases, the transfer rights of individual households were unaffected by the reform. We define an affected province as one in which at least 5% of the rural population inhabited repartition communes (provinces shaded green in Fig. 2); there were forty-one such provinces.<sup>1</sup> We then apply difference-in-differences analysis to European provinces (*guberniyas*) that were affected and those that were unaffected by the land titling reform. Using this technique, we can attribute 170,000 of the 441,000 migrating households during the postreform period to the land titling reform. To unpack the causal channels, we investigate how the variation in the number of land title conversions affected migration flows, employing an instrumental variables approach to deal with the potential endogeneity. We find that the number of households that exit the commune with land title conversions

explains the migration of 80,000 households, nearly half of the overall effect of the reform. This result represents a lower bound, since land titling reforms tend to (1) decrease the attractiveness of out-migration by potentially improving living standards in the areas affected by the reform and (2) reduce the pool of potential migrants to rural areas by encouraging rural–urban migration. With these baseline results, we contribute to the empirical literature on migration, showing in a unique and rigorous manner that improvements in land liquidity contribute to an increase in migration.

The migration literature has focused primarily on imperfect labor markets and neglected the role of property rights and land liquidity (Greenwood, 1997; Lucas, 1997). Those papers that deal with property rights and migration place more attention on the impact of tenure security on the opportunity cost of migrating rather than on the importance of land liquidity for financing migration (de Janvry et al., 2012; De la Rupelle et al., 2009; Field, 2007; Mullen et al., 2011; Yang, 1997). To address this gap, we build a simple migration model that incorporates liquidity constraints due to restrictive property rights into the households' migration decision. We then contrast the land liquidity effect with the tenure security effect considered in the previous literature by separately investigating the impact of the reform for permanent and temporary migrants. The tenure security effect matters little for migrants who plan to permanently resettle but should matter a lot for migrants who plan to return. Empirically, we show that there is a positive effect of the reform on both types of migrants but the economic significance is much larger in the case of permanent migrants, suggesting that, for Europe–Asia migration in late Imperial Russia, the land liquidity effect had a larger impact than the tenure security effect.

This evidence about the relative importance of the liquidity of land for migration informs the debate about internal migration in contemporary policy discourse. For example, a 2010 report by the World Bank (2010) argues that Russians should be moving around within the country more than they are, considering the economic costs and benefits of migrating. Placing more emphasis on the capacity of property rights to affect the financing and the opportunity cost of migration would shift the policy focus toward improving the liquidity of Russians' assets. Unfortunately, with our data, we cannot cleanly distinguish empirically between the financing effect and the opportunity cost effect, which obviously matters for policy. However, we present evidence that favors the financing effect as the dominant effect for peasants in late Imperial Russia.

Given the nature of the reform as a quasi-natural experiment, we address several concerns about the validity of our approach. First, we separate the impact of the land titling reform from the government's migration policy. The latter largely existed independently of the titling reform but changed concurrently with it. The government supported migration through the use of subsidies, and these subsidies became more generous after 1906, although the government tended to require that households have wealth above a particular threshold. We study separately subsidized and unsubsidized migrants to check how the shifts in the governmental migration policy influence our findings. We find that the effect of the reform is primarily for unsubsidized migrants, further highlighting the role of financing constraints in explaining migration patterns. Second, we use data on land sales to verify that transfer rights did indeed matter for migrants. Third, we address concerns about omitted variables by incorporating additional controls. Fourth, we perform a sensitivity analysis of our control and treatment groups, both contracting and expanding the control group. Finally, we evaluate several alternative hypotheses and run several placebo regressions derived from historical accounts of migration promotion. The results are remarkably robust to this analysis.

The structure of this paper is as follows: First, we briefly describe the relevant literature. Next, we review the necessary historical details of the Stolypin land titling reform and Europe–Asia migration. Third, we present a simple model of the migration decision and discuss the economic arguments for which outcomes we might expect. Fourth, we

<sup>1</sup> Repartition communes represent more than half of the peasants in our treatment provinces, with the exception of Kiev, Poltava, and Bessarabiya provinces (shaded light green in Fig. 2) (Dubrovskii, 1963, pp. 570–573). Repartition communes in Cossack communities in Don, Kuban', Orenburg, and Terek provinces (green) existed under different tenure regimes. Cossack communities enjoyed privileged status in the empire, possessing extensive land estates at the expense of extended military duty for males. There were no communes in the Baltic (Liflandia, Estlandia, Kurlandia) provinces (light blue). All communes in Grodno, Kovno, Vilno, Minsk, Podolia, and Volin provinces (blue) had hereditary (*podvornaya*) tenure under which an exit from the commune was possible and could include compensation for land. We have no migration data on Arkhangelsk, Olonetz, and Yaroslavl' provinces (white), nor for Polish and Finnish provinces (grey).

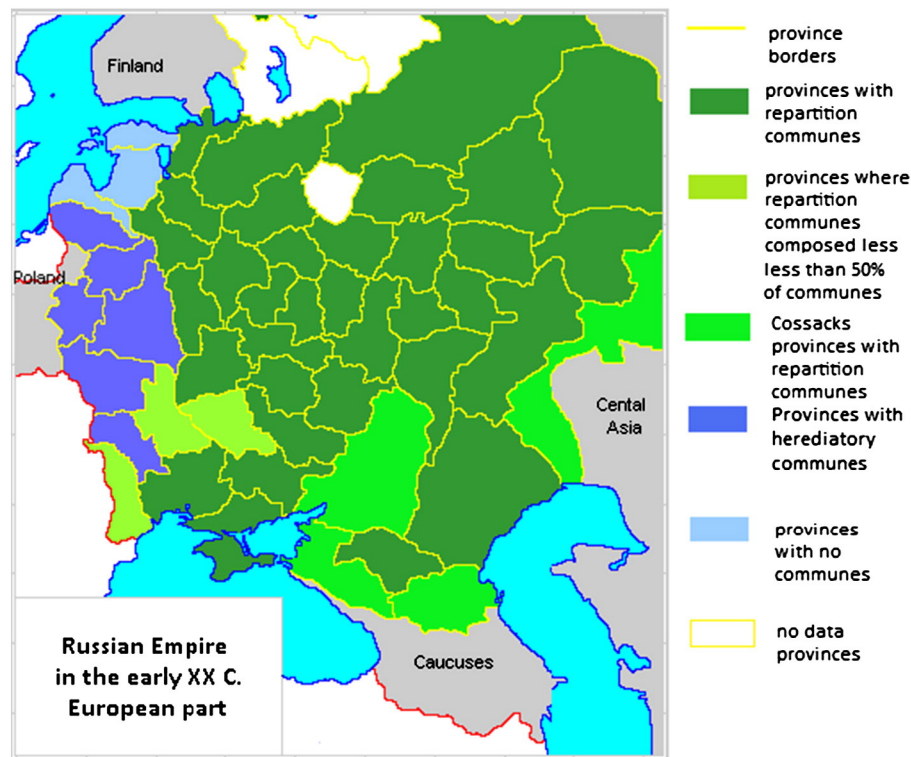


Fig. 2. A map of provinces in the European part of Imperial Russia. (For interpretation of the references to color in this figure, the reader is referred to the web version of this article.)

introduce and summarize the data, discuss how we obtain evidence for the land liquidity effect, and present the basic empirical results. Finally, we draw conclusions about the empirical relation between the land titling reform, land liquidity, and internal migration.

## 2. Literature review

The focus on land liquidity fits naturally into the new economics of migration literature. Whereas the canonical migration literature focuses narrowly on the rural–urban wage gap, assuming that potential migrants in rural areas have moveable resources at their command, such as their labor, that are not being put to their most valued use (Lewis, 1954; Todaro, 1969), the new economics of migration literature recasts the household's optimal resource-allocation problem to account for other imperfect markets, financial constraints, and various transaction costs (see Stark (1991) for a review). In this latter literature, migration flows are less sensitive to wage differentials because of the location-specific nature of important factors in the migration decision. For example, Munshi and Rosenzweig (2009) show how social networks along caste lines restrict mobility by providing social insurance where insurance markets are undeveloped. We contribute to this literature by introducing land liquidity, which influences the location- and time-specific value of land, as an important constraint on the migration decision.

Despite a vast empirical literature on the determinants of migration, relatively few studies have been able to show how much financial constraints matter for migration.<sup>2</sup> Separating a household's opportunities at the place of origin and financial constraints is difficult. Some studies provide evidence of such constraints (Andrienko and Guriev, 2004; Du et al., 2005; Halliday, 2006; Hatton and Williamson, 1994) but they must resort to testing whether there is a positive correlation between income or wealth and migration below some threshold, which opens

the door for potential bias in the estimates. McKenzie and Yang (2010) have called for more experiments, both natural and controlled, to help understand the causes and effects of migration given the empirical difficulties associated with selection and endogeneity. Our research is a step in this direction; using a quasi-natural experiment, we show that in late Imperial Russia greater land liquidity promoted internal migration.

In general, the evidence connecting land rights and migration is scant, and we are not aware of any study that shows evidence of the effect of changes in the right to sell individual plots on migration. In the most cited paper in this literature, Field (2007) instead emphasizes the role of tenure security and shows how obtaining a formal individual land title encourages individuals to supply more labor in urban areas of Peru. In the context of rural China, Mullen et al. (2011) also find evidence of a positive effect on migration due to improvements in tenure security but show little effect of being able to transfer usage rights on migration. Consistent with this finding, de la Rupelle et al. (2009) argue that land tenure insecurity is a major cause of return migration in China. While these two studies on China are instructive, they are not well suited to address the question of land liquidity because land is owned by the state and there are complicated rules about land usage. The paper closest to ours, De Brauw and Mueller (2012), which investigates the role of land transferability on migration in Ethiopia, also suffers from this criticism, since all land is state-owned. The strength of this paper is that it addresses the question of rights to transfer land explicitly. In contrast to our results, they find a slightly negative effect. However, we believe that our identification strategy is on safer ground. As a source of variation in land rights, the authors employ the share of usage rights to land that the household perceives to be transferable, a measure that is certainly determined endogenously along with the migration decision.<sup>3</sup>

<sup>2</sup> An interesting solution to financial constraints arises for illegal migrants. The future supply of cheap labor can serve as collateral to secure a loan that covers the cost of migration (Assunção and Carvalho, 2010; Friebe and Guriev, 2006). This mechanism works less well for internal migration, however.

<sup>3</sup> The authors attempt to address these concerns in several ways. In their favored approach, land rights must co-move with observable and unobservable variables in the same direction and normalized magnitude, ruling out any unobservable variable, such as strong customary tenure rules, that correlates negatively with observables such as wealth and education levels yet correlates positively with perceptions of land transferability.



Finally, our paper relates to the literature on political and institutional constraints that prevent both emigration and internal migration. Galor et al. (2009) argue that the agricultural elite suppressed the education of rural labor, resulting in delays in industrialization, i.e., movement from rural areas to urban industrial centers. Hence, the lack of internal labor mobility may reflect institutional barriers to economic development. These barriers are especially likely to emerge in countries with a frontier, as Domar (1970) argued, by creating perverse incentives for the landed aristocrats. Our paper gives empirical evidence of how the removal of restrictive land rights leads to greater mobility and greater economic growth.

### 3. Historical section

In the early twentieth century, Imperial Russia's political system defended the interests of the gentry—large landowners—against the peasants. The unequal land distribution provoked mass unrest that eventually led to the 1905 revolution. While the government quickly managed to overcome disorder in the countryside with military force, it also initiated a new agrarian policy—the Stolypin reform—designed to secure political support in the Russian village without major land redistribution. The titling component of the Stolypin reform aimed to increase the number of landowners by allowing peasants to privatize their communal plots. The government also started to promote Europe–Asia peasant migration more actively, with the intention of increasing peasants' landholdings per capita in Europe at the expense of resettling some population to the virgin lands in the East. In this section, we first provide details on the Stolypin land titling reform—the key initiative that increased land liquidity—and then review Europe–Asia migration and migration to other potential destinations.

#### 3.1. The Stolypin land titling reform

The Stolypin land titling reform—introduced by the 9th of November 1906 decree—dramatically changed the power of the repartition commune, and peasants' property rights, in particular. Before the reform, the only move that peasants could undertake without the commune's consent was resettling to the East, but without any compensation for the left-behind allotment.<sup>4</sup> The reform granted peasants an opportunity to exit the repartition commune freely, converting titles of land strips they cultivated from communal to individual tenure. On average, 150,000 households left the commune annually with a spike of 700,000 during the first two years after the reform. In total, by January 1, 1916, the reform created 2,008,432 exits, with individual titles to land of 15,429,157 ha, meaning 22% of households obtained individual titles to 14% of communal land. Individual titles gave peasants the right to sell plots,<sup>5</sup> which radically increased the liquidity of peasants'

land.<sup>6</sup> In contrast, the land titling reform left peasants' property rights almost unaffected in hereditary communes<sup>7</sup> or rural settlements without communes.

Under the 1906 decree, a household<sup>8</sup> that wished to obtain an individual title in a repartition commune appealed to the commune to arrange details of its exit. If they failed to come to an agreement, a local land captain (*zemskii nachalnik*) arranged exit details under his authority. In both cases, with or without the commune's consent, a district court on peasant affairs (*uezdnii s'ezd*), of which all land captains in a district as well as other district officials were members, had to confirm the details of the land title conversion. Land captains and district courts generated a lot of bureaucratic red tape in the exiting procedure. This red tape slowed down exiting and caused additional variation between the provinces in the yearly totals of exiting households. The imperial government constantly put pressure on provincial governors as well as land captains and district court officials to make the exit procedure shorter, though without much success.<sup>9</sup>

Importantly, the main determinants of this specific type of bureaucratic efficiency were largely independent of the reform or the allocation of migration subsidies. The reform-related red tape depended on the competence of local officials; this competence was largely determined in the previous decade when the state policy was instead procommune rather than anticommune and the reform was not on the agenda. First, land captains suffered from insufficient education and from being overloaded with other responsibilities (B.Zh., 1898; Dubrovskii, 1963, p. 167).<sup>10</sup> Many of these officials had held their positions long before the reform, and few land captains were added after the reform (2615 in 1913 vs. 2604 in 1906, Central Statistical Committee of the Ministry of Interior Affairs, 1907, 1914). The government faced a very limited supply of candidates. Originally, the government intended to hire only local gentry for the positions of land captains, but it had to extend the pool of potential officials to retired military officers, graduates of Orthodox divinity schools, and other nongentry, middle-class citizens (though not to peasants themselves) (B.Zh., 1898). Second, the approval process relied on how well local documents recorded the history of the division of commune lands into household strips, and poor documentation prolonged the procedure (Maksimov, 1999, p. 95). Commonly, it was exactly the same local land captains who oversaw the reform that were in charge of creating this

<sup>6</sup> There is evidence of informal land sales under the commune's authority (Nafziger, 2007), but such informal contracts did not fully guarantee individual property rights (Ministry of Internal Affairs, 1912, Vol. 2, p. 49), and that drove selling prices down (Yamzin, 1912, pp. 42, 107).

<sup>7</sup> Hereditary tenure assumed ownership passing down within the family. Before the reform, an exit from a hereditary commune did not require the commune's consent; it required only an individual either inside or outside the commune who was willing to take the land allotment and related obligations. The transfer might include monetary compensation for land, i.e., quasi-selling of plots was possible in the commune with hereditary tenure. For households with hereditary land tenure, the land titling reform introduced an opportunity to get individual tenure (like for households in repartition communes), but this did not extend land rights much. Individual tenure formalized the right to sell land that already had existed in practice in hereditary communes before the reform.

<sup>8</sup> The reform did not affect the number of households in repartition communes. Both before and after the reform, the head of households could block potential splitting. Contemporaries argued that customs regulated inheritance and splitting in peasant households (Leontiev, 1914, pp. 320–321, 327).

<sup>9</sup> See, for example, decrees of the Ministry of Internal Affairs issued December 30, 1909, and June 14, 1910 (Ministry of Internal Affairs 1910, Vol. 1, p. 15; 1912, Vol. 3, p. 106). The government also tried to use career incentives to promote and fire local officials based on their performance in reform implementation, though again without success. The government failed to fire officials from noble families or with connections; several governors were openly opposed to such a strict policy of the central government (Dubrovskii, 1963, pp. 167–174). Maksimov (1999, p. 96) provides further examples how the government tried to speed up the approval procedure.

<sup>10</sup> The government tried to improve the quality of local imperial officials and even opened two-month courses for them in 1908, but education levels remained low (Dubrovskii, 1963, p. 167).

<sup>4</sup> The 1889 law introduced the right to migrate to the East without the consent of repartition commune for the subsidized migrants. Unsubsidized migrants formally got this right in March 1906, but in practice they had enjoyed it since 1896, when the government abolished the law of forced back-migration of unsubsidized migrants (Shilovskii, 2003, 2006; Simonova 1965; Tukavkin, 2001, p. 223); between 1896 and 1906, the number of unsubsidized migrants was growing together with the number of subsidized ones, comprising of 52% of all migrants. After the reform, the share of unsubsidized migrants fell to 43% because of the rapidly increasing number of subsidized migrants following the shift towards a pro-migration policy in March of 1906. In repartition provinces, this fall in the share of unsubsidized migrants was smaller than in non-repartition ones, which is consistent with the land liquidity effect.

<sup>5</sup> To be precise, the 1904 temporary rules introduced an opportunity for migrants to ask for compensation from the commune but did not grant the right to sell the allotment (Shilovskii, 2006). In practice, however, it was difficult for peasants to obtain compensation during these years because of the 1904–1905 Russian–Japanese War that cut migration flows. Tukavkin (2001) provides an example of Kharvovskaya province, where only about 3.5% of all migrants managed to sell their plots, in contrast to 83% in 1910. As a robustness check, we allow the reform to occur in 1904 instead of 1906; the results (not reported) do not support the hypothesis that 1904 rules already produced greater land liquidity that affected migration.

paperwork in the 1890s and early 1900s. Thus, we view this locally generated red tape as exogenous to the migration decision.

The land titling reform also had additional effects on peasants' opportunity costs of migration besides the liquidity effect. On the one hand, an individual title provided greater exclusion rights on household land, securing immunity from periodical redistributions of plots within the repartition commune. In addition, the Stolypin reform granted those who exited the right to request for consolidation of land into a joint allotment, i.e., the reform could improve land productivity and the future income stream derived from farming through improved incentives and the possibility to consolidate land. Indeed, 80.4% of peasants who exited the commune continued to cultivate their allotments (Dubrovskii, 1963, pp. 359, 375). However, in the technological and institutional frameworks of the early twentieth century Russian agriculture, changes in productivity required time to take effect. We argue that in the short-run a land title's contribution to the value of not migrating meant little if the peasant simply continued the same production activities. On the other hand, the reform's abrupt introduction of individual rights, especially for consolidated plots, may have intensified tensions and conflicts over land issues in the Russian commune (Pallot, 1999). These tensions may have simultaneously made living in the commune undesirable and migration more attractive.

### 3.2. Europe–Asia migration

Approximately 516,000 peasant households migrated from the European part to the Asian part of the empire between 1901 and 1914. Since each household contained on average six individuals, this means that about 3.1 million people moved to the East (Turchaninov, 1910, 1915). The majority of migrants considered the move as permanent and the government put a lot of efforts to resettle newcomers successfully (D.Ya. and Shilovskii, 2005). The Russian level of 221,000 migrating persons annually was similar to the rate of settlement of the American West in the nineteenth century. Between 1800 and 1900, around 450,000 individuals migrated to the American West annually (calculated from Mitchell (1998), inclusive of population growth due to fertility and mortality rates).

European migrants mostly resettled to the Siberian and Asian steppe provinces: about 75% migrated to modern Western Siberia and Northern Kazakhstan, and another 20% went to Eastern Siberia and the Far East (calculated from Yamzin, 1912, p. 175). Within these large regions, the government tried to distribute the newcomers to areas with similar local climates to the climate in their mother provinces in order to take advantage of migrants' local knowledge (Nagnibeda, 1913, p. XXI). The densely populated Black Earth region stretched from Ukraine to mid-Volga produced the highest number of migrants per province; Poltava, Chernigov, Kiev, and Voronezh being the first four sending provinces. In general, geography affected out-migration patterns: areas with higher population density had, on average, smaller land plots, increasing the returns to migration.

Emigration from the Russian Empire was also quite substantial at about 1.6 million people from 1900 to 1914, but migration to the New World or abroad was prohibitively costly for peasants in that time. Peasants from European provinces composed only about 10% of all transatlantic migrants from Russia. The majority of emigrants were Jews, Poles, and Finns, comprising 40%, 27%, and 8% of emigrants, respectively (Obolensky, 1928); 98% of Jewish migrants were city dwellers, and Poles and Finns migrated from the Polish Kingdom and Finnish Grand Duchy, in other words, not from European provinces that we study. The numbers of Russian transatlantic migrants per province are not known; however, Obolensky (1928) argued that there was an increase in trans-Atlantic migration caused by the Stolypin reform similar to Europe–Asia migration. In the robustness section, we check whether province participation in overseas migration affected movement eastward.

Migration to urban settlements was another alternative. Growing industrial areas around Saint-Petersburg and Moscow were an increasing source of demand for industrial labor. Again the numbers of rural–urban migrants per province are not known, but we can account for changes in the demand for workers using the dynamics of local wages and urban population.<sup>11</sup> According to our data, the average share of urban population in a European province increased from 12.4% in 1897 to 13.9% in 1913, while the net fertility rate was lower in cities than in the countryside. There was substantial seasonal migration to urban areas as well. The impact of the Stolypin reform on rural–urban migration was widely debated in the economic history literature. In his classic work, Alexander Gerschenkron (1965) argued that the reform contributed to the growth of industrial labor force easing institutional constraints on peasants' rural–urban mobility. We explore whether this possible increase in rural–urban migration affects our results in the robustness section.

Both contemporary surveys and interviews with former migrants from the 1950s and 1960s find large increases in welfare of Europe–Asia migrants relative to their living standards at the regions of out-migration (Tukavkin, 2001). It is difficult to estimate the returns to migration precisely because peasants received a huge part of their income in kind. According to a 1911–1912 survey conducted in Tomsk province, total value of their houses and buildings, cattle, farm implements, etc., minus arrears increased by 150% on average. Returns were even larger, with total value of these assets minus arrears increasing by 360%, for households that migrated more than eighteen years prior to the survey (Nagnibeda, 1913, p. XXIV). In terms of land allotments, the returns to migration, both before and after the Stolypin reform, were roughly 335%.<sup>12</sup> Since average yields in Siberia were roughly the same as in the Black Earth region—the most productive European region (Sklyarov, 1962, p. 402)—an increase in land suggested an increase in income from grain production at a similar or larger scale.<sup>13</sup>

After the construction of the trans-Siberian railroad in 1896, the newcomers covered a large part of the journey by trains; however, migration remained costly.<sup>14</sup> Travel expenses alone required 100–120 rubles per family (Tukavkin, 2001, p. 272); that equaled 80% to 100% of Russian 1913 per capita GDP, (Markevich and Harrison, 2011), not to mention startup expenditures estimated by officials to be at least 150 rubles (Voskresenskii, 1909). Given the costs of migration, the government subsidized resettlement to the Asian part of the empire both before and after the reform. The government provided individual travel and startup loans as well as collective infrastructure loans. However, only authorized migrants could get them.

To become authorized migrants, a peasant household had to apply for an official permit to migrate, granted by the Ministry of Internal Affairs and the Ministry of State Property, and send a delegate (*khodak*) to the settlement region to collect information and to enlist an allotment. Almost all authorized migrants applied for and received subsidies

<sup>11</sup> According to the 1897 census, 60% of city dwellers who were former peasants migrated within the same province and the rest migrated across provinces, but mainly to neighboring provinces (Troinitskii, 1905. Vol 1, pp. 104, 108). We use province urban share to account for the first group and urban wages within an industrial region for the second. We also use provincial rural wages to account for labor demand in agriculture.

<sup>12</sup> This number is calculated by taking the ratio of average allotment in the Asian part of the empire which the government leased to newcomers on a long-term basis (45 desyatinas, i.e., 49,1625 ha) and the average peasant allotment in the European part in 1905 (13.4 desyatinas, i.e., 14,6395 ha).

<sup>13</sup> For reference, Hatton and Williamson (1994) estimate a return of 150% and Abramitzky et al (2010) estimate 60% to 120% for European immigrants to the United States during the nineteenth and early twentieth centuries.

<sup>14</sup> The Trans-Siberian railroad went from Syzran' in Saratov province via Chelyabinsk to Omsk and then Irkutsk via Krasnoyarsk. It continued behind Baikal Lake and went to Chita and then to Vladivostok via Manchuria. It crossed roughly two thirds of the Russian Empire along the forty-fifth latitude, i.e. in Southern Siberia. On the last part of their journey, from railway station to their final destination, peasants traveled either by foot or by water.

in the amount close to the upper official limit (Ablazhei et al., 2010; Trekhsvyatskii, 1918). Unauthorized migrants had no right to loans or subsidies; in addition, they were last in line for allotments and did not receive tax or military service exemptions. Unauthorized migrants could, however, send delegates to eastern provinces to collect information (Resettlement Administration, 1908, pp. 14–15; Resettlement Administration, 1911, p. 74; Voshchinin, 1915).<sup>15</sup> To capture this difference between peasants with and without official permits, we refer to them below as subsidized and unsubsidized migrants.

The government shifted to a more pro-migration policy in March 1906, half a year before the land titling reform. New types of loans and subsidies were established, and upper limits for the old ones were extended (and extended further in 1908 and 1912). Governmental spending on migration increased tremendously, from just under 5 million rubles in 1906 to almost 30 million in 1914 (Trekhsvyatskii, 1918). Formally, the new policy simplified the procedure to get official permits to migrate (Voshchinin, 1915); however, in practice, the government continued to grant permits conditional on the applicant's wealth and on the availability of land in the place of destination (Ministry of Internal Affairs 1910, Vol. 4, p. 168).<sup>16</sup> Despite the huge increase in subsidies and migration flows, infrastructure in the Asian provinces remained underdeveloped (Trekhsvyatskii, 1918), suggesting that returns to migration did not increase as more migrants settled in these provinces.

The central government had no geographical preferences either before or after 1906 in distributing subsidies in European provinces. In particular, the government subsidized railway tariffs for migrants on the basis of pure distance, subsidizing per kilometer of travel equally for all provinces of out-migration (Resettlement Administration, 1908, p. 73). When the government had to introduce provincial quotas for subsidies in 1908 and 1909, it distributed them between provinces, taking into account only the level of the previous-year migration and current demand (Resettlement Administration, 1908, p. 17). The government also initiated an informational campaign on migration after 1906, but it did not have any clear geographical pattern. In 1907 alone, the government published 6.5 million brochures and leaflets, 130,000 handbooks, and 400,000 clarifications on the subject to disseminate among potential migrants all across the European part of the empire (Sklyarov, 1962, p. 125). The information campaign was mainly designed to address the fact that peasants did not have accurate expectations of what migration to the Asian part of the empire would bring them.<sup>17</sup> The participation of peasants as soldiers on the battlefield of the 1904–1905 Russian–Japanese War could have improved their knowledge of the regions of destination. During the two war years, the army enlisted 923,000 recruits and 1,166,000 soldiers were drafted from the reserve. Over a million soldiers were sent to the Far East, experiencing the long journey on the trans-Siberian railroad. However, conscription and mobilization showed no clear geographical pattern and affected peasants from both repartition and non-repartition provinces (Ministry of War 1906, 1907).<sup>18</sup>

<sup>15</sup> Between 70% and 80% of all delegates went back to pick up their families to resettle; the rest ordered their families to join them directly in their new places of living (Tukavkin, 2001, p.262). There is no underestimation of migration, even in the latter case, since we measure migration in the number of migrated households.

<sup>16</sup> The minimum wealth requirements varied between 125 and 300 rubles per family. In addition to this wealth requirement, the family could not have tax arrears.

<sup>17</sup> Isaev (1891) writes: "Many from the poor majority do not have right expectations. ... Some of them do not have any: it's hard to live at home, so poor peasants resettle to Siberia with a confidence it is not going to be worse."

<sup>18</sup> There were seven mobilizations from the reserve in 1904 and another two in 1905. They affected Saint-Petersburg, Moscow, Kiev, Vilno, Warsaw, Kazan and Siberian military districts, consisting of both repartition and nonrepartition provinces. Unfortunately, the precise distribution of mobilized reserves by province or even by military district is not known, in contrast to the figures of recruits conscripted under usual annual conscription procedure. Regular conscription occurred in both types of provinces as well (Ministry of War 1906, 1907).

The level of support of migration from local governments did vary across provinces and over time. Elected local governments, or *zemstvos*, which existed in thirty-eight European provinces in 1913, usually were more active; and *zemstvos* in nine southern provinces were among the leaders (Resettlement Administration, 1911; Smirnov and Smirnova, 2000; Zenchenko, 1912). The latter united in a special organization to promote migration. They issued a special journal to disseminate information among potential migrants, helped to organize groups of subsidized migrants, hired special agents to accompany these groups during their moves, and hired agents in Siberian and steppe provinces to help subsidized migrants resettle (Resettlement Administration, 1911, p. 4). They assisted mainly or even exclusively subsidized migrants (Yamzin, 1912).

The government hoped that the financing effect of greater land liquidity granted by the Stolypin land titling reform would promote Europe–Asia migration further and would help to overcome the problem of poor access to private short-term credit (Korelin 1988).<sup>19</sup> Indeed, because of the reform, peasants in repartition communes got the opportunity to sell or lease to other peasants or mortgage their plots to finance migration. In practice, land mortgage was not really an option, being allowed only in the state-owned Peasant Bank; the bank's statistics reveal that very few migrants actually chose this option.<sup>20</sup> According to various surveys, about 70% of postreform migrants sold their plots (Ablazhei et al., 2010; Tukavkin, 2001; Yamzin, 1912).<sup>21</sup> Land sales substantially increased funds for potential migrants. An average peasant household could get about 816.5 rubles if it managed to sell its plot at market prices.<sup>22</sup> The sum that actual migrants received was smaller, both because of smaller plots relative to nonmigrants<sup>23</sup> and time pressure that could drive land prices down. Some have suggested that it was as low as 239 rubles (Ablazhei et al. 2010, p. 31; Yamzin, 1912, p.111). But even this amount was about the minimum necessary to resettle in the East. In terms of household size, migrating households were slightly larger than average ones: 6.0 household members versus 5.8 (estimated from the 1897 population census figures as total rural population in European provinces divided by number of rural households).

#### 4. A simple model of migration and testable hypotheses

In the following subsection, we develop a stylized model of migration with liquidity constraints that is simple in two respects. First, we focus on the decision to migrate or not and abstract from other land, capital, and

<sup>19</sup> As an influential member of the Stolypin government, the head of chief administration of agriculture and land management, Alexander Krivoshein, said in his speech to the Russian parliament on the November 10, 1908: "The 9th of November [1906] decree provides ... a stable basis to one of the most important programs for the Russia government—the resettlement program. ... The decree gives them [peasants] an opportunity to leave and find a new life not as paupers but with some funds ... which will help them to set up new households at their new settlements. In the past peasants could not sell their allotment, they could only hand it to over peasants without getting money" (Karpovich, 1995). The contemporary booklets published by the government were full of advice to potential peasant migrants on how to privatize and sell their allotments (Dashkevich, 1912; Vvedenskii, 1909).

<sup>20</sup> The number of mortgages did not exceed 2500 per year for the whole empire (Dubrovskii, 1963; Zak, 1911).

<sup>21</sup> Among those who sold their plot, 12.6% migrated to the East; this share was 2.3% for those who sold a part of their plot (results of the 1912 survey of twelve districts in the European Russia cited by Dubrovskii, 1963, pp. 359, 375).

<sup>22</sup> This amount is based on an average price per hectare of 75.06 rubles in 1900–1902 (Opyt' ... 1906) multiplied by the average household plot size of 10.82 ha (Central Statistical Committee of the Ministry of Interior Affairs, 1914). These numbers were not substantially different in repartition provinces, where the price per hectare was 78.72 rubles (Opyt' ... 1906) and the average plot size was 11.03 ha (Central Statistical Committee of the Ministry of Interior Affairs, 1914).

<sup>23</sup> In 1899, migrants' average plot size per capita was 0.8 ha (estimated from Resettlement Administration (1902)). Unfortunately, there are no similar figures for the post-reform years.



labor allocation decisions that a household might face. In particular, we neglect the intrahousehold dynamics that have characterized the new economics of migration literature (Stark, 1984). However, we retain the message of these models in the sense that the migration decision is not completely driven by expected wage differentials. Second, we abstract from differences in places of destination, abandoning the emigration-function approach and gravity-type models. Wages at the origin and the returns to migration still determine the migration decision; there is simply only one destination. All these simplifications are reasonable from a theoretical and an empirical point of view. Since the friction we study is a departure from previous models, we focus the comparative statics on changes in land liquidity, leaving family dynamics and, even more important, commune-specific interactions in the background.<sup>24</sup> Empirically, we only observe the out-migration of entire households aggregated at the provincial level and to a single destination, the Asian part of the Russian Empire. Hence, we cannot address within household behavior, nor can we track migrants from their origin to their exact destination.

#### 4.1. The model

The household faces a stark decision: to migrate and parlay its wealth into a new life in the East or to remain and take advantage of the best-available opportunity at hand in the European part of the empire. This decision depends on the benefits and costs of migrating, the economic opportunities that the household faces in the European part of the empire, and liquidity constraints.

We represent the expected payoff of migrating with the variable  $\nu_{\text{Mig}}$ . This variable takes an expectation over all costs and benefits of migrating once the household begins production activities at the new destination. We motivate taking this value to be homogenous across the whole population with the fact that households had poor information about the unknown land in the East that did not vary by province and varied little by households. The costs of migration,  $C_p$ , include transportation costs and startup funds, which are not insignificant. Here, we assume that households in the same province face the same cost because the largest share of costs, railway and startup costs, should have been fairly homogenous across households. Subsidies,  $S_i$ , available through the government's migration policy, partially offset the cost of migration and could vary by household but not by province.

Each household  $i$  in province  $p$  can be described by the exogenous variable,  $x_{ip}$ , which summarizes its wealth, determined by the size and quality of land allotments and other assets in the household's possession. The variable characterizes the economic opportunities that the household faces in the European part of the empire. The household's  $x_{ip}$  is independently drawn from the following distribution,  $F(x; \mu, \sigma)$ , where  $\mu$  and  $\sigma$  are location and scale parameters. For the moment, we assume  $\mu$  and  $\sigma$  are fixed and, in the short-run, this is a reasonable assumption to simplify the analysis. However, we will discuss the case when these parameters change over time and how the reform might influence the distribution of  $x$ .

The key friction in the model is represented by a parameter expressing the liquidity of household wealth,  $\lambda^{\circ}_p$ , written in expected terms. The parameter  $\lambda^{\circ}_p$  reflects restrictive land rights and obligations to the commune associated with the household's allotment; consequently, it depends on the type of province, repartition or nonrepartition, with  $\lambda^{\circ}_R > \lambda^{\circ}_{NR}$ . The realization of this parameter,  $\lambda_{ip}$  with  $E(\lambda_{ip}) = \lambda^{\circ}_p$ , occurs when the household wants to exit the commune. Thus, only a fraction of wealth,  $(1 - \lambda_{ip})x_{ip}$ , is transferable and available to fund

migration. We assume that the distribution of this parameter is independent of  $x$ . Averaging across all households in a province, we approximately obtain  $(1 - \lambda^{\circ}_p)\mu$  as the average amount a household could transfer to finance migration.

Those whose liquid wealth is below the cost of migration (net of subsidies) cannot migrate; such households will find it beneficial to migrate but they are not able to finance it.<sup>25</sup> We can then interpret  $\xi_{ip} = (C_p - S_i) / (1 - \lambda^{\circ}_p)$  broadly as the average budget constraint that governs the migration decision for either subsidized or unsubsidized migrants. An increase in subsidies (an increase in  $S$ ) and greater land liquidity (a decrease in  $\lambda^{\circ}$ ) both slacken this constraint. To ensure that migration is attractive for at least part of the population in a province, we assume that  $\nu_{\text{Mig}} > \xi_{ip}$  for each province.<sup>26</sup> Now we can determine the share of the population that migrates, given by  $F(\nu_{\text{Mig}} > x \geq \xi; \mu, \sigma)$ , and formulate our main prediction:

**Prediction 1 (land liquidity).** The proportion of migrants from a province is positively related to the expected liquidity of land. In particular,  $\partial F(\nu_{\text{Mig}} > x \geq \xi; \mu, \sigma) / \partial \lambda^{\circ} < 0$ .

So far we have developed the model in terms of the financing effect of land liquidity. We can also think of  $x_{ip}$  as a variable that represents the opportunity cost of migrating. In this alternative interpretation,  $\lambda$  still represents a friction in the labor allocation decision but now functions as the likelihood that the household loses its land if labor is allocated in Asia. Here, one can immediately see the source of the identification problem mentioned in the introduction. We have defined the value of migration in relative terms, but  $\xi$  is defined in real terms. While both effects lead to an expansion in the distance between  $\nu_{\text{Mig}}$  and  $\xi$ , and hence an increase in migration, we cannot determine whether, relative to  $x$ , it is  $\nu_{\text{Mig}}$  that is moving to the right or  $\xi$  is moving to the left. We have argued in the historical section that the financing effect is more likely, and we will keep with this interpretation when describing the model, but it is important to be clear that these two effects have very different interpretations about which households are migrating.

The actual effect of the reform depends upon on how many peasants take advantage of the reform by exiting the commune and converting their claim to an allotment to an individual title to land. We assume that exiting improves the expected liquidity of wealth by decreasing  $\lambda^{\circ}_R$  to  $\lambda^{\circ}_{ER}$ , where  $ER$  refers to a household that has exited a repartition commune. As discussed in the historical section, exiting may increase or decrease expected wealth relative to the value of migrating. Let  $L_{ip}(x_{ip})$  be the value of the expected wealth if household  $i$  exits the commune with wealth  $x_{ip}$ , where  $L_{ip}(x_{ip}) > x_{ip}$  or  $L_{ip}(x_{ip}) < x_{ip}$ . The function  $L$  captures the institutional changes that result from exiting households. On one hand, expected wealth may increase because of better property rights but, on the other hand, expected wealth may decrease because of reform-induced conflict or the fact that some services of the commune have been crippled. We must index on  $i$  because  $x$  is forward-looking, whereas  $L$  may depend on the particular history of the household. To simplify matters, we assume that exiting does not alter  $x$  for those who intend to migrate relative to the value of migrating. Only the households with  $L_{ip}(x_{ip}) > x_{ip}$  or  $\nu_{\text{Mig}} > x_{ip}$  will exit. Therefore, Prediction 1 is no longer sufficient to determine the effect of the reform on the outcome of interest.

Peasants who want to exit face two different types of costs. The first is a direct cost,  $c$ , consisting of survey and basic administrative costs and does not vary by household or province. The second cost comes from

<sup>24</sup> Yang (1997), Mullen et al (2011), and De Brauw and Mueller (2011) all have models that incorporate tenure security and allow for changes in land rights to affect the opportunity cost of migrating. In their models, expropriation risk functions as a tax on migration. These papers also discuss how rights to rent can finance migration, but none focus on the institutional aspects of the liquidity constraint explicitly.

<sup>25</sup> For simplicity, we assume that this transferable wealth does not affect the expected payoff from migrating. One can think of the surplus  $(1 - \lambda_{ip})x_{ip} - C_p$  as either fully depreciating upon arrival in Siberia, i.e., those migrating households with better  $x$  simply enjoyed a more comfortable journey, or these funds going into a common pool for insurance purposes after settlement in the East.

<sup>26</sup> This assumption can easily be relaxed without changing the qualitative results. We use it here to simplify the exposition of the model.

the bureaucratic red tape involved in the processing of the exit application. We choose to model this cost,  $\kappa_p$ , as a probability that the liquidity constraint will not relax after the decision to exit. This measure of bureaucratic red tape only operates through the budget constraint and captures the differential cost that liquidity-constrained potential migrants face when they have to delay their trip because the title is not ready.<sup>27</sup> The measure varies by province and is independent of  $x_{ip}$ , so we can rewrite the effect of this cost in terms of the budget constraint, i.e., instead of  $\xi_{iER}$  the household faces  $\kappa_p \xi_{iR} + (1 - \kappa_p) \xi_{iER}$  in expectation.<sup>28</sup>

The decision to exit depends upon the relative value of  $L_{ip}(x_{ip})$  and  $\nu_{Mig}$ . If  $L_{ip}(x_{ip}) > \nu_{Mig}$ , then the household exits when  $L_{ip}(x_{ip}) - c > x_{ip}$ . If  $L_{ip}(x_{ip}) < \nu_{Mig}$ , then the household will exit when  $\nu_{Mig} - c > x_{ip}$  and  $x_{ip} \geq \kappa_p \xi_{iR} + (1 - \kappa_p) \xi_{iER}$ . Now we can state the second prediction necessary for our empirical analysis that bureaucratic red tape provides a source of exogenous variation in the number of exiting households.

**Prediction 2 (red tape).** The proportion of those exiting the commune in a province is negatively related to the amount of bureaucratic red tape. In particular,  $\partial F(\nu_{Mig} > x \geq \xi; \mu, \sigma) / \partial \kappa < 0$ .

This prediction easily follows from the fact that an increase in  $\kappa_p$  tightens the constraint on the exit decision.

Without making further assumptions on the nature of  $L_{ip}$ , we cannot apply Prediction 1 in general. Nevertheless, it is instructive to consider the simple case when  $L_{ip}$  is the identity function and Prediction 1 is applicable. In this case, only those households that want to migrate will exit since the cost to exit is positive and exiting has no impact on  $x$ . Even in this case, not all those who decide to exit will migrate because the liquidity constraint might fail to relax in time, causing household  $i$  to instead face  $C_p + c - S_i > (1 - \lambda_R)x_{ip}$ ; neither will all those who exit in time to migrate eventually migrate, since it is possible that the realization of liquidity constraint turns out to be a high draw. However, on average, the reform will increase migration from repartition provinces.

The agrarian reform may also have an impact on the distribution of the value of not migrating relative to the value of migration. When exiting does impact  $x$ , the distribution of  $x$  after all exit decisions is not a straightforward calculation. The composition of exiting households will in general differ from the composition of migrating households. For example, consider the simple case when  $L(x) = x + d$  where  $d > 0$  due to the productivity effect. The inframarginal migrants will no longer want to migrate, and consequently not all those that decide to exit will migrate. In this case, one can show under reasonable assumptions, i.e.,  $x$  is normally distributed and a large share of households lies to the left of the budget constraint, that whatever the proportion of households who plan to exit the expected number of migrants would increase. Through aggregation to the province level, we might obtain  $L$  functions similar to identity function or the simple affine transformation above. We note that all these assumption affect which households migrate, implying there are important distributional issues at stake that ideally would be explored in a more complete model. We do not present a more complete model here because these functions are not observable with our data and we lack a model of the commune from which to infer what these  $L$  functions

would look like. Since the trade-offs crucially depend on how the  $L$  functions are specified, we choose to leave the validity of Prediction 1 as an empirical matter.

While this setup is simpler than previous models on land rights and migration, we can still incorporate the relevant features of other models. In the literature, the primary feature of land rights is tenure security, modeled as a probability the household retains possession of its allotment. In the Russian Empire, tenure security of households within the commune was rather high if households followed the rules. We model this conditional tenure security,  $\gamma_{ip}$ , as the probability that the household will retain its allotment upon returning to the commune.<sup>29</sup> We also assume that since this change in tenure security only affects migrants, changes in this parameter do not affect  $x$ .<sup>30</sup> Since, in expected terms, when temporary migration is optimal, return migration allows the household to reenter the commune, possibly with a profit  $\varphi$  from the commune allotment. Thus, the value of migration for temporary migrants who return at date  $T$  is  $\nu(x_{ip}, \gamma_{ip}) = \nu_{Mig} [1 - (\delta - \delta^T)] / (1 - \delta) + (\gamma_{ip} x_{ip} + \varphi) \delta^T / (1 - \delta)$ , where  $\delta$  is the discount factor. For simplicity, we assume that  $T$  is determined exogenously by other factors.

**Prediction 3 (tenure security).** The proportion of migrants in a province is positively related to the strength of tenure security. In particular,  $\partial F(\nu > x \geq \xi; \mu, \sigma) / \partial \gamma > 0$ .

Note that  $\partial^2 F(\nu > x \geq \xi; \mu, \sigma) / \partial \gamma \partial T < 0$ . The greater the tenure security effect is, the more temporary migration is, i.e., the lower  $T$  is. In particular, at the limit when  $T$  tends to infinity, as would be the case for permanent migrants, the tenure security parameter plays no role. Therefore, since we want to study permanent migration, we do not focus on the tenure security parameter.

#### 4.2. Hypotheses

Using the model, we can derive several hypotheses concerning the impact of the land titling reform on migration patterns. The first hypothesis comes as no surprise: the titling reform should have a stronger effect on migration for repartition provinces than nonrepartition provinces. The titling reform exogenously increases land liquidity for repartition provinces while holding the land liquidity of nonrepartition provinces untouched. The right to exit the commune with an individual allotment that the peasant can more easily sell or rent out improves the budget constraint for all repartition provinces by shifting  $\lambda^\circ$  from  $\lambda_R^\circ$  to  $\lambda_{AR}^\circ$ , where the subscript  $AR$  refers to households in repartition provinces with the option to exit the commune as a result of the agrarian reform. In practice, we only know that for each province  $\lambda_R^\circ \geq \lambda_{AR}^\circ$ . The inequality is not strict because peasants may not take advantage of the reform. However, we argue that  $\lambda_R^\circ > \lambda_{AR}^\circ$  and correspondingly  $\xi_R > \xi_{AR}$  because at least some peasants were eager to take advantage of the reform. Early on, the impact of the reform on expected wealth in the absence of migration is likely to be small especially for potential migrants, meaning that  $F$  is a reasonable approximation at least in the short-run. Thus, from Prediction 1, on average, the reform should increase migration.

What we do not know is the relative magnitude of the impact of the land titling reform on migration, which should depend on the

<sup>27</sup> If the measure of bureaucratic red tape were to affect all exiting households, the cost of exiting would increase resulting in a possible decrease in the number of exiting households, although the proportion of exiting households that migrate would increase. Since the measure only affects households that intend to migrate, this assumption is consistent with our empirical approach of using IV to estimate the local average treatment effect (LATE).

<sup>28</sup> We have not modeled time, but understanding what we mean by red tape is the easiest using time periods. In the period that the household wants to migrate, there is a probability that the application will not be processed in time and the migrating household will have to wait until the next period to exit.

<sup>29</sup> Mullen et al (2011) demonstrate that tenure security can have both a positive and a negative effect on temporary migration. The negative effect comes from a general expropriation parameter whereby less expropriation leads to greater land in expectation and, due to the complementarity in labor and land, migration is less attractive. In our model, we do not have a general expropriation parameter and, hence, do not have this negative effect. Property rights are secure within the commune, and tenure insecurity is contingent on the decision to migrate.

<sup>30</sup> This assumption is reasonable for the short-run with low migration flows or with migrants who are myopic.



household decision to exit the commune. Thus, our second hypothesis is the relative magnitude of the impact of the reform on migration which positively depends on the decision to exit the commune. The greater number of households that exit the commune would correspond to a greater number of liquidity constraints relaxed. According to Prediction 1 again, we would expect to see relatively more migration from provinces with relatively more households that exit. However, the exit variable presents an econometric problem, since unobservables may drive the decision to exit and the decision to migrate. Using Prediction 2, we can make use of an exogenous and observable source of variation to apply an instrumental variable technique to estimate the effect of exits on migration. The testing of these first two hypotheses establishes our basic results.

The next two hypotheses serve to check our interpretation of the basic results as a land liquidity effect. We first turn to the difference between subsidized and unsubsidized migrants, since the latter group should not have been affected by the contemporaneous changes in the migration policy. Since subsidies did not vary by province, they could be differenced out in the standard way, but liquidity constraints complicate the empirical analysis. To account for a shift in migration policy toward more generous subsidies per household, we denote (dropping for a moment the province type)  $\xi_{Spre}$  and  $\xi_{Spost}$  for budget constraints that subsidized migrants faced before and after the reform accordingly, with  $\xi_{Spre} > \xi_{Spost}$ . For repartition provinces, we know  $\xi_R > \xi_{R+Spre} > \xi_{R+Spost} \geq \xi_{AR+Spost}$  and  $\xi_R \geq \xi_{AR} > \xi_{Spost+AR}$ . For provinces without repartition communes, the only change that coincides with the land titling reform is for subsidized migrants with  $\xi_{NR+Spre} > \xi_{NR+Spost}$ . Difference-in-differences analysis is problematic for subsidized migrants because we might have  $\xi_{NR+Spre} - \xi_{NR+Spost} < \xi_{R+Spre} - \xi_{R+Spost}$ . We address this issue by focusing on unsubsidized migrants, making our third hypothesis that the observed effect of the reform on migration holds for unsubsidized migrants.<sup>31</sup>

An important hypothesis that cannot be derived from the model but may allow us to distinguish between the opportunity cost and financing effects is that the opportunity cost effect should be similar for subsidized and unsubsidized migrants whereas the financing effect is less pronounced for subsidized migrants. We also expect that the income effect is positive for both types of migrants but smaller for subsidized migrants given relatively large amount of subsidies per household both before and after the reform (see the Historical section for details).<sup>32</sup> Under liquidity constraints, as wealth increases, at some point, migration will respond dramatically in a positive way, and then gradually decrease in response, eventually becoming an inferior good. Hence, the income effect due to the migration policy and the Stolypin land titling reform are more ambiguous.

Taking into consideration the previous literature's emphasis on tenure security, we arrive at our fourth and final hypothesis: the positive effect of tenure security on migration is stronger for temporary migrants than permanent ones, as demonstrated in Prediction 3. Empirically, we use two measures of temporary migration, seasonal migration and short-run migration, to separate the tenure security story from the land liquidity story for Europe–Asia migration.

## 5. Data

We combine several sources to construct a panel dataset on regional migration from fifty European provinces to the Asian part of Russian Empire before and after the Stolypin reform. The bulk of our data come from the official periodical publications by various imperial authorities that normally reported current statistics at the province level.

First, we use the Resettlement Administration migration statistics (Turchaninov, 1910, 1915). This administration registered both subsidized and unsubsidized migrants as well as delegates when they passed through (in both directions) two key railway stations of the trans-Siberian railroad, namely Syzran and Chelyabinsk.<sup>33</sup> Since the trans-Siberian railroad was basically the only transport for migrants to get to their destinations, they could not bypass these stations.<sup>34</sup> The government demanded that migrants register, and they had incentives to do this, because registration affected their access to canteens, and medical, bath, and laundry services, which the government provided for them during their move, regardless of their status. We count the number of migrants in households because our main independent variable of interest, exits from the commune, is measured in households. We also use a cumulative migration variable (a running sum of total migration). The data also allow us to construct two measures of temporary migration, seasonal migration (households that migrated to the East and back within one year) and short-run migration (return migration minus seasonal migration). For return migration, we do not know when the migrants originally left for the East. As a result, we produce two long-term migration variables; the first is prospective, defined as total migration minus seasonal migration, based on the assumption that migrants to the East who were not seasonal expected the move to be permanent, and the second is retrospective, defined as prospective long-term migration minus the short-term migration lead, based on the assumption that migrants were able to decide within one period whether the permanent move was a mistake.

Second, we extract information on peasants' applications to convert their land titles and the actual number of exits from an official periodical journal published by the Ministry of Internal Affairs (*Izvestiya zemskogo otdela MVD*). The government needed information about success of the Stolypin reform and demanded provincial governors to report exit statistics regularly to the center (Ministry of Internal Affairs 1912, Vol. 3, p. 106). Using actual exits in the current period relative to the total stock of unsatisfied applications to exit, we construct a confirmation rate for exits that reflects the efficiency of bureaucracy in a province or the absence of red tape during the implementation of the reform.<sup>35</sup>

Third, we gather information on sales of privatized peasant plots in repartition communes from annual statistical volumes of the Ministry of Justice for 1907–1909 and 1914, we know only aggregate figures of sales in both repartition and hereditary communes; we reconstruct repartition sales applying their 1910–1913 shares among total sales, since historians argue that there was little variation over time (Dubrovskii, 1963, p. 360).<sup>36</sup> Notaries had to register all peasant land sales under the 1906 decree and report them to the government. Informal land transactions between peasants within the commune were obviously not counted in this registry, neither before nor after the reform,

<sup>31</sup> If the underlying  $F$  distributions are different in the treatment and control group, we may observe a larger increase in migration in repartition provinces from subsidies. For example, the scale of the distribution of the treatment provinces may be compressed relative to the control provinces given that one role of the commune was to ensure minimum living conditions for all.

<sup>32</sup> The generous financial support after 1906 may have decreased the share of potential migrants who would eventually migrate unsubsidized. If subsidies were less likely to cover the financing gap for migration in repartition provinces, then the reduction in the pool of unsubsidized migrants would be disproportionately smaller for repartition provinces. This story would generate a negative treatment effect for subsidized migrants, something we do not observe.

<sup>33</sup> Delegates were peasants who traveled to the Asian provinces to collect information and to enlist allotments before the whole family would migrate.

<sup>34</sup> Only migrants traveled via Tyumen's station and those who traveled by ocean vessels from Odessa to Vladivostok via Indian and Pacific oceans were outside of this registration procedure, but their number was small (Dubrovskii, 1963, p. 389; Tukavkin, 2001, p. 250).

<sup>35</sup> We employ a value of zero for the confirmation rate in the provinces not affected by the reform because there was no reform-specific bureaucracy.

<sup>36</sup> It might be, however, that there were more repartition sales in the early years of the reform, given fewer restrictions on land transfers in hereditary communes before the reform.

**Table 1**

The Stolypin reform, migration to the East, and provincial economic performance, 1896–1914.

Variable	Obs	Mean	Std. dev.	Min	Max
All migrants (hh per 1000 rural pop.)	349	0.38	0.51	0	3.07
Subsidized migrants (hh per 1000 rural pop.)	349	0.23	0.35	0	2.71
Unsubsidized migrants (hh per 1000 rural pop.)	349	0.15	0.24	0	2.14
Repartition province	350	0.82	0.38	0	1
Post	350	0.86	0.35	0	1
Repartition province * Post	350	0.7	0.46	0	1
Exits (hh per 1000 rural pop.)	327	2.81	5.36	0	38.86
Number of exit applications per 1000 rural pop.	350	7.09	8.39	0	42.7
Confirmation rate	348	0.19	0.22	0	0.98
Population (1000)	350	2442.48	901.11	450.35	4890.25
Rural population density	350	45.94	22.39	4.69	114.94
Livestock per 1000 rural pop.	350	558.72	190.8	305	1320
Yield (tons/ha)	350	0.88	0.23	0.16	1.59
Urban share of population	350	0.13	0.12	0.02	0.74
Rural wage (rubles per harvest month)	344	30.16	8.95	15.75	65.22
Urban wage (rubles per month)	347	19.89	4.56	9.33	31.29
Railway tariff (kopeks)	50	361.2	76.41	130	475
All return migrants (hh per 1000 rural pop.)	341	0.05	0.07	0	0.63
Subsidized return migrants (hh per 1000 rural pop.)	341	0.02	0.03	0	0.31
All seasonal migrants (hh per 1000 rural pop.)	341	0.02	0.02	0	0.18
Subsidized seasonal migrants (hh per 1000 rural pop.)	341	0.01	0.01	0	0.17
Short-term migrants (hh per 1000 rural pop.)	341	0.04	0.06	0	0.61
Long-term migrants, prospective (hh per 1000 rural pop.)	340	0.35	0.47	0	2.88
Long-term migrants, retrospective (hh per 1000 rural pop.)	290	0.32	0.45	0	2.83
Cumulative migrating households	348	7945.57	8462.51	0	42253.17
Delegates per 1000 rural pop.	342	0.66	0.83	0	6.12
Privatized land sales per 1000 rural pop.	337	0.98	1.6	0	14.09
Land consolidations per 1000 rural pop.	350	2.54	2.8	0	14.79
Zemstvo province	350	0.71	0.46	0	1
South Union province	50	0.18	0.38	0	1
Transatlantic province	50	0.26	0.44	0	1
1897 share of Orthodox	50	0.78	0.27	0.30	0.99
1897 share of old believers	50	0.019	0.02	0	0.73
Private land Gini in 1905	47	0.83	0.08	0.6	0.96
Share of private land in 1905	47	0.39	0.15	0.04	0.74
Share of commune land in 1905	47	0.46	0.13	0.11	0.72
Peasant share of private land in 1905	47	0.14	0.08	0	0.39
Pre-1897 migrants per 1000 population in 1897	50	13.26	12.17	1.95	40.30
Violent unrests in 1901–1904	47	8.79	9.97	1	44
Land captains in 1906	47	53.81	20.84	8	106
Higher education graduates per 1000 pop. in 1897	50	11.21	7.95	0.17	45.81
Russian–Japanese War recruits in 1904–05 per 1000 rural pop.	50	5.66	2.52	0.79	16.33

worsening the quality of this data. While we will use land sales as a direct test of the financing mechanism, one can also think of this variable as a measure of the extent of the land market. Finally, we collect data on a number of control variables from various sources. Table A1 in Appendix A provides the full list of variables from our dataset and sources; a detailed description of each variable follows this table.

We construct seven cross-sections, one before and six after the reform: 1901–1906, 1907, 1908–1909, 1910–1911, 1912, 1913, and 1914, i.e., 350 province-period observations estimated as annual averages in total. The availability of exit statistics, which were published irregularly, determines the construction of the reform periods. The prereform period is sufficiently longer because we do not have annual data on many controls for these years, only for particular years.<sup>37</sup> For the majority of variables, we also have information for an additional prereform period, 1896–1900, with the important exception of rural wages. To make our variables scale-free, we divide them by rural population in a province where necessary. The controls constructed using the 1897 population census and 1905 land census, the South Union and transatlantic dummies<sup>38</sup> and the variables on railway tariff, 1901–1904

peasant violent unrests, the number of land captains in 1906 and the number of Russian–Japanese War recruits are time invariant. Table 1 presents the summary statistics of our sample. Approximately 2.5 million citizens lived in an average province. Orthodox Christians were the dominant religious group with nearly 78% of the population in an average European province, but there were provinces where their number was small. Old-believers and Jews represented substantial religious minorities. Out of this population, on average about 3.7 families migrated annually per 10,000 people between 1900 and 1914 and about twice this rate for delegates. About 60% of migrants received subsidies from the government. The province with the largest migration rate produced eight times more migrating households than an average province, and there were several provinces without any migrants at all. On average, the distance to Chelyabinsk was not correlated with number of migrants per capita in a province (correlation coefficient of  $-0.02$ ). Before the reform, the number of unsubsidized migrants per capita was negatively associated with the distance to Chelyabinsk (correlation coefficient of  $-0.39$ ,  $p$ -value of .01); however, after the reform, this association dramatically weakened (correlation coefficient of  $-0.25$ ,  $p$ -value of .08), consistent with the idea that binding liquidity constraints were relaxed with the reform. Return migration was close to one household per 20,000 people annually, i.e., roughly 14% of those who migrated to the East; this figure was even lower among subsidized migrants (10%). Seasonal migration to the East was about 1.5 households per 100,000 people annually.

Around 25 households per 10,000 people converted their titles and exited the commune annually, if one considers all provinces during

<sup>37</sup> In addition, 1904 and 1905 were years of abnormally low migration to the East; the government prevented migration due to the need to use the trans-Siberian railroad to transport regiments to the front of the 1904–1905 Russian–Japanese War.

<sup>38</sup> Note that *zemstvo* dummy varies over time since *zemstvos* were established in six provinces in 1911.

the whole period under study. For the postreform reform-affected provinces, this figure was 40 households per 10,000 people on average. Similarly, 70 and 100 households per 10,000 people applied for exits; 25 and 35 per 10,000 applied for plot consolidation, and almost 15 per 10,000 sold their plots. Exit and sale figures were about ten times higher in the peak years in the provinces that were among the leaders of the reform, indicating substantial variation in reform variables.

Our main control variables are rural population density, agricultural yield per capita, number of livestock per capita, population share in urban areas, urban wages, rural wages, and the railway tariff. Average rural population density was quite high, at forty-six inhabitants per square kilometer. The Yield and Livestock variables proxy for average income and assets available to migrants. For every 1000 peasants in an average province, there were 558 cows and horses; average yield was about 0.87 tons/ha. The level of urbanization was very low, 13% on average. Urban wages were on average about 20 rubles per month; rural wages were generally lower, but during the

harvest season (for which we have data) they were higher, reaching 30 rubles per month. In addition to the main controls, we use several variables on land institutions. According to the 1905 land census, communes possessed 46% of land, 39% was in private ownership, and the remainder was state-owned. In an average province, the Gini index of the distribution of private land was more than 0.8, and only 13% of private land belonged to peasants. Openly violent conflicts over land were not rare; we have data only for 1900–1904, when their number was about nine per province.

## 6. Analysis

### 6.1. The econometric specification

Since the provinces without repartition communes were relatively unaffected by the land titling reform, the most natural approach is to obtain a difference-in-differences estimate of the effect of the reform.

**Table 2**

The effect of the reform on migration (per capita).

Dependent variable	All migrants per capita				Exits per capita
	Diff-n-Diff	Pooled OLS	Fixed effects		
	(1)	(2)	(3)	(4)	
Repartition province * Post	0.20*** [0.058]	0.17*** [0.056]	0.21*** [0.07]		3.81*** [0.589]
Repartition province * 1907 period dummy				0.34** [0.167]	
Repartition province * 1908–09 period dummy				0.55*** [0.135]	
Repartition province * 1910–11 period dummy				0.07 [0.047]	
Repartition province * 1912 period dummy				0.1 [0.059]	
Repartition province * 1913 period dummy				0.14** [0.066]	
Repartition province * 1914 period dummy				0.06 [0.053]	
Post	0.16** [0.078]	0.05 [0.078]			
Repartition province	0.14 [0.095]	0.27** [0.11]			
Rural population density	0.01*** [0.002]	0.00 [0.003]	−0.02* [0.008]	−0.02* [0.009]	0.05 [0.080]
Livestock	0.00* [0.000]	−0.00 [0.000]	−0.00 [0.001]	−0.00 [0.000]	−0.01 [0.004]
Yield	−0.24 [0.174]	−0.39** [0.185]	−0.14 [0.204]	−0.13 [0.206]	−1.79 [1.949]
Urban share of population	−0.05 [0.405]	−0.03 [0.154]	0.09 [1.613]	0.04 [1.654]	38.55** [15.151]
Rural wage	−0.01* [0.005]	−0.02*** [0.005]	−0.02*** [0.005]	−0.02*** [0.005]	0.02 [0.071]
Urban wage	−0.02** [0.009]	−0.01 [0.008]	0.02 [0.013]	0.02 [0.014]	−0.02 [0.145]
Railway tariff	0.00 [0.001]	−0.00 [0.001]			
Period effects	No	No	Yes	Yes	Yes
Region effects	No	Yes	No	No	No
Province effects	No	No	Yes	Yes	Yes
Constant	−0.04 [0.304]	1.00*** [0.307]	1.83*** [0.664]	1.91*** [0.713]	−2.96 [6.598]
Observations	340	340	340	340	320
R-squared	0.233	0.404	0.385	0.415	0.532
Number of provinces			50	50	47

Notes: Results are for migrant households per 1000 rural inhabitants in columns 1–4 and household exits from the commune per 1000 rural inhabitants in column 5. The variable *Repartition province* is the treatment indicator variable = 1 if the observation is a province affected by the reform and = 0 otherwise. The variable *Post* is a dummy variable = 1 if the observation is from a period after Nov. 1906 and = 0 otherwise. The variable *Railway tariff* does not vary over time. Province effects are individual fixed effects whereas region effects are group fixed effects. Clustered-robust standard errors are in brackets.

\* p-Value < 0.1.

\*\* p-Value < 0.05.

\*\*\* p-Value < 0.01.



Tables A2 and A3 in Appendix A provide summary statistics for provinces affected and not affected by the reform, separately. There is no significant variation in controls between provinces with and without the reform, although variation in migration is large and it grows strongly after the reform. The migration patterns leading up to the reform give no reason to question the parallel trend assumption, which we will discuss in more detail in Table 4. The regression-adjusted difference-in-differences model can be formulated by the following equation:

$$\text{Migration}_{it} = \alpha + \beta * \text{Repartition}_i + d * \text{Post}_t + \gamma * \text{Repartition} * \text{Post}_{it} + \phi X_{it} + \varepsilon_{it} \quad (1)$$

where *Migration* is the number of migrants per thousand citizens, *Repartition* is a dummy indicating those provinces affected by the reform, *Post* is a time dummy to reflect the occurrence of the reform,  $X_{it}$  is the set of control variables discussed in the previous section, and  $\varepsilon_{it}$  is a random disturbance. The coefficient  $\gamma$  is the effect of interest,

and we expect it to be positive. We will run several modifications of Eq. (1). In particular, we replace *Post* and *Repartition* with period and province fixed effects in our preferred specification.

Eq. (1) is in levels and not in logs because the main variable of interest is a dummy variable that changes over time. This implies that the unit of measurement of this effect is fixed while the outcome variable's unit is variable. Moreover, an initial spike in migration, possibly due to the removal of liquidity constraints, will necessarily reduce the magnitude of subsequent jumps, artificially dampening the effect of the fixed-unit treatment variable. For completeness, we estimate Eq. (1) in logs and report results in the appendix (Table A4).

The specification in Eq. (2) provides a more direct measure of the reform, based on the intensity of exiting households within the treatment provinces.

$$\text{Migration}_{it} = \alpha + \beta * \text{Number of exits}_{it} + \phi X_{it} + u_i + \tau_t + \varepsilon_{it}. \quad (2)$$

**Table 3**  
The effect of the reform on subsidized and unsubsidized migration (per capita).

Dependent variable	Subsidized migrants per capita			Unsubsidized migrants per capita		
	OLS	FE	FE	OLS	FE	FE
	(1)	(2)	(3)	(4)	(5)	(6)
Repartition province * Post	0.07 [0.041]	0.1 [0.060]		0.1*** [0.021]	0.11*** [0.018]	
Repartition province * 1907 period dummy			0.23 [0.146]			0.1*** [0.034]
Repartition province * 1908–09 period dummy			0.16* [0.089]			0.39*** [0.074]
Repartition province * 1910–11 period dummy			0.03 [0.036]			0.04*** [0.018]
Repartition province * 1912 period dummy			0.07 [0.039]			0.03 [0.022]
Repartition province * 1913 period dummy			0.1* [0.052]			0.04*** [0.018]
Repartition province * 1914 period dummy			0.02 [0.042]			0.04* [0.02]
Post	0.05 [0.05]			0.00 [0.037]		
Repartition province	0.25*** [0.088]			0.02 [0.038]		
Rural population density	0.00 [0.002]	−0.01 [0.007]	−0.01 [0.007]	−0.00 [0.001]	−0.01* [0.003]	−0.01* [0.004]
Livestock	−0.00 [0.000]	−0.00 [0.000]	−0.00 [0.000]	−0.00 [0.000]	−0.00* [0.000]	−0.00* [0.000]
Yield	−0.36** [0.144]	−0.17 [0.156]	−0.20 [0.159]	−0.03 [0.067]	0.02 [0.072]	0.07 [0.073]
Urban share of population	−0.01 [0.108]	−0.68 [1.314]	−0.56 [1.359]	−0.02 [0.067]	0.77 [0.505]	0.59 [0.479]
Rural wage	−0.01*** [0.003]	−0.01*** [0.004]	−0.01*** [0.004]	−0.00 [0.003]	−0.01* [0.003]	−0.01*** [0.003]
Urban wage	−0.00 [0.006]	0.01 [0.011]	0.01 [0.012]	−0.01* [0.003]	0.01* [0.005]	0.01** [0.005]
Railway tariff	0.00 [0.000]			−0.00 [0.001]		
Period effects	No	Yes	Yes	No	Yes	Yes
Region effects	Yes	No	No	Yes	No	No
Province effects	No	Yes	Yes	No	Yes	Yes
Constant	0.51** [0.206]	1.31*** [0.466]	1.35** [0.506]	0.49*** [0.156]	0.52* [0.306]	0.56* [0.329]
Observations	340	340	340	340	340	340
R-squared	0.432	0.319	0.330	0.271	0.374	0.439
Number of provinces		50	50		50	50

Notes: Results are for subsidized migrant households per 1000 rural inhabitants in columns 1–3 and unsubsidized migrant households per 1000 rural inhabitants in column 4–6. The variable *Repartition province* is the treatment indicator variable = 1 if the observation is a province affected by the reform and = 0 otherwise. The variable *Post* is a dummy variable = 1 if the observation is from a period after Nov. 1906 and = 0 otherwise. The variable *Railway tariff* does not vary over time. Province effects are individual fixed effects whereas region effects are group fixed effects. Clustered-robust standard errors in are brackets.

\* p-Value < 0.1.

\*\* p-Value < 0.05.

\*\*\* p-Value < 0.01.

We include year and province fixed effects along with our controls in the preferred specification. Here, the effect of interest,  $\beta$ , should more precisely identify the impact of the reform through the mechanism we have in mind.

The primary concern with Eq. (2) is the endogeneity of the exit decision, either from reverse causality or omitted variable bias. We cannot simply use the reform to generate an instrumental variable because of the changing migration policy. Instead, we instrument for the number of exits with the proportion of applications for title conversion that were quickly confirmed by the local authorities (*Confirmation rate*), a measure of bureaucratic quality or inverse red tape. This instrument should be positively related to the number of exiting households and should not correlate with unobservable variables that affect the migration decision, since this specific measure of bureaucratic quality emerges as a necessary feature of the reform and was largely predetermined by the historical distribution of public officials who were subsequently entrusted with the task to implement the reform.

We reestimate Eq. (2) using 2SLS with the first stage as in Eq. (3):

$$\text{Number of exits}_{it} = \alpha + \beta * [\text{Confirmation rate}]_{it} + \phi X_{it} + u_i + \tau_t + h_{it} \quad (3)$$

To appease concerns about the instrument validity, we can include into Eq. (3) two determinants of bureaucratic efficiency, the quantity and quality of these public officials, represented by the number of *zemsky nachalniki* on the eve of the reform and the number of graduates of secondary and tertiary education taken from the 1897 population census. In this case,  $u_i$  will revert to regional fixed effects, since these measures of quantity and quality of land captains are time-invariant.

We also run the specifications in Eqs. (1), (2), and (3) for subsidized and unsubsidized migrants and in Eqs. (2) and (3) for temporary migrants.

## 6.2. Empirical results

Moving directly to the punch line, our estimates suggest that the Stolypin land titling reform had a strong positive effect on migration. We obtain per year, per province estimates that there are between 415 and 513 households that migrate in response to the reform, meaning as many as 170,000 households migrated due to the Stolypin land titling reform, i.e., 39% of all 441,000 households that migrated after the reform. We argue that a significant portion of this effect can be understood as a direct effect of households' ability to obtain an individual title for their communal land allotments. Our estimates show that, on average, for each 1000 title conversions, about thirty to forty

**Table 4**  
Pretrend analysis: the effect of the reform on migration (per capita).

Dependent variable	Migration per capita					
	All migrants			All migrants		
	Subsidized migrants			Subsidized migrants		
	Unsubsidized migrants			Unsubsidized migrants		
	Pooled OLS			Fixed effects		
	(1)	(2)	(3)	(4)	(5)	(6)
Repartition province * Post				0.14*	0.05	0.09***
				[0.076]	[0.055]	[0.03]
Repartition province * Pre	−0.1*	−0.07*	−0.03	−0.09	−0.06*	−0.02
	[0.055]	[0.034]	[0.031]	[0.054]	[0.035]	[0.027]
Pre	−0.04	−0.02	−0.02			
	[0.046]	[0.025]	[0.028]			
Repartition province	0.18**	0.12**	0.06*			
	[0.074]	[0.054]	[0.034]			
Rural population density	0.00*	0.00*	0.00	−0.00	−0.00	0.00
	[0.002]	[0.001]	[0.001]	[0.005]	[0.003]	[0.002]
Livestock	−0.00	0.00	−0.00	−0.00	−0.00	−0.00**
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Yield	−0.17**	−0.14**	−0.04	−0.16	−0.20*	0.04
	[0.086]	[0.058]	[0.044]	[0.145]	[0.114]	[0.054]
Urban share of population	−0.11	−0.04	−0.07	0.59	−0.08	0.67
	[0.096]	[0.047]	[0.063]	[1.295]	[0.891]	[0.514]
Urban wage	0.00	0.00	−0.00	0.01	0.01	−0.00
	[0.006]	[0.004]	[0.003]	[0.010]	[0.008]	[0.005]
Railway tariff	−0.00	0.00	−0.00			
	[0.000]	[0.000]	[0.000]			
Period effects	No	No	No	Yes	Yes	Yes
Region effects	Yes	Yes	Yes	No	No	No
Province effects	No	No	No	Yes	Yes	Yes
Sample	Pre-reform only			All periods		
Constant	0.12	−0.13	0.25	0.31	0.22	0.09
	[0.276]	[0.143]	[0.18]	[0.355]	[0.241]	[0.154]
Observations	100	100	100	396	396	396
R-squared	0.614	0.633	0.456	0.342	0.281	0.345
Number of provinces				50	50	50

Notes: Results are for all migrant households per 1000 rural inhabitants in columns 1 and 4, subsidized migrant households per 1000 rural inhabitants in columns 2 and 5, and unsubsidized migrant households per 1000 rural inhabitants in column 3 and 6. The variable *Repartition province* is the treatment indicator variable = 1 if the observation is a province affected by the reform and = 0 otherwise. The variable *Post* is a dummy variable = 1 if the observation is from a period after Nov. 1906 and = 0 otherwise. The variable *Pre* is a dummy variable = 1 if the observation is from the period 1901–1906 and = 0 otherwise. The variable *Railway tariff* does not vary over time. The “pre-reform only” sample includes only the observation form of the 1896–1900 and 1901–1906 periods. The “all periods” sample adds the observation form of the 1896–1900 period to the main sample. Province effects are individual fixed effects whereas region effects are group fixed effects. Clustered-robust standard errors are in brackets.

\* p-Value < 0.1.

\*\* p-Value < 0.05.

\*\*\* p-Value < 0.01.

households subsequently migrate. This proportion is at least fifteen times higher than the proportion of migrants in the population. This sudden jump in migration makes sense if households faced financial constraints. Our estimates imply that around 80,000 migrating households can be attributed to title conversion, or nearly 50% of the total effect of the reform.

We first discuss the difference-in-differences estimates in Table 2, presented with and without regional dummies in columns 1 and 2. The coefficient of interest is positive and significant in both columns, although it decreases by around 15% when we include regional dummies. Wages, yields, urban share, and rural population density each have the intuitive sign. In column 1, one may take issue with the positive and significant coefficient on Livestock since there should be more horses and cows in prosperous regions. However, if liquidity constraints mattered, livestock could be used to finance migration. The effect of Livestock completely disappears in column 2, suggesting that regional variation, perhaps in wealth, fully explains the positive coefficient. In column 3, we switch to our preferred specification, with fixed effects and year effects. The estimated coefficient of interest states that 513 households per year, per province migrated in response to the reform and is significant at the 1% level. The coefficient on rural density changes signs. This could reflect the fact that, once we control for

province fixed effects, this variable no longer tracks overpopulated areas but relatively more attractive agricultural areas. All other controls have similar signs or become statistically insignificant. Next, we allow for the effect of the reform to vary over time to analyze the effect of the reform in the short run. Column 4 shows a stronger and statistically significant effect for the year following the reform and, even larger in the next two subsequent years, 1908 and 1909, peaking at a yearly per province average of 1344 households migrating in response to the reform in this two-year period; in following years, the effect of the reform decreases. The estimates support the interpretation that the land liquidity effect dominates the productivity effect in the short run. In column 5, we verify that the reform did indeed have a strong and statistically significant impact on the number of exits from repartition communes, predicting around 3 million exits during the postreform period.

Since, as described in the historical section, the migration policy occurred concurrently with the Stolypin land titling reform, we estimate the effect of the reform separately for subsidized and unsubsidized migrants. Continuing with the difference-in-differences estimates, columns 1 and 4 of Table 3 show that the effect of the reform is positive but insignificant for subsidized migrants, and positive and significant for unsubsidized migrants. In column 1, the additional insignificant

**Table 5**  
The effect of exit from the commune on migration per capita.

Dependent variable	All migrants Per capita		Exits	All migrants			
	Pooled	Fixed effects		IV fixed	IV	Fixed	IV fixed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Exits	0.04*** [0.008]	0.03*** [0.008]		0.03** [0.013]	0.04*** [0.013]	0.03*** [0.008]	0.03** [0.012]
Repartition province	0.24** [0.102]				0.24** [0.111]		
Land sales						0.04* [0.02]	0.03* [0.019]
Repartition province * Post				0.07 [0.064]			
Confirmation rate			20.01*** [3.16]				
Rural density	−0.00 [0.002]	−0.02*** [0.007]	0.01 [0.093]	−0.02** [0.007]	0.00 [0.002]	−0.02** [0.007]	−0.02** [0.007]
Livestock	−0.00 [0.000]	−0.00*** [0.001]	0.00 [0.004]	−0.00*** [0.001]	−0.00 [0.000]	−0.00*** [0.001]	−0.00*** [0.001]
Yield	−0.22 [0.146]	−0.16 [0.195]	0.32 [1.247]	−0.14 [0.191]	−0.23 [0.147]	−0.14 [0.193]	−0.12 [0.190]
Urban share of population	−0.14 [0.118]	−0.47 [1.289]	11.17 [11.082]	0.08 [1.441]	0.25 [1.139]	−0.79 [1.228]	−0.22 [1.412]
Rural wage	−0.01** [0.004]	−0.02*** [0.005]	0.05 [0.053]	−0.02*** [0.005]	−0.01** [0.004]	−0.02*** [0.006]	−0.02*** [0.005]
Urban wage	0.00 [0.007]	0.01 [0.012]	−0.07 [0.124]	0.01 [0.012]	0.00 [0.007]	0.00 [0.012]	0.00 [0.012]
Railway tariff	0.00 [0.001]				0.00 [0.001]		
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region effects	Yes	No	No	No	Yes	No	No
Province effects	No	Yes	Yes	Yes	No	Yes	Yes
1st-stage F-statistic				37.01	43.84		
Constant	0.53** [0.234]	2.78*** [0.624]	−3.07 [6.230]			2.78*** [0.618]	
Observations	319	319	319	318	318	318	317
R-squared	0.594	0.481	0.712	0.473	0.428	0.489	0.481
Number of provinces		47	47	47		47	47

Notes: Results are for migrants per 1000 rural inhabitants, except for the first stage results for the number of household exits from the commune per 1000 rural inhabitants in column 3. The variable *Repartition province* is the treatment indicator variable = 1 if the observation is a province affected by the reform and = 0 otherwise. The variable *Post* is a dummy variable = 1 if the observation is from a period after Nov. 1906 and = 0 otherwise. The variable *Railway tariff* does not vary over time. Column 5 includes two additional time-invariant control variables designed to capture unobserved differences in bureaucratic efficiency. These variables are the number of *zemsky nachalniki* and the number of graduates of higher education. Province effects are individual fixed effects whereas region effects are group fixed effects. Clustered-robust standard errors are in brackets.

\* p-Value < 0.1.

\*\* p-Value < 0.05.

\*\*\* p-Value < 0.01.



**Table 6**

The role of exit from the commune for subsidized and unsubsidized migration (per capita).

Dependent variable	Subsidized migrants per capita		Unsubsidized migrants per capita	
	Fixed effects	IV fixed effects	Fixed effects	IV fixed effects
	(1)	(2)	(3)	(4)
Exits	0.01*	0.02	0.018***	0.014**
	[0.008]	[0.011]	[0.006]	[0.006]
Repartition province * Post	0.02	0.02	0.03	0.05**
	[0.051]	[0.053]	[0.026]	[0.021]
Rural density	−0.01*	−0.01*	−0.01*	−0.01*
	[0.006]	[0.006]	[0.004]	[0.004]
Livestock	−0.00*	−0.00**	−0.00***	−0.00***
	[0.000]	[0.000]	[0.000]	[0.000]
Yield	−0.19	−0.16	0.03	0.02
	[0.153]	[0.153]	[0.064]	[0.063]
Urban share of population	−0.91	−0.37	0.43	0.45
	[1.201]	[1.155]	[0.609]	[0.618]
Rural wage	−0.02***	−0.02***	−0.01**	−0.01**
	[0.004]	[0.004]	[0.003]	[0.003]
Urban wage	0.00	0.00	0.01	0.01*
	[0.011]	[0.010]	[0.005]	[0.005]
Period effects	Yes	Yes	Yes	Yes
Province effects	Yes	Yes	Yes	Yes
Constant	1.87***		0.87**	
	[0.452]		[0.331]	
Hansen's J Statistic (p-value)				
Observations	319	318	319	318
R-squared	0.367	0.351	0.48	0.478
Number of provinces	47	47	47	47

Notes: Results are for subsidized migrant households per 1000 rural inhabitants in columns 1 and 2 and for unsubsidized migrant households per 1000 rural inhabitants in column 3 and 4. The variable *Repartition province* is the treatment indicator variable = 1 if the observation is a province affected by the reform and = 0 otherwise. The variable *Post* is a dummy variable = 1 if the observation is from a period after Nov. 1906 and = 0 otherwise. The instrumental variable is *Confirmation rate* in columns 2 and 4. Province effects are individual fixed effects whereas region effects are group fixed effects. Clustered-robust standard errors are in brackets.

\* p-Value &lt; 0.1.

\*\* p-Value &lt; 0.05.

\*\*\* p-Value &lt; 0.01.

coefficient on the reform-year dummy suggests that subsidized migrants may not have faced financial constraints, either before or after 1906, since neither the land titling reform nor the more generous governmental subsidies substantially affected subsidized migrants flow. From a policy perspective, there was no need to have both increased subsidies and the titling reform to ease peasants' budget constraints; the titling reform alone was already sufficient. Columns 2 and 5, for subsidized migrants and unsubsidized migrants, respectively, show that province fixed effects do not alter the results. The remaining columns, 3 and 6, allow the effect of the reform to vary by year. The results for unsubsidized migrants conform to the results in Table 2, showing a positive and significant effect for the two periods that follow the reform and then a diminishing effect for subsequent years. The results of this table support that what we are identifying is the effect of the agrarian reform on migration and not simply the effect of the migration policy. We can also speculate that the financing effect mattered more, since both types of migrants experienced changes in the opportunity cost.

Before leaving the discussion of the difference-in-differences estimates, we would like to discuss the results of the pretrend analysis. For the pretrend analysis, we include an additional prereform period (1896–1900). In columns 1–3 of Table 4, we move the difference-in-differences analysis back one period and exclude all postreform periods to check for a trend occurring before the reform. For all, subsidized and unsubsidized migrants, there is no statistically significant positive trend occurring before the reform. The negative coefficient can be explained by the introduction of the railroad, which equalized provincial migration patterns. In columns 4–6 of Table 4, using all the data, we show the results of running our preferred specification (same as column 3 in Table 2) adding the prereform interaction term between repartition province and the dummy indicating the

1901–1906 period. The coefficient giving the effect of the reform is positive and statistically significant for unsubsidized and all migrants, although smaller in size. As with the previous pretrend analysis, the coefficient on the interaction term is negative, but is not significant at the 5% level. Thus, the pretrend analysis further substantiates our regression-adjusted difference-in-differences approach.

We now turn to the estimates of the direct mechanism based on the number of exits. Column 1 of Table 5 gives the estimates of pooled OLS, including the repartition-province dummy. The coefficient on the number of exits is positive and significant. The magnitude is consistent with the estimates given by the difference-in-differences analysis. For example, in 1908, there were close to 500,000 exits, predicting that 20,000 households would migrate in that year as a result of the reform. Column 2 presents our preferred specification, which includes year and province fixed effects. The effect suggests that for every 1000 exits, thirty households migrate. This marginal propensity to migrate is fifteen times higher than the migration rate among the population. Running the specifications in logs reports an elasticity of 0.05 for all migrants and 0.04 for unsubsidized migrants (shown in columns 5 and 6 of Table A4).

In columns 3 and 4 of Table 5, we explore the potential endogeneity problem of the observed correlation between peasant migration and exits from the commune. Column 3 reports the first stage regression. The F-statistic of 37.01 suggests that there is enough explanatory power to use *Confirmation rate*.<sup>39</sup> The second stage results in column 4

<sup>39</sup> In the pooled OLS IV regression for which we can introduce all the control variables, the first stage F-statistic is 28.33 with a partial R-squared of 0.3178, which is similar to the partial R-squared of exits in the pooled OLS IV specification with the basic set of controls.

show that the coefficient on exits remains positive and significant and the size of the coefficient is stable. Since the IV procedure consistently estimates the LATE, we interpret this effect of exits not for the average household but for the average household whose behavior is affected by bureaucratic red tape, perhaps because the household is liquidity-constrained. The stability of the effect may indicate then that the average potential migrating household is liquidity-constrained. To strengthen the IV validity claim, column 5 adds the two variables that determine bureaucratic efficiency as controls (here, fixed effects are replaced with regional dummies). The effect of exits is again stable.

Next, we incorporate land sales using our preferred fixed effects and IV fixed effect regressions. Ideally, we would also instrument for land sales, but it is difficult to find a variable that satisfies the necessary exclusion restriction once we also instrument for exits. Columns 6 and 7 of Table 5 show that selling repartition plots positively predicts migration patterns. The positive and significant coefficient on land sales suggests that the financing effect matters. The estimates of the effect of exits are robust to the inclusion of land sales, giving reason to suspect that opportunity-cost effect also influenced migration. The standard deviation of exits is five times larger than the standard deviation of land sales, supporting the relative magnitude of the effect of exits. Ultimately, though, this evidence is not conclusive, because the data on land sales are incomplete and not entirely representative (see Section 5 above). At the very least, we can view land sales as a measure of the extent of the land market and hence provide more robust evidence of the effect of the reform.

The effect of exits on migration for subsidized and unsubsidized migration follows the same pattern as the difference-in-differences

results. Columns 1 and 3 of Table 6 repeat column 3 of Table 5 for subsidized and unsubsidized migrants. Again, we see that the reform has a positive and significant effect and this effect is larger for the unsubsidized migrants. While the magnitude of the coefficient is smaller than for all migrants, in terms of elasticity the effect is just as high for unsubsidized migrants. These results hold up when we turn to the instrumental variable estimates, shown in columns 2 and 4 of Table 6, repeating column 5 of Table 5 for each group.

We now address whether the reform increased migration indirectly through better tenure security. As discussed in Section 4, the tenure security effect should matter more for temporary migration. Table 7 shows the results for seasonal migrants, shown in column 1, the coefficient on exits is positive and significant, but much smaller in magnitude, both in absolute and relative terms. We also look at subsidized seasonal migrants because seasonal migrants may have simply migrated in order to obtain subsidies; column 2 shows that the effect of exits on seasonal migration is not driven by subsidies. For short-term migrants, we include both one- and two-period lagged exits, shown in columns 3 and 4, since short-term migrants by definition did not migrate in the current period. For short-term migration, the one-period lag has a positive and significant coefficient. In absolute terms, the size of the effect is nearly 10 times less than the effect on long-term migrants; in relative terms, however, the size of the effects are similar since there is much less short-term migration. We present the results for our two different measures of long-term migrants, prospectively defined in column 5 and retrospectively defined in column 6. In both cases, the effect for long-term migrants is positive and significant, as expected. Thus, there is some evidence for a tenure security effect; however, the effects are

Table 7

The role of exits from the commune for seasonal, short-term and long-term migration.

Dependent variable	Seasonal migrants per capita		Short-term migrants per capita		Long-term migrants per capita (prospective)	Long-term migrants per capita (retrospective)
	All	Subsidized	All	All	All	All
	Fixed effects	Fixed effects	Fixed effects	Fixed effects	Fixed effects	Fixed effects
	(1)	(2)	(3)	(4)	(5)	(6)
Exits	0.001*** [0.000]	0.00 [0.000]			0.03*** [0.007]	0.03*** [0.006]
One-period lagged exits			0.003*** [0.001]	0.004*** [0.001]		
Two-period lagged exits				0.00 [0.001]		
Rural population density	−0.00*** [0.000]	−0.00** [0.000]	−0.00 [0.001]	−0.00 [0.001]	−0.01** [0.006]	−0.01** [0.006]
Livestock	−0.00*** [0.000]	−0.00*** [0.000]	−0.00** [0.000]	−0.00** [0.000]	−0.00*** [0.001]	−0.00*** [0.001]
Yield	0.00 [0.008]	0.00 [0.006]	0.02 [0.030]	0.00 [0.036]	−0.10 [0.200]	−0.02 [0.194]
Urban share of population	0.07 [0.094]	0.06 [0.077]	0.34 [0.213]	0.31 [0.215]	−0.28 [1.218]	0.16 [1.473]
Rural wage	0.00 [0.000]	−0.00 [0.000]	0.00 [0.001]	0.00 [0.002]	−0.02*** [0.004]	−0.01*** [0.005]
Urban wage	0.00 [0.001]	−0.00 [0.000]	0.01*** [0.002]	0.01** [0.003]	0.01 [0.011]	0.01 [0.013]
Period effects	Yes	Yes	Yes	Yes	Yes	Yes
Province effects	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.11*** [0.030]	0.07*** [0.021]	−0.07 [0.093]	−0.06 [0.081]	2.46*** [0.552]	1.88*** [0.586]
Observations	311	311	312	267	310	267
R-squared	0.391	0.177	0.394	0.395	0.455	0.429
Number of provinces	47	47	47	47	47	47

Notes: Results are for seasonal migrant households per 1000 rural inhabitants in column 1, seasonal subsidized migrant households per 1000 rural inhabitants in column 2, short-term migrant households per 1000 rural inhabitants in columns 3 and 4, and long-term migrant households per 1000 rural inhabitants in columns 5 and 6. Province effects are individual fixed effects whereas region effects are group fixed effects. Clustered-robust standard errors are in brackets.

\* p-Value < 0.1.

\*\* p-Value < 0.05.

\*\*\* p-Value < 0.01.

**Table 8**

Robustness checks: additional control variables and alternative treatment and control groups.

Dependent variable	Main result with additional controls		Excluding Baltic and Lithuanian provinces		Expanded control group < 50% repartition commune + Cossack provinces	
	AllM	UnsM	AllM	UnsM	AllM	UnsM
	Pooled OLS					
	(1)	(2)	(3)	(4)	(5)	(6)
Repartition province * Post	0.18** [0.079]	0.10*** [0.025]	0.13 [0.083]	0.10*** [0.026]	0.12* [0.067]	0.09*** [0.027]
Repartition province	0.25 [0.159]	−0.04 [0.063]	0.30** [0.124]	0.02 [0.043]	0.19* [0.114]	0.09* [0.050]
Post	−0.02 [0.108]	−0.05 [0.048]	0.07 [0.110]	−0.00 [0.048]	0.09 [0.082]	0.02 [0.036]
Livestock	−0.00 [0.004]	−0.00 [0.002]	0.00 [0.003]	−0.00 [0.001]	0.00 [0.003]	−0.00 [0.001]
Rural population density	−0.00 [0.000]	−0.00 [0.000]	−0.00 [0.000]	−0.00 [0.000]	−0.00 [0.000]	0.00 [0.000]
Yield	−0.41* [0.202]	0.04 [0.061]	−0.40** [0.195]	−0.03 [0.072]	−0.25 [0.180]	0.06 [0.057]
Urban share of population	0.34* [0.202]	0.30** [0.112]	−0.02 [0.151]	−0.02 [0.068]	0.00 [0.154]	−0.01 [0.060]
Urban wage	−0.01 [0.010]	−0.00 [0.004]	−0.01 [0.008]	−0.01* [0.003]	−0.01 [0.008]	−0.01** [0.003]
Rural wage	−0.02*** [0.006]	−0.01** [0.003]	−0.02*** [0.006]	−0.00 [0.003]	−0.02*** [0.004]	−0.01** [0.002]
Railway tariff	−0.00 [0.001]	−0.00 [0.001]	−0.00 [0.001]	−0.00 [0.001]	−0.00 [0.001]	−0.00 [0.001]
1897 share of Orthodox	−0.33 [0.255]	−0.05 [0.130]				
1897 share of old believers	−2.67 [2.263]	−1.07 [1.179]				
Private land Gini in 1905	0.91 [0.763]	0.71* [0.351]				
Share of private land in 1905	0.90*** [0.326]	0.39** [0.151]				
Share of commune land in 1905	0.78* [0.392]	0.48** [0.205]				
Peasant share of private land in 1905	2.01** [0.765]	1.50*** [0.367]				
Pre-1897 migrants	0.01*** [0.003]	0.01*** [0.002]				
Violent unrests during 1901–04	0.00 [0.005]	0.00* [0.003]				
Region effects	Yes	Yes	Yes	Yes	Yes	Yes
Constant	−0.12 [0.898]	−0.48 [0.419]	1.05*** [0.346]	0.52*** [0.167]	0.76* [0.422]	0.22 [0.174]
Observations	295	295	307	307	340	340
R-squared	0.459	0.361	0.388	0.248	0.407	0.296

Notes: Results are for all migrant households (AllM) per 1000 rural inhabitants in columns 1, 3 and 5 and for unsubsidized migrant households (UnsM) per 1000 rural inhabitants in columns 2, 4 and 6. The variable *Repartition province* is the treatment indicator variable = 1 if the observation is a province affected by the reform and = 0 otherwise. The variable *Post* is a dummy variable = 1 if the observation is from a period after Nov. 1906 and = 0 otherwise. In columns 3 and 4, we exclude the Baltic and Lithuanian provinces from the control group. In columns 5 and 6, we remove the Cossack provinces and provinces with less than 50% of the peasant population in repartition communes from the treatment group and place them instead in the control group. Province effects are individual fixed effects whereas region effects are group fixed effects. Clustered-robust standard errors are in brackets.

\* p-Value < 0.1.

\*\* p-Value < 0.05.

\*\*\* p-Value < 0.01.

very small and cannot explain the main results. To be fair, the tenure security effect is likely to operate more on rural–urban migration for which we do not have data.

### 6.3. Robustness checks and alternative hypotheses

The first robustness check that we employ is to add a battery of additional controls, addressing concerns about omitted-variable bias. We include variables describing religion, the share of private land, private land inequality, peasants' share of private land, and the share of commune land, peasant violence, and previous migration to Asian provinces. The results are reported for all migrants and unsubsidized migrants in

columns 1 and 2 of Table 8. The effect of the reform is slightly diminished but remains positive and significant for both groups of migrants. One control in particular is worth discussing further. The coefficient on violence is positive and statistically significant for unsubsidized migrants.<sup>40</sup>

<sup>40</sup> We do not include this variable as a control in our preferred specifications for two reasons. First, missing data would reduce our sample size by nearly 30%. Second, accounting for the effects of this variable, while important in our context, is a historical peculiarity and not a variable that we would prima facie include.



While the revolution affected provinces with both repartition and hereditary communes alike, one could argue that peasant violence reflected underlying dissatisfaction, which may have been worse in provinces with repartition communes due to greater restrictions on peasant behavior. Since peasant dissatisfaction with the commune may also have influenced migration, we may have observed a spurious relationship between repartition communes and migration that coincides with the surge in peasant discontent. Our IV results, of course, show this is not the case; however, for thoroughness, if we include an interaction term between exits and peasant violence, this term (not reported) has an insignificant but positive coefficient, while the coefficient on exits is still positive and significant.

For the second robustness check, we modify the control and treatment groups. We first exclude the Baltic and Lithuanian provinces (five of the original nine) from the control group. We rerun the basic specification for all migrants and unsubsidized migrants. The results are presented in Table 8, columns 3 and 4. The effect of the reform is diminished for both types of migrants, but remains significant for the unsubsidized migrants. We next use a stricter definition of treatment by including only those provinces with more than 50% of peasants located in repartition communes and extending the control group to include the Cossack provinces. The Cossack population in these provinces faced very different tenure

rules and, as a consequence, these provinces did not face the same kind of changes due to the reform. In total, the control group increases by a set of seven provinces. Augmenting the control group also goes some way toward addressing the poor power of the statistical tests using unbalanced treatment and control groups. In columns 5 and 6 of Table 8, we see that the effect of the reform is positive and significant for both all migrants and unsubsidized migrants.

For our third robustness check, we explore several placebo treatments. First, we run two placebo regressions based on a “treatment” of migration promotion (both monetary and nonmonetary). The institution that promoted migration most heavily was *zemstvo*, a form of local governance, and its correlation with repartition provinces is high (0.74). We show the results of rerunning the main specifications for all migrants and subsidized migrants in columns 1 and 2 of Table 9. This placebo has no effect for all migrants or subsidized ones. The negative and significant coefficient on *zemstvo* could be attributed to better local governance in these provinces. Since not all *zemstvo* provinces were equally active in promoting migration, we run an additional placebo regression using only those *zemstvo* provinces that were known to contribute monetarily to promote subsidized migration (the South Union). The coefficient on the placebo interaction term is positive and

**Table 9**  
Placebo regressions of promotional treatment and the Gerschenkron hypothesis.

Dependent variable	AIIM Pooled OLS (1)	SM Pooled OLS (2)	AIIM Pooled OLS (3)	AIIM Pooled OLS (4)	UnsM Pooled OLS (5)	AIIM Pooled OLS (6)	AIIM Pooled OLS (7)	AIIM Pooled OLS (8)	AIIM Pooled OLS (9)	AIIM Pooled OLS (10)
Zemstvo province * Post	−0.01 [0.139]	−0.10 [0.120]								
South Union province * Post			0.28*** [0.082]	0.27*** [0.077]	0.09** [0.041]					
Repartition province * Post				0.15** [0.060]	0.10*** [0.021]		0.19*** [0.059]			0.16*** [0.056]
Repartition province				0.24* [0.122]	−0.00 [0.049]		0.34*** [0.099]			0.29*** [0.112]
Zemstvo province	−0.30** [0.136]	−0.25** [0.098]								
South Union province			0.03 [0.103]	0.00 [0.112]	0.06 [0.055]					
Russo–Japanese War recruits * Post						0.01 [0.016]	0.01 [0.018]			
Russo–Japanese War recruits						0.04** [0.016]	0.04** [0.018]			
Not a transatlantic province * Post								0.06 [0.082]		
Not a transatlantic province								−0.03 [0.116]		
Urban share * Post									−0.52** [0.204]	−0.49** [0.199]
Urban share									0.45** [0.172]	0.41** [0.187]
Post	0.19 [0.140]	0.16 [0.116]	0.11 [0.077]	0.00 [0.077]	−0.02 [0.033]	0.12 [0.114]	−0.03 [0.129]	0.14 [0.106]	0.24** [0.091]	0.12 [0.086]
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	No	No	No	No	No	No	No	No	No	No
Region effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	1.82*** [0.435]	1.25*** [0.325]	1.27*** [0.335]	0.96*** [0.3]	0.46*** [0.155]	1.04*** [0.364]	0.54 [0.324]	1.37*** [0.406]	1.31*** [0.339]	0.94*** [0.304]
Observations	340	340	340	340	340	340	340	340	340	340
R-squared	0.403	0.458	0.408	0.423	0.293	0.409	0.437	0.385	0.386	0.406

Notes: Results are for all migrant households (AIIM) per 1000 rural inhabitants in columns 1, 3, 4 and 6–10, for subsidized migrant households (SM) per 1000 rural inhabitants in column 2, and for unsubsidized migrant households (UnsM) per 1000 rural inhabitants in column 5. The variable *Repartition province* is the treatment indicator variable = 1 if the observation is a province affected by the reform and = 0 otherwise. The variable *Post* is a dummy variable = 1 if the observation is from a period after Nov. 1906 and = 0 otherwise. Basic controls are *Livestock*, *Rural population density*, *Yield*, *Urban wage*, *Rural wage*, *Urban share of population* and *Railway tariff*. Province effects are individual fixed effects whereas region effects are group fixed effects. Clustered-robust standard errors are in brackets.

\* p-Value < 0.1.

\*\* p-Value < 0.05.

\*\*\* p-Value < 0.01.

**Table 10**  
Alternative hypotheses.

Dependent variable	AllM	UnsM	AllM	UnsM	AllM	UnsM	AllM	UnsM	AllM	UnsM
	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exits	0.03*** [0.006]	0.02*** [0.006]	0.03*** [0.008]	0.02*** [0.006]	0.03*** [0.008]	0.02*** [0.006]	0.03*** [0.008]	0.02*** [0.007]	0.03*** [0.008]	0.02*** [0.006]
Delegates	0.43*** [0.044]	0.12*** [0.031]								
Pre-1897 migrants * Post			0.01*** [0.003]	0.01*** [0.001]						
Consolidations					0.01 [0.012]	−0.00 [0.005]				
Private land Gini in 1905 * Post							0.73* [0.426]	0.34* [0.195]		
Cumulative migrants									−0.00*** [0.000]	−0.00*** [0.000]
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	1.74*** [0.514]	0.53* [0.296]	2.43*** [0.696]	0.62* [0.338]	2.65*** [0.644]	0.79** [0.340]	2.03*** [0.685]	0.50 [0.335]	1.50** [0.637]	0.38 [0.342]
Observations	312	312	319	319	319	319	312	312	314	314
R-squared	0.791	0.579	0.490	0.499	0.484	0.480	0.484	0.480	0.540	0.509
	47	47	47	47	47	47	46	46	47	47

Notes: Results are for all migrant households (AllM) per 1000 rural inhabitants in columns 1, 3, 5, 7 and 9, and for unsubsidized migrant households (UnsM) per 1000 rural inhabitants in columns 2, 4, 6, 8 and 10. The variable *Repartition province* is the treatment indicator variable = 1 if the observation is a province affected by the reform and = 0 otherwise. The variable *Post* is a dummy variable = 1 if the observation is from a period after Nov. 1906 and = 0 otherwise. Basic controls are *Livestock*, *Rural population density*, *Yeild*, *Urban wage*, *Rural wage*, and *Urban share of population*. Province effects are individual fixed effects whereas region effects are group fixed effects. Clustered-robust standard errors are in brackets.

\* p-Value < 0.1.

\*\* p-Value < 0.05.

\*\*\* p-Value < 0.01.

significant for all migrants. Given these results, we include the reform interaction term in columns 4 and 5 for both all migrants and unsubsidized migrants. The reform effect is still positive and significant and of similar magnitude, demonstrating the reform had an independent effect from the migration policy. For the second placebo, we verify that the Russian–Japanese War and the information about the Asian provinces that soldiers could have gained while on the battlefield do not drive our findings. In column 6, using the measure of recruits from each province interacted with the postreform dummy, we see no evidence that the increase in migration after the reform is due to the Russian–Japanese War. However, the coefficient on the recruit variable is positive and significant and may be correlated with unobservables that influence the migration decision. In column 7, we show that the effect of the reform does not change when we include the Russian–Japanese placebo variables. For the final placebo treatment, we use provinces that experienced little to no transatlantic migration. Column 8 of Table 9 reports no effect of this treatment. This result is not surprising given the fact that most of those who did migrate drew from urban areas not to mention that peasants would have had great difficulty financing transatlantic migration.<sup>41</sup>

We now turn to discuss alternative hypotheses alluded to in the historical section. First, using an interaction between urban share and the postreform time dummy, we check whether the Gerschenkron hypothesis, that the reform increased rural–urban migration, affects our results. If

the Gerschenkron hypothesis is true, our results are strengthened or weakened depending on whether rural–urban and rural–rural migrations are substitutes or complements. Greater rural–urban migration outflows could reduce the pool of potential migrants to the East, making our results underestimate the reform's effect, or they could cripple the commune, making Europe–Asia migration more attractive. In fact, we observe a negative and statistically significant coefficient on the interaction term between urban share and the postreform period (column 9), suggesting that the reform-induced rural–urban migration substituted for migration to the East. However, we note that Gerschenkron claims are validated, since the reform dramatically affected the rural–urban migration patterns. To verify that the nature of migration outflows to urban areas does not explain our earlier results, in column 10, we include repartition province and the repartition, postreform period interaction. The estimated coefficient of interest remains positive and statistically significant, although the magnitude diminishes slightly.

Second, historical anecdotes suggest that the most industrious tended to migrate, making it necessary to explore the issue of positive selection (Tukavkin, 2001). We could misinterpret the effect of the reform if selection is correlated with the timing and implementation of the reform, which could happen if repartition provinces responded differently to the migration policy. The pure selection story would hold if industrious peasants in repartition provinces responded more to the increase in subsidies, which could happen if industrious peasants have not been compensated for their efforts or abilities in the repartition commune. When industrious peasants migrate, the commune becomes less attractive for the remaining peasants. This story is inconsistent with the results in Table 5, which show that the initial jump is mainly due to unsubsidized migrants. Positive selection may still interact with the opportunity cost effect. Human capital reduces the relative magnitude of the opportunity cost effect attributed to land liquidity, because human capital is more mobile than land. Thus, on top of subsidies

<sup>41</sup> We also use Jewish share of the population (1897 census) interacted with the reform as a placebo and, similarly, find no effect (results are not reported).

reducing the financing effect, positively selected subsidized migrants would also see less of an opportunity cost effect. An additional selection issue is that unsubsidized migrants had worse access to destination plots. Since one would expect the repartition communes to have more equally distributed plots, this type of selection would bias our results downward.

Lastly, we explore three remaining alternative hypotheses concerning the reform effect: informational and migration network differences, reform-induced tensions, and agglomeration effects. First, in columns 1 and 2 of Table 10, we include the number of delegates to account for informational flows, and the coefficient is positive and significant for both types of migrants. Recall that unsubsidized migrants (in column 2) were not required to send delegates, so information appears to matter. The evidence on the Russian–Japanese War recruits also speaks to the importance of information. In columns 3 and 4, we include an interaction term between the postreform dummy and the total number of migrants from a province living in Asian provinces in 1897, signifying provincial differences in information and migration networks. Again, we see a positive and significant coefficient on this variable, implying that better information and networks improved migration flows. Second, we discuss the commune tension hypothesis motivated by the arguments in Pallot (1999). We use two controls, both chosen to capture how the reform might affect reform-related tensions in the countryside, the number of land consolidations and land inequality of private land in 1905 interacted with the postreform dummy. In columns 5 and 6, the land consolidation variable has no effect, suggesting that tensions within the commune likely did not result in greater propensities to migrate. Land inequality, on the other hand, in columns 7 and 8, is positive and significant. Greater private land inequality likely meant a stronger agricultural elite who could better resist industrialization (Galor et al., 2009).<sup>42</sup> In these provinces, work options outside of the commune would be limited and, in response to the reform, peasants would be more likely to prefer to migrate to the Asian provinces. Lastly, to imperfectly address the agglomeration hypothesis, we simply include the total number of previous migrants in a province as a control. In columns 9 and 10, we see that this actually has a negative effect, suggesting that agglomeration effects are not at work. While evidence is consistent with both the information and tensions hypotheses, we want to underline the fact that the coefficient on exits remains unaffected in all specifications in Table 10 and unreported IV estimates.

## 7. Conclusion

This paper contributes to three different literatures, the literature on migration and economic development, the literature on the effects of land titling, and the historical literature on the Stolypin reform. First, the unique nature of the Stolypin land titling reform permits the identification of an important factor in the migration decision, land liquidity. Our main finding is that greater land liquidity, stemming from the Stolypin reform, promoted migration by easing financial constraints and decreasing opportunity costs. This result adds to the understanding of migration because an improvement in land markets leads to migration outflows, a result that contrasts with the new economics of migration literature's usual argument that outflows should be curbed with improvements in poorly developed markets, although their focus has been primarily on capital or insurance and not land markets (Halliday, 2006; Rosenzweig and Stark, 1989).

Second, the literature on land titling argues that individual land titles influence the decision to migrate. However, the previous literature mostly emphasizes the effect of land titles on tenure security. We distinguish between the tenure security and land liquidity effects by demonstrating that the liquidity effect on permanent migration is substantially larger than the tenure security effect on temporary migration. The magnitude of the effect of the reform on permanent migration is large and land title conversions under the reform explain at least 18.1% of internal Asiatic migration in late Imperial Russia. Our back-of-the-envelope calculation estimates the overall impact of the reform through the reallocation of labor to the East to be approximately 0.65% of annual national GDP or 12% of GDP growth during the reform period.<sup>43</sup>

Third, our findings address the economic history literature on the Stolypin reform. Traditionally, the primary aspects of the reform discussed by historians are the creation of private property, the fostering of land productivity, and the releasing of labor from agriculture to industry (Gerschenkron, 1962, 1965). We add to this understanding of the Stolypin agrarian reform by showing how the reform reallocated labor within the agricultural sector. The effect of the reform on migration was crucial for its rapid expansion during the years before World War I, and the previous literature, with the exception of Tukavkin (2001), tends to overlook the mutually reinforcing nature of the imperial policy objectives, namely improving agricultural productivity and encouraging migration (Dubrovskii, 1963; Williams, 2006; Zyrianov, 1992). Moreover, our estimates show that the migration policy was actually less important than the land titling reform for Asiatic migration.

We conclude with a final remark on the direction of future research concerning land liquidity. In addition to a direct effect on resource allocation, land liquidity can indirectly influence the growth process in a path dependent way. Do and Iyer (2008) show that individual land titles increase rural labor devoted to nonfarm activities, anticipating the possibility that land liquidity could affect the choice of nonagricultural projects in both rural and urban areas. One could speculate that changes in land liquidity could impact the industrialization and urbanization process more broadly. Indeed, when credit markets are limited, greater land liquidity enables migrants to depend less on wage income and to engage in more self-financed projects. Taken in this light, our results apply more generally to modern developing economies with restrictive land rights, such as China and Mexico.

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<sup>42</sup> Note that this measure of land inequality is estimated for private lands only, i.e. it does not account for inequality in peasant communal holdings.

<sup>43</sup> About 2.65 million people, or about 1.5% of total population of the empire, migrated to the East during the eight years after the reform's implementation. Using the average returns to migration in terms of land (335%) and assuming similar land productivity in the Asian and European parts of Russia, the contribution of the reform would be 0.64% per year or approximately 5.12% of GDP over the whole period. Since GDP growth over this period was 42.87% (Gregory, 2004), the reform accounted for 12% of this growth.



## Appendix A

Table A1

Data sources.

Variable name	Variable definition	Sources
<i>All migrants</i>	Number of households migrating to the East from a province per 1000 rural population	Turchaninov (1910, 1915)
<i>Subsidized migrants</i>	<i>All migrants</i> having official permits and able to receive subsidies	
<i>Unsubsidized migrants</i>	<i>All migrants</i> minus <i>Subsidized migrants</i> <sup>a</sup>	
<i>All return migrants</i>	Number of households returning to home provinces per 1000 rural population	
<i>Subsidized return migrants</i>	<i>All return migrants</i> having official permits and able to receive subsidies	
<i>All seasonal migrants</i>	<i>All return migrants</i> returning to home provinces within the same year as departure	
<i>Subsidized seasonal migrants</i>	<i>All seasonal migrants</i> having official permits and able to receive subsidies	
<i>Short-term migrants</i>	<i>All return migrants</i> minus <i>all seasonal migrants</i> <sup>b</sup>	
<i>Long-term migrants, prospective</i>	<i>All migrants</i> minus <i>all seasonal migrants</i> <sup>b</sup>	
<i>Long-term migrants retrospective</i>	<i>Long-term migrants; prospective</i> minus number of the lead of short-term migrants <sup>b</sup>	
<i>Delegates</i>	Number of delegates ( <i>khodaks</i> ) who traveled to the East (to collect information and to enlist an allotment for future resettlement) from a province per 1000 rural population	
<i>Exits</i> <sup>c</sup>	Number of households with a land title conversion from the commune in a province per 1000 rural population	Ministry of Internal Affairs 1908–1914
<i>Number of applications to exit</i> <sup>d</sup>	Number of applications for a land title conversion in a province per 1000 rural population	
<i>Confirmation rate</i>	<i>Exits</i> divided by cumulative stock of unsatisfied applications in a province (constructed with the cumulative <i>number of applications to exit</i> subtracting out previous period <i>exits</i> )	
<i>Land consolidations</i>	Number of households that consolidated their plots in a province per 1000 rural population	Chief administration of agriculture and land engineering (1908–1912, 1911, 1913–1915)
<i>Population</i>	Population in thousands on January 1 of each year in a province	'Electronic depository of Russian historical statistics' dataset; Central Statistical Committee of the Ministry of Interior Affairs (1902, 1903, 1905–1916) <sup>e</sup>
<i>Rural population density</i>	Rural population per square kilometer on January 1 of each year in a province	
<i>Urban share of population</i>	Share of urban population in a province	
<i>Livestock</i>	Number of horses and cows in a province per 1000 rural population	
<i>Yield</i>	Grain yield in tons per hectare	
<i>Zemstvo province</i>	Zemstvo dummy for provinces with elected local governments ( <i>zemstvos</i> )	
<i>Regional fixed effects</i>	13 regional dummies (each region is a group of provinces) <sup>f</sup>	
<i>Privatized land sales</i>	Number of privatized land sales in the repartition commune in a province per 1000 rural population	Ministry of Justice 1907–1915
<i>South Union province</i>	South Union dummy for provinces where <i>zemstvos</i> united into the "South Union" to promote migration	Zenchenko (1912)
<i>Transatlantic province</i>	Transatlantic dummy for provinces with "high" (according to Obolensky outflow of trans-Atlantic migrants)	Obolensky (1928)
<i>Urban wage</i>	Average monthly industrial wage in rubles in an "industrial" region (each region is a group of provinces)	Ministry of Finance (1904–1915)
<i>Rural wage</i>	Average monthly wage in rubles in agriculture during the harvest season	Ministry of Agriculture. Department of agricultural economics and statistics, 1906–1914
<i>Railway tariff</i>	Railway tariff per person in kopeks to get to Chelyabinsk from a provincial capital city	Resettlement administration (1911)
<i>Violent unrests in 1901–04</i> <sup>g</sup>	Number of openly violent peasant unrests in a province during 1901–1904	Anfimov Andrei (1998)
<i>1897 share of Orthodox</i>	Share of Orthodox in a province in 1897	Troinitskii (1905)
<i>1897 share of old-believers</i>	Share of old-believers in a province in 1897	
<i>Migrants pre-1897</i>	Households whose household head was born in a province but located in Siberian and steppe provinces in 1897	

(continued on next page)

**Table A1** (continued)

Variable name	Variable definition	Sources
<i>Cumulative migrants</i>	Number of migrants pre-1897 plus number of all migrants up to the current year	Troinitskii (1905) Turchaninov N. 1910, 1915
<i>Share of private land in 1905</i>	Share of private land in a province in 1905	Central Statistical Committee of the Ministry of Interior Affairs (1907)
<i>Share of commune land in 1905</i>	Share of commune land in a province in 1905	
<i>Peasant share of private land in 1905</i>	Share of private land belonging to peasants among all private land in a province in 1905	
<i>Private land Gini in 1905</i>	Provincial Gini index estimated using the distribution of private land in 1905	Central Statistical Committee of the Ministry of Interior Affairs (1906)
<i>Land captains in 1906</i>	Number of land captains in a province	
<i>Higher education graduates in 1897</i>	Number of higher education graduates in a province per 1000 population in 1897	'Electronic depository of Russian historical statistics' dataset
<i>Russian–Japanese War recruits</i>	Number of recruits conscripted into the army under the usual annual procedure	Ministry of War (1906, 1907)

For the 1901–1906 cross-section, we do not have data on population, urban share, rural density, and livestock for 1901, 1902, and 1903, or yield and rural wages for 1901. We assign them missing values, estimating annual averages for this cross-section.

<sup>a</sup> Estimated from the original sources as the number of all migrants minus subsidized migrants. Negative values because of deducting and counting errors were replaced by zeros. All results hold if we do not correct for negative values.

<sup>b</sup> Negative values because of deducting and counting errors were replaced by zeros. All results hold if we do not correct for negative values.

<sup>c</sup> Because of data availability we use data on exits from the commune since November 9, 1906 (the date when the government issued the reform decree), till January 31, 1908, for the 1907 period, and since February 1, 1908, till December 31, 1909, for the 1908–1909 period.

<sup>d</sup> There is no data on number of applications to exit in 1907. We approximate them linearly from annual 1908–1909 figures.

<sup>e</sup> For the 1896–1900 cross-section, we use 1897 figures from the "Electronic depository of Russian historical statistics" dataset (<http://www.nes.ru/en/science/russiastats>).

<sup>f</sup> We use the early twentieth century regional classification of provinces.

<sup>g</sup> Not available for the later years.

**Table A2**

Prereform summary statistics by control and treatment groups.

Variable	Obs	Mean	Std. dev.	Min	Max
<i>Control group</i>					
All migrants (hh per 1000 rural pop.)	18	0.1	0.08	0.01	0.25
Subsidized migrants (hh per 1000 rural pop.)	18	0.06	0.06	0	0.22
Unsubsidized migrants (hh per 1000 rural pop.)	18	0.04	0.05	0	0.17
Population (1000)	18	1796.71	982.64	412.72	3525.63
Rural population density	18	44.70	21.75	21.47	102.45
Livestock per 1000 rural pop.	18	559.41	142.76	336.94	893.33
Yield (tons/ha)	18	<b>0.87</b>	0.24	0.58	1.34
Urban share of population	18	0.15	0.08	0.07	0.29
Rural wage (rubles per harvest month)	9	36.22	5.43	31.44	46.14
Urban wage (rubles per month)	18	17.64	5.67	9.33	24.12
Railway tariff (kopeks)	18	<b>435</b>	16.09	405	465
<i>Treatment group</i>					
All migrants (hh per 1000 rural pop.)	82	0.19	0.23	0	1.09
Subsidized migrants (hh per 1000 rural pop.)	82	0.1	0.16	0	0.89
Unsubsidized migrants (hh per 1000 rural pop.)	82	0.1	0.1	0	0.61
Population (1000)	82	2112.08	732.29	873.30	4175.93
Rural population density	82	38.46	19.09	4.12	96.13
Livestock per 1000 rural pop.	82	535.55	241.21	148.67	1320
Yield (tons/ha)	82	<b>0.69</b>	0.2	0.2	1.14
Urban share of population	82	0.12	0.11	0.02	0.7
Rural wage (rubles per harvest month)	41	40.03	10.57	25.50	65.22
Urban wage (rubles per month)	82	16.33	4.15	9.33	24.12
Railway tariff (kopeks)	82	<b>345</b>	74.39	130	475

Notes: bold type indicates that the difference in means between the treatment and control groups is statistically significant at the 5% level (two-sided), allowing for unequal variances.

**Table A3**

Postreform summary statistics by control and treatment groups.

Variable	Obs	Mean	Std. dev.	Min	Max
<i>Control group</i>					
All migrants (hh per 1000 rural pop.)	54	0.2	0.24	0	1.2
Subsidized migrants (hh per 1000 rural pop.)	54	0.17	0.21	0	1.13
Unsubsidized migrants (hh per 1000 rural pop.)	54	0.04	0.04	0	0.21
Population (1000)	54	2231.29	1139.11	457.45	4215.4
Rural population density	54	50.49	25.08	24.58	114.94
Livestock per 1000 rural pop.	54	<b>599.57</b>	146.19	340	874.
Yield (tons/ha)	54	<b>1.04</b>	0.22	0.52	1.42
Urban share of population	54	0.16	0.08	0.07	0.39
Rural wage (rubles per harvest month)	54	27.13	6.96	15.75	45
Urban wage (rubles per month)	51	<b>22.56</b>	5.49	13.84	31.29
Railway tariff (kopeks)	54	<b>435</b>	15.78	405	465
<i>Treatment group</i>					
All migrants (hh per 1000 rural pop.)	245	0.47	0.57	0	3.07
Subsidized migrants (hh per 1000 rural pop.)	245	0.28	0.39	0	2.71
Unsubsidized migrants (hh per 1000 rural pop.)	245	0.19	0.27	0	2.14
Population (1000)	246	2537.53	836.88	696	4890.25
Rural population density	246	45.45	21.97	4.78	101.05
Livestock per 1000 rural pop.	246	<b>537.86</b>	190.14	305	1240
Yield (tons/ha)	246	<b>0.85</b>	0.22	0.16	1.59
Urban share of population	246	0.13	0.12	0.03	0.74
Rural wage (rubles per harvest month)	240	28.93	7.92	15.75	57.6
Urban wage (rubles per month)	246	<b>20</b>	3.91	13.84	31.29
Railway tariff (kopeks)	246	<b>345</b>	74.08	130	475

Notes: bold type indicates that the difference in means between the treatment and control groups is statistically significant at the 5% level (two-sided), allowing for unequal variances.

**Table A4**

The effect of the reform on migration (in logs).

Dependent variable	Migration (in logs)					
	Fixed effects					
	AllM	UnsM	AllM	UnsM	AllM	UnsM
	(1)	(2)	(3)	(4)	(5)	(6)
Repartition province * Post	0.41*	0.45**				
	[0.209]	[0.204]				
Exits (in logs)					0.05*	0.04*
					[0.026]	[0.021]
Repartition province * 1907 period dummy			0.15	0.53*		
			[0.253]	[0.292]		
Repartition province * 1908–09 period dummy			0.29	0.47**		
			[0.184]	[0.194]		
Repartition province * 1910–11 period dummy			0.08	0.24		
			[0.161]	[0.16]		
Repartition province * 1912 period dummy			0.57***	0.27		
			[0.166]	[0.176]		
Repartition province * 1913 period dummy			0.54***	0.45**		
			[0.169]	[0.207]		
Repartition province * 1914 period dummy			–0.14	0.1		
			[0.281]	[0.274]		
Population (in logs)	0.15	–0.02	0.39	0.06	–0.11	–0.22
	[0.452]	[0.393]	[0.480]	[0.386]	[0.474]	[0.422]
Rural population density	0.02	0.02	0.02	0.02	0.02	0.01
	[0.013]	[0.015]	[0.013]	[0.015]	[0.012]	[0.014]
Livestock	–0.00	0.00	–0.00	–0.00	–0.00**	–0.00***
	[0.002]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Yield	0.50	0.57*	0.35	0.49	0.36	0.41
	[0.310]	[0.302]	[0.291]	[0.293]	[0.303]	[0.285]
Urban share of population	–3.55	–1.82	–2.38	–0.97	–3.06	–0.93
	[4.193]	[3.112]	[4.148]	[3.082]	[4.176]	[3.178]
Urban wage	–0.05***	–0.03**	–0.05***	–0.03**	–0.05***	–0.03**
	[0.014]	[0.012]	[0.014]	[0.012]	[0.015]	[0.013]
Period effects	Yes	Yes	Yes	Yes	Yes	Yes
Region effects	No	No	No	No	No	No
Province effects	Yes	Yes	Yes	Yes	Yes	Yes
Constant	4.91	4.60	3.05	3.97	9.27**	8.47**
	[3.771]	[3.369]	[3.960]	[3.291]	[3.793]	[3.447]

(continued on next page)

Table A4 (continued)

Dependent variable	Migration (in logs)					
	Fixed effects					
	AllIM	UnsM	AllIM	UnsM	AllIM	UnsM
	(1)	(2)	(3)	(4)	(5)	(6)
Observations	340	340	340	340	319	319
R-squared	0.578	0.564	0.602	0.572	0.611	0.592
Number of provinces	50	50	50	50	47	47

Notes: Results are for log number of migrant households in columns 1, 3 and 5 and log number of unsubsidized migrant households in column 2, 4, and 6. The variable *Repatriation province* is the treatment indicator variable = 1 if the observation is a province affected by the reform and =0 otherwise. The variable *Post* is a dummy variable = 1 if the observation is from a period after Nov. 1906 and =0 otherwise. The variable *Exits (in logs)* is the log number of household exits. Province effects are individual fixed effects whereas region effects are group fixed effects. Clustered-robust standard errors are in brackets.

\* p-Value < 0.1.  
 \*\* p-Value < 0.05.  
 \*\*\* p-Value < 0.01.

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