Credit and income mobility in Russia

David Aristei 1 · Cristiano Perugini 1

Received: 3 September 2019 / Accepted: 3 January 2022 / Published online: 16 March 2022
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Abstract
This paper investigates the microeconomic drivers of short-term income mobility in Russia over the period 1994–2018, focusing on the role of access to credit in triggering household income growth. Controlling for a large set of household-level characteristics and accounting for endogeneity, we provide robust evidence on the positive and significant impact of credit on income growth. We also find that the pro-mobility effect of access to credit is heterogeneous both over time and across household characteristics. Our empirical evidence corroborates the idea that the beneficial impact of credit on income mobility mainly occurs through channels related to the labour market, particularly an increase in labour supply at the intensive margin in the short run and at both the extensive and intensive margins in the longer run.

Keywords Income mobility · Credit access · Russia · RLMS

JEL classification D31 · H81 · J60 · O15

1 Introduction

The analysis of income mobility provides an informative and complementary perspective to the cross-sectional study of income distribution. Tracking the fortunes of individuals over time allows light to be shed on how absolute and relative economic positions evolve over the income ladder and drive changes in inequality. This is particularly the case for contexts undergoing large-scale and turbulent transformations that reshape social and economic structures. This study investigates access to credit as one of the possible drivers of household absolute income mobility in Russia, a country that experienced massive institutional changes during its transition towards a market economy and joined the club of high-income inequality economies (Mitra and Yemtsov 2007; Novokmet et al. 2018).
Previous empirical studies have documented higher levels of economic mobility in Russia compared to other transition countries, especially during the first stage of the transition process (Commander et al. 1999; Jovanovic 2001). Most of the literature has focused on relative income mobility (see Lukiyanova and Oshchepkov 2012; EBRD 2016; and Nissanov 2017), while only few studies have investigated the drivers of absolute individual/household income growth. Lokshin and Ravallion (2004) show that, during the period 1994–1998, higher upward income mobility was associated with larger households, urban areas, higher levels of education and access to land. Dang et al. (2020) find rising income levels and decreasing inequality from 1994 to 2015, with the latter mostly caused by pro-poor growth rather than redistribution. They also provide evidence that specific employment features (full-time, high-skill jobs; employment in the private and formal sector) favour upward absolute income growth.

The main novel contribution of our study is to shed light on the role of access to credit on absolute income mobility. The theoretical and empirical literature on the drivers of income mobility is extensive and has focused on individual and household demographic factors, physical and human capital endowments, initial income levels and institutional settings (see Burkhauser and Couch 2009; Jäntti and Jenkins 2015). Little attention has been devoted to the role of credit, despite the fact that the efficient functioning of credit markets is often invoked as crucial for achieving higher growth (e.g., Arestis and Demetriades 1997; Levine 2005) and as a key driver of distributive patterns in the long run (e.g., Greenwood and Jovanovic 1990; De Haan and Sturm 2017). Although the channels through which credit is believed to affect inequality are microeconomic and dynamic, research that explicitly considers how access to credit shapes individual income mobility is limited.

Our choice to focus on absolute mobility is, first of all, motivated by the fact that perceptions of changes in income inequality are often reported to refer to absolute comparisons (see Ravallion 2018), which are therefore important in shaping individual incentives and behaviours. Moreover, absolute income mobility has a clear normative interpretation: while the upward relative mobility of an individual necessarily comes at the expense of someone else, individual upward mobility in absolute terms does not and is therefore associated with improved social welfare. This seems particularly relevant for Russia in the period covered by our analysis, when a significant share of the population experienced remarkable absolute income movements that changed their material well-being conditions and living standards, irrespective of their relative shifts across the income ladder.

To our aims, we use household-level data from the Russian Longitudinal Monitoring Survey (RLMS) from 1994 to 2018, a sample period that enables us to consider all phases of Russia’s transition and all its macroeconomic shocks, including its most recent crisis (2014–2015). The extensive time coverage of the RLMS allows us to assemble a longitudinal sample of households observed repeatedly over three-year time intervals. Our empirical analysis relies on fixed-effects panel regression techniques that allow us to tackle the endogeneity caused by unobserved time-invariant heterogeneity at the household level. In addition, we explicitly address the potential endogeneity of access to credit with respect to household income growth by means of an instrumental variable approach. Our estimation results clearly indicate that access to credit boosts household income growth and that the effect is heterogeneous both across household characteristics and over time. In particular, we find that the significance of this pro-mobility effect is mainly driven by the impact of access to credit on income growth during the 2004–2018 period, when some important institutional evolutions resulted in a better functioning and more efficient banking and financial sector. We also investigate the possible channels through which credit may affect income mobility and that are related to possible
adjustments in labour supply (increases at the intensive and extensive margins), better job/worker matching, and an increase in non-labour income sources. Our findings indicate that the short-term pro-mobility effects of credit unfold by increasing labour supply at the intensive margin only (i.e., increasing hours worked). However, when the analysis is replicated on longer mobility intervals of 5 and 7 years, a positive effect also emerges at the extensive margin. In contrast, our results do not support the idea of credit triggering a better match between jobs and workers’ skills/education or activating capital incomes.

The remainder of the paper is structured as follows. In the next section, we define our research hypotheses on the effects of credit on absolute income mobility. In Section 3, we illustrate the empirical model and the econometric methods used in the analysis. Section 4 provides a description of the data (4.1) and a snapshot of income mobility in Russia over the period considered (4.2). In Section 5, we present and discuss the estimation results, focusing on the impact of credit on absolute income mobility (Section 5.1) and investigating possible heterogeneity over time and across households (5.2). In Section 6, we shed light on the channels through which the pro-mobility effect of access to credit materializes. Section 7 concludes.

2 Intragenerational income mobility and access to credit: research hypotheses

We hypothesize that the short-run impact of credit on income mobility unfolds through three different mechanisms that are related to labour market adjustments (labour supply and job/worker matching) and non-labour income sources.

The first channel relates access to credit to changes in labour supply decisions. Within the life cycle/permanent income literature, some articles emphasize how labour supply can be used as an important smoothing device in the presence of liquidity constraints (Pijoan-Mas 2006; Blundell et al. 2008). Within this framework, Rossi and Trucchi (2016) show that since consumption can be enhanced (or smoothed) through borrowing, access to credit may decrease labour supply. Based on data for Italian young male workers, they find that liquidity constraints lead to longer hours worked. This result is largely driven by the self-employed, probably due to their greater flexibility in adjusting their labour supply. Bui and Ume (2020) report a similar outcome for the US, with credit expansion decreasing the intensity of labour supply but not affecting the extensive margin of participation. The effect of liquidity constraints therefore seems limited to the intensive margin, particular age groups and individual (not household-level) labour supply decisions. However, the presence of labour market frictions, macroeconomic imbalances and structural adjustment processes may impose severe limitations on such labour supply adjustments. The possible irreversibility of even temporary decisions is indeed likely to prevent rational agents from reducing their labour supply in response to having accessed credit. When structural and macroeconomic conditions shape a context of generally low (or declining) earnings, even downward adjustments at the intensive margin might become problematic. This might be the case for Russia in the decades after its transition, when downward flexibility in wages and working hours shaped the main features of the ‘Russian way in labour market adjustment’ (Layard and Richter 1995), a model that survived a number of negative and positive shocks and remained largely in action through the 20 years of transition and beyond (Gimpelson and Kapeliushnikov 2013; Gurvich and Vakulenko 2017; Gimpelson 2019). In addition, on a more general level, extensive evidence
exists that households with loans/mortgages are more committed and thus more inclined to increase their participation in the labour market in view of their debt repayment (Fortin 1995; Bottazzi et al. 2007; Bottazzi 2004; Del Boca and Lusardi 2003; Belkar et al. 2007; Butrica and Karamcheva 2014). Based on this discussion, our first research hypothesis (H1) is that access to credit in Russia might be associated with an increase in labour supply and, consequently, with higher income mobility, provided that the institutional system and credit markets allow borrowers capable (or willing) to repay their debt to be identified (Besley 1995).

The second channel concerns the quality of job/worker matching. Bui and Ume (2020) explain how improved access to credit allows individuals to align their work effort with their productivity, i.e., they can afford to work less if their productivity and wages are low. This is not possible in the presence of liquidity constraints, as individuals need to supply more labour to fund consumption even if their productivity/wage is low. This line of reasoning can be extended to education/skills and job matching if access to credit can temporarily relax liquidity constraints and enable job-search efforts aimed at reducing mismatches (typically overeducation or overqualification) that are normally associated with low productivity and low wages (Allen and Van der Velden 2001; OECD 2016; ILO 2016). Our second research hypothesis (H2) is that access to credit might trigger better education-occupation matches and higher earnings and, in this way, improve income mobility.

A third possible channel is related to non-labour market income sources, specifically capital income. Capital income can consist of either dividends or interest on/returns from trust funds or other financial assets or returns from property investments. No evidence exists on how capital income shapes income mobility; however, a few studies have investigated how non-labour income sources influence distributive patterns, showing their relevance especially at the top of the distribution (see, e.g., Fräßdorf et al. 2011, and the references cited therein). Capital incomes have also been shown to be significantly more volatile than labour incomes and very important to the increase in income inequality in recent decades (Atkinson 2000; Gottschalk and Smeeding 1997; Jenkins 2000; Jäntti 1997). When credit taken out by households is not used up for current needs or for human capital accumulation, it can be employed to increase the stock of financial and physical assets. If financial and credit markets are developed and efficient enough to allocate credit to agents able to invest their resources profitably, this materializes into capital income flows. Hence, our third research hypothesis (H3) is that access to credit might trigger income growth via (higher) returns to assets.

3 Methods and empirical strategy

3.1 Modelling income mobility and the role of credit

Income mobility can be analysed from a microeconomic perspective with the aim of identifying which individual units display larger income changes and identifying the main underlying determinants of those changes (Fields et al. 2007). We use households as the unit of reference rather than individuals, as the household is the pivotal dimension around which the decisions of individuals (e.g., parenthood, labour supply, and employment decisions) are interdependently made. This perspective also allows us to incorporate all income sources, the effects of household demographic factors and the impact of credit on individual and household decisions and income patterns into the analysis. Last, access to credit is a
household-level variable in the dataset used (see Section 4.1), and it is therefore natural to investigate its effects on the whole household.

To model the microeconomic drivers of income changes over time, we rely on the approach proposed by Fields et al. (2003). They start from the simple framework for the determinants of household income proposed by Duncan (1983) and derive a model of income changes driven by time invariant household characteristics (both observable and unobservable), base year income, time-varying characteristics in the base year and changes in those time-variant characteristics. As shown by Woolard and Klasen (2005), this approach is consistent with a standard household utility maximization model with adult-equivalent household income as the measure of utility dependent on household assets and the economic environment in which the household generates income (see also Fields et al. 2003, and Aristei and Perugini 2015a). Accordingly, the model of changes in household income (in logarithmic terms) can be written as:

\[
\Delta \ln y_{it} = \ln y_{it} - \ln y_{i(t-1)} = f\left(c_{it-1}, \ln y_{i(t-1)}, d_{it-1}, k_{it-1}, e_{it-1}, \Delta d_{it}, \Delta k_{it}, \Delta e_{it}\right)
\]

which can be expressed as the following estimable equation:

\[
\Delta \ln y_{it} = \beta_1 c_{it-1} + \beta_2 \ln y_{i(t-1)} + x_{i(t-1)}^\prime \beta_3 + \Delta x_{i(t-1)}^\prime \beta_4 + \sum_{t=1}^{T_i} \theta_{it} D_t + \eta_{it} + \epsilon_{it}
\]

for \( i = 1, \ldots, N, t = 1, \ldots, T_i \), where \( y_{it} \) and \( y_{i(t-1)} \) are real household equivalized income in the initial and final years of each time interval, respectively. \( x_{i(t-1)} = (d_{it-1}^\prime, k_{it-1}^\prime, e_{it-1}^\prime)^\prime \) is the vector of initial-year explanatory variables that describe obstacles or stepping stones to income mobility: \( d_{it-1} \) includes the demographic characteristics of household \( i \) (and/or of its head), \( k_{it-1} \) includes the physical and human assets of the household (and/or of its head), and \( e_{it-1} \) proxies the employment status/occupation of the head of household and/or of the other members (as a share of total household size). The \( \Delta \) operator refers to the change in the corresponding time-varying variables that account for changes in key demographic and economic features between the initial and final year. In light of the main purpose of our analysis, the set of regressors in (2) includes household access to credit in the initial year (\( c_{it-1} \)), and its estimated coefficient sheds light on the impact of credit on income mobility. As explained in Section 4.1, \( c_{it-1} \) can be a dichotomous or a continuous variable. The model can be readily extended to include interaction terms between credit and other explanatory variables in order to allow for heterogeneity in the effect of credit both over time and across households.

The correct identification of the effect of access to credit on mobility is endangered by its potential endogeneity with respect to the household’s economic conditions and income growth prospects. A first way to address this issue consists of exploiting the longitudinal dimension of...
the data, as we are able to observe income changes between the initial and final years for the same household for two periods or more. The use of fixed effects (FE) estimation techniques allows us to eliminate unobserved individual (household) time-invariant characteristics ($\eta_i$ in Eq. (2)) that could be driving the endogeneity. The set of time dummy variables $D_t$ further controls for the impact of specific macroeconomic shocks common to all households observed in a given interval; time fixed effects can be replaced by country-level time-variant policy/institutional variables to explore their effect on income mobility at the household level (see Section 5.1).

The potential endogeneity of credit access is also taken into account by means of a fixed-effects instrumental variable (FE-IV) estimation approach. The set of instruments, all of which refer to the initial year ($t-1$) of each period, includes a dummy variable for home ownership, the average access to credit for similar households and the number of bank branches per capita in the region. The validity of the instrumental variables considered, assessed by means of the usual specification tests (see estimation tables), hinges on the assumption that conditional on the other regressors, they are correlated with $\Delta \ln y_{it}$ only through $c_{it-1}$. The availability of assets to be pledged as loan collateral represents an assurance for lenders that facilitates access to credit (Calcagnini et al. 2015; Kumarasamy and Singh 2018; see Barisitz 2013, and IMF 2006, for the specific case of Russia). Although no evidence based on Russian data is available, home ownership in particular has been found to increase the probability of obtaining secured loans and a wider variety of unsecured credit arrangements (Bridges et al. 2006), to reduce the household’s probability of facing financing constraints and of resorting to informal loans (Zanin 2017), and to facilitate access to home equity-based borrowing (Defusco 2018). A second instrument for $c_{it-1}$ is the average of the credit access dummy for those households with similar characteristics in terms of region of residence and household head employment characteristics. The rationale behind the use of group average variables that are computed by excluding the $i$-th household from the computation is that the behaviour of each household is shaped by, among other things, the behaviour of similar households defined according to a set of observable characteristics (see Fields et al. 2003). The third instrumental variable (the number of bank branches per capita in each region, drawn from the regional statistics of the Central Bank of the Russian Federation) accounts for the effects of the development of local credit markets on the availability of credit.

3.2 Disentangling the channels of transmission from credit to mobility

To provide evidence on our research hypotheses, we need to go beyond the model of household income changes in Eq. (2) and attempt to disentangle the channels through which access to credit affects income mobility.

As a first step, we estimate the direct impact of credit access in the initial year on the change over time in a set of variables assumed to transmit the effects of credit to household income growth. To this end, maintaining the same empirical specification as in (2), we test the impact of access to credit on changes in the extensive and intensive margins of labour supply (H1), in job/worker mismatch (H2) and in capital income (H3). Formally, we consider the following fixed effects model:

$$
\Delta Ch_{it}^j = \alpha_1 c_{it-1} + \alpha_2 Ch_{it-1}^j + \alpha_3 \ln y_{it-1} + x_{it-1}^j \alpha_4 + \Delta x_{it}^j \alpha_5 + \sum_{t=1}^{T_i} \theta_2 t D_t + \eta_{2i} + \varepsilon_{2it} \tag{3}
$$
where $\Delta Ch^j_{it}$ is the change in one of the variables assumed to transmit the effects of credit to household income growth, and the potential endogeneity of $c_{it-1}$ is addressed using the additional identifying instruments discussed in Section 3.1.

Equation (3) allows us to assess the effects of access to credit on variables normally associated with income mobility, but this last linkage has not yet been empirically modelled. To shed light on this whole chain of effects, we consider a trivariate recursive system of simultaneous equations, which allows us to jointly estimate the effect of credit on each channel variable and the impact of that channel variable on income mobility. Formally, we define the recursive system of fixed-effects equations for the three endogenous variables (i.e., income mobility, channel variables and access to credit) as:

$$
\Delta \ln y_{it} = \beta_1 c_{it-1} + \beta_2 \Delta Ch^j_{it} + \beta_3 Ch^j_{it-1} + \beta_4 \ln y_{it-1} + x'_{it-1} \beta_5 + \Delta x'_{it} \beta_6 + \sum_{t=1}^{T_i} \theta_1 D_t + \eta_{1i} + \varepsilon_{it}
$$

$$
\Delta Ch^j_{it} = \alpha_1 c_{it-1} + \alpha_2 Ch^j_{it-1} + \alpha_3 \ln y_{it-1} + x'_{it-1} \alpha_4 + \Delta x'_{it} \alpha_5 + z'_{1it-1} \alpha_6 + \sum_{t=1}^{T_i} \theta_2 D_t + \eta_{2i} + \varepsilon_{2it}
$$

$$
c_{it-1} = \gamma_1 \ln y_{it-1} + x'_{it-1} \gamma_2 + \Delta x'_{it} \gamma_3 + z'_{2it-1} \gamma_4 + \sum_{t=1}^{T_i} \theta_3 D_t + \eta_{3i} + \varepsilon_{3it}
$$

(4)

where $z_{1it-1}$ and $z_{2it-1}$ are the vectors of additional identifying instruments for $\Delta Ch^j_{it}$ and $c_{it-1}$, respectively; in particular, we include the group average values (by year and region) of the corresponding channel variables in $z_{1it-1}$. Model (4) is estimated for each of the channel variables that reflect the mechanisms conjectured under hypotheses H1, H2 and H3. The inclusion of $c_{it-1}$ in the equation for $\Delta \ln y_{it}$ is meant to capture the impact of credit on mobility through channels other than the one explicitly considered in the model (in the equation for $\Delta Ch^j_{it}$).

As a robustness check, we further extend model (4) to assess the effect on income mobility of all the transmission channels considered together while controlling for the endogeneity of credit. To this end, we consider a six-equation recursive system composed of: one income mobility equation that controls for the effects of access to credit and of the four transmission channels on income mobility; four equations that estimate the impact of credit on each transmission channel variable; and one equation that models the determinants of access to credit at time $t-1$.

4 Data and descriptive evidence

4.1 Data and variables

Our empirical analysis relies on the Russian Longitudinal Monitoring Survey (RLMS), a unique panel survey based on a representative sample of over 10,000 individuals that started in 1992. The survey provides detailed information at both the individual and household levels, enabling repeated cross-sectional and longitudinal analyses. As the survey was redesigned in 1994 and consistency with the previous two waves was lost, we use the longitudinal data from 1994 to 2018, the latest survey year available at the time of writing. The database has been
used extensively to analyse inequality, poverty dynamics, and inter and intragenerational income mobility in Russia (see Bogomolova and Tapilina 1999; Lokshin and Ravallion 2004; EBRD 2016; Nissanov 2017; Dang et al. 2020; Borisov and Pissarides 2020). As most longitudinal surveys, the RLMS suffers from issues related to non-random attrition and measurement error. In the RLMS, attrition is due to natural causes, refusal to continue participation and migration to another area, as no effort is made to trace respondents who have left their original residence (see Kozyreva et al. 2016).2 Regarding measurement error, the RLMS tends to underestimate top incomes, which increased spectacularly during the transition and especially following the financial crisis in 2008 (see Novokmet et al. 2018).3 To limit the impact of such issues, we focus our analysis of mobility on short time intervals and trim the sample at the top of the distribution to reduce the influence of extreme observations on the relationships of interest.

To analyse income mobility at the household level, we construct a longitudinal dataset by identifying and tracking households over time based on their heads.4 The main reason behind this choice is that in the longitudinal RLMS dataset it is possible to uniquely identify individuals across rounds (by means of the variable “idind”). Conversely, household identifiers vary across rounds and households can be linked over time only using the identifier from the earlier round. However, the presence of households that move from their original addresses and of those that split up between rounds further complicates the matching procedure, especially when the analysis spans a long time period. Following Dang et al. (2020), we thus define a panel of household heads: this allows us to uniquely identify the individual having the main responsibility for household affairs and, at the same time, to account for changes in the composition of the corresponding household over time, as well as for household splits across rounds.

With the preceding caveats in mind and in view of our empirical strategy, we assembled a longitudinal dataset composed of 12 3-year time intervals (1994–1996, 1996–1998, and so on until 2016–2018), in which each household is observed for at least two intervals. The short length of the interval reflects our choice to focus on the short-run effects of access to credit while minimizing the impact of attrition and preserving a sample large enough to carry out our complementary analyses (i.e., sub-period analyses and regressions with interaction terms). To ensure comparability over time and to properly measure household income mobility, we dropped from the sample those households having a household identifier in the final year of

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2 On average, 8% of respondents were lost each year from 1994 to 2009. In 2010 the sample was increased by approximately 2000 households in order to improve its representativeness across the regions of the country, and the sampled population increased from 13,991 individuals in 2009 to 21,343 in 2010. Following Lukiyanova and Oshchepkov (2012), we compared year-specific sample characteristics between the cross-sectional and panel samples and found that differences in terms of the average and median equivalized incomes and levels of inequality are rather small, suggesting that sample attrition is not a relevant issue in our analysis.

3 The consequences of this bias on income mobility are not easy to predict. On the one hand, a heavy underestimation of top incomes would overestimate survey-based income mobility: with much higher true high-income levels, the range of the top decile would substantially extend and this would imply a higher persistence at the top (hence lower overall mobility). At the same time, if those individuals reaching the top of the distribution due to high-income growth underreport their new level of income only (or proportionally more), mobility would be underestimated.

4 We define the head of a household according to the demographic hierarchy recommended in the RLMS manual: (1) the oldest working-age male in the household; (2) if there are no working-age males, then the oldest working-age female; (3) if there are no working-age females, then the youngest retirement-age male; (4) if there are no retirement-age males, then the youngest retirement-age female; and finally (5) if there are no retirement-age females, then the oldest child.
the time interval different from their identifier in the initial year (about 1.5% of the sample). The sample is restricted to households that reported non-missing and strictly positive incomes in each year. Furthermore, the top and bottom 0.5% of each year’s distribution of household incomes was trimmed to minimize the impact of extreme incomes, which can have remarkable impacts on measures of mobility (Cowell and Schluter 1999); in addition, trimming increases the consistency of our analysis across samples (Ayala and Sastre 2008; Lukiyanova and Oshchepkov 2012). The final estimation sample is an unbalanced panel of 8648 households, observed on average 4.1 times over the period 1994–2018, for a total of 35,762 observations. Two additional longitudinal datasets of 5- and 7-year time intervals are used for robustness checks.5

The income variable used to measure mobility is monthly household equivalized income (in 2010 roubles), based on total self-reported monetary income received by the household in the last 30 days (variable F14 of the RLMS).6 In this respect, it is worth remarking that the large majority of the interviews in our sample (approximately 86%) were conducted in October or November of the reference year (and 97% were conducted between October and December); furthermore, over 90% of the households were surveyed in the same quarter in the initial and final year of each period that is considered in the assessment of income mobility (and more than 50% were surveyed in the same month).7

Table 1 reports some of the main characteristics of our sample in the initial and final year of each interval. The overall concordance of the series of average/median incomes and inequality with similar indicators from the World Bank Povcalnet database suggests that the RLMS data provide information consistent with international data sources.8

To measure household access to credit, which represents the main explanatory variable, we define a dummy variable (Credit) to be equal to 1 if the household took out money on credit in the last 30 days (variable F13.11A of the RLMS) and/or spent any money in the last 30 days for the payment of credit/repayment of loans (variable E13.72A). Table 1 shows that the percentage of households that had access to credit decreased to approximately 18% by 2002, when it reached its minimum level. Afterwards, the proportion increased to 21% in 2004 and continued to grow until it stabilized at approximately 30% in the years 2012–2016. In 2018, access to credit returned to a lower level (24.5%). We also consider the amount of credit repaid (in absolute terms or as a percentage of income) and of credit taken out in the last 30 days to assess the robustness of our baseline results and to test for possible nonlinearities in the effect of credit on income mobility (see Section 5.1). While the amount of credit increased significantly over time in absolute terms, the last column of Table 1 highlights a clear decline in credit relative to income starting from the mid 2000s, possibly as a result of the reforms

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5 The first dataset considers six intervals (1994–1998, 1998–2002, 2002–2006, 2006–2010, 2010–2014, 2014–2018) and consists of 4186 households, observed on average 2.9 times, for a total of 12,220 observations. The second considers four time intervals (1994–2000; 2000–2006; 2006–2012; 2012–2018) and includes 2165 households, observed on average 2.5 times, for a total of 5380 observations. The sharp decrease in the sample size is indicative of potential attrition bias when we extend the period over which mobility is assessed.

6 Household incomes are equivalized using the OECD scale, which assigns a value of 1 to the first household member, of 0.7 to each additional adult and of 0.5 to each child.

7 To account for possible biases due to the remaining cases in which the month of the interview differs across households and/or over time, all the specifications used in our empirical analysis include: (a) month-of-interview fixed effects and (b) a dummy variable equal to 1 if the month of interview differs between the initial and the final year and zero otherwise.

8 As pointed out by Novokmet et al. (2018), inequality estimates based on RLMS and other official survey data tend to under-estimate the concentration of incomes in Russia especially after 2010 and should be considered as lower bounds of the true levels of income inequality.
Another robustness check is carried out by using an alternative credit access dummy (Credit 12 m) indicating whether the household obtained a line of credit in the last 12 months, which is available for 2006 and later only. In interpreting our outcomes, it should be kept in mind that we could not account for the fact that access to credit might be restricted for certain household types, as the RLMS data do not provide any information on households’ loan application behaviour and financing constraints. In the presence of credit rationing, we cannot exclude that those households that had access to credit in our sample were the households with better economic characteristics and prospects. Should this be the case, we would incur endogeneity due to reverse causality. However, the fact that we are addressing the potential endogeneity of access to credit with respect to income mobility (see Section 3.1) should also account for this additional bias.

Table 1 Sample characteristics and descriptive statistics

| Year | Obs. | Equivalised income | Proportion of households with access to credit | Share of credit repaid on income |
|------|------|--------------------|-----------------------------------------------|----------------------------------|
|      |      | Average | Median | St. Dev. | Gini | Credit | Credit 12 m |
| 1994 | 2555 | 7554.8  | 5349.5 | 7676.1 | 0.435 | 0.248 | -- | 0.346 |
| 1996 | 2808 | 7011.1  | 4804.3 | 7148.9 | 0.457 | 0.244 | -- | 0.424 |
| 1998 | 3052 | 4364.0  | 3055.6 | 4281.1 | 0.437 | 0.227 | -- | 0.741 |
| 2000 | 3328 | 5825.1  | 4074.1 | 5827.7 | 0.434 | 0.199 | -- | 0.521 |
| 2002 | 3806 | 7731.3  | 5737.5 | 6939.3 | 0.408 | 0.178 | -- | 0.356 |
| 2004 | 4013 | 8777.4  | 6666.7 | 7559.7 | 0.406 | 0.210 | -- | 0.344 |
| 2006 | 4378 | 11,754.5| 9112.4 | 9188.9 | 0.377 | 0.226 | 0.233 | 0.242 |
| 2008 | 4161 | 15,732.8| 12,048.2| 14,438.8| 0.394 | 0.246 | 0.204 | 0.299 |
| 2010 | 5753 | 16,249.0| 13,000.0| 12,771.2| 0.351 | 0.251 | 0.189 | 0.206 |
| 2012 | 6653 | 18,107.0| 14,386.0| 14,462.5| 0.348 | 0.294 | 0.215 | 0.216 |
| 2014 | 5843 | 18,157.2| 14,617.3| 13,720.7| 0.337 | 0.313 | 0.172 | 0.234 |
| 2016 | 6184 | 17,396.0| 14,201.0| 13,242.7| 0.333 | 0.281 | 0.121 | 0.194 |
| 2018 | 5334 | 18,337.6| 15,117.8| 13,955.5| 0.324 | 0.245 | 0.114 | 0.190 |

Notes: Equivalised income in 2010 Russian roubles. The share of credit repaid during the last 30 days on monthly income is computed on the subsample of households with access to credit. Descriptive statistics are computed using sample weights.

Source: Own calculations on RLMS data (1994–2018)

introduced in the financial sectors (see Section 5.2). Another robustness check is carried out by using an alternative credit access dummy (Credit 12 m) indicating whether the household obtained a line of credit in the last 12 months, which is available for 2006 and later only. In interpreting our outcomes, it should be kept in mind that we could not account for the fact that access to credit might be restricted for certain household types, as the RLMS data do not provide any information on households’ loan application behaviour and financing constraints. In the presence of credit rationing, we cannot exclude that those households that had access to credit in our sample were the households with better economic characteristics and prospects. Should this be the case, we would incur endogeneity due to reverse causality. However, the fact that we are addressing the potential endogeneity of access to credit with respect to income mobility (see Section 3.1) should also account for this additional bias.

![Field and Ok Index (FO)](image1)

![Shorrocks Index (M_s)](image2)

**Fig. 1** Short-run income mobility in Russia (1994–2018)
For the other drivers of income mobility, we take full advantage of the wealth of information available in the RLMS data and account for a large set of relevant household and household-head characteristics. Table A1 of the Online Appendix reports a list of all the control variables included in our empirical specifications, together with their definitions and descriptive statistics. Consistent with previous studies (Fields et al. 2003; Woolard and Klasen 2005; Aristei and Perugini 2015a and b) and with our empirical model, we include among the regressors both the household and household-head characteristics observed in the initial year of each period and the changes in household size and composition between the initial and final year.

4.2 A descriptive picture of income mobility in Russia

The first measure of mobility considered is the Fields and Ok (1999a) (FO) index, which refers to absolute mobility and is defined as the average distance between the logarithms of equivalent household income in the initial and final distributions. An interesting property of this index is that it can be additively decomposed into a ‘growth’ and a ‘transfer’ component of social utility (see Fields and Ok 1999b). Formally, for a growing economy, the index can be defined and decomposed as:

\[
FO(y_{t-1}, y_t) = \frac{1}{n} \sum_{i=1}^{n} |\ln y_{it} - \ln y_{it-1}| = \frac{1}{n} \sum_{i=1}^{n} (\ln y_{it} - \ln y_{it-1}) + \frac{2}{n} \sum_{i \in L} (\ln y_{it-1} - \ln y_{it}) \tag{5}
\]

where \( y_{t-1} = \{y_{1t-1}, y_{2t-1}, \ldots, y_{nt-1}\} \) and \( y_t = \{y_{1t}, y_{2t}, \ldots, y_{nt}\} \) are the initial and final distributions of income, respectively, and \( n \) is the number of households. The first term of the decomposition \( \text{Growth}(y_{t-1}, y_t) \) measures mobility due to income growth, while \( \text{Transfer}(y_{t-1}, y_t) \) represents mobility due to transfers of income among households with total income held constant, which is equal to twice the amount lost by the losers \( L \) (i.e., those households whose income decreases over the period). Interestingly, the addends of the ‘growth’ component correspond to the dependent variable in our income mobility (growth) model in Section 3.1. The left panel of Fig. 1 (and Table B1 in the Online Appendix) depicts the evolution of the FO absolute mobility index and its components over the period 1994–2018 for our three-year time
intervals. The index confirms existing evidence of a high level of mobility in Russia compared to that in other transition countries. However, income mobility declined in the 2000s compared to that in previous years (1994–2002) when a relatively higher share of mobility was associated on average with the ‘growth’ component. Significantly lower levels of mobility emerge in the later years and in particular after 2006/2008, when the role of the ‘growth’ component also declines, becoming negligible in the most recent years. Overall, mobility in Russia decreased significantly over time and more markedly beginning in the mid 2000s, when the ‘transfer’ component became more prevalent.

The right panel of Fig. 1 provides complementary information by means of the relative income mobility measure proposed by Shorrocks (1978a, b). The index conceptually links mobility and the evolution of inequality over time by comparing inequality in incomes averaged longitudinally over a period of T years and single-year income inequality. The index \( M_S(T) \) is the complement to one of the ratio of inequality in the T-averaged incomes (longer-term inequality) to a weighted average of period-specific inequality. \( M_S(T) \), calculated for Russia using the Gini index as a measure of inequality, confirms the decreasing trend described by the \( FO \) index (their correlation is 0.94, significant at the 1% level), indicating that both absolute and relative mobility followed a similar trend in the period covered by our analysis.

To conclude this section, we explore some descriptive evidence on the link between mobility and access to credit. To this end, we first take advantage of the decomposability of the \( FO \) index into subgroups (see Fields and Ok 1999b); the left panel of Fig. 2 (see also Table B1) describes the level of mobility for households with and without access to credit in our short-run subsamples. With only two exceptions (2008–2010 and 2004–2006, for which the difference is negligible), households with credit experienced higher levels of mobility. This is confirmed by the positional mobility measures derived from transition probability matrixes. The right panel of Fig. 2 reports the average decile jump (Bartholomew 1973), i.e., the average number of deciles crossed in each period by households with and without credit. Although mobility is declining in both cases, households with access to credit systematically experience higher relative mobility. If we calculate the share of households experiencing upward mobility (see the last columns of Table B1 in the Online Appendix), with few exceptions (1996–1998 and 2008–2010), households with access to credit exhibit a significantly higher probability of moving to the upper deciles of the income distribution.

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9 Aristei and Perugini (2015b) show that, similar to Russia, income mobility declined for all of the eastern EU economies in the period 2008–2011 compared to that in 2004–2007. Levels of mobility comparable to that in Russia only emerge in Bulgaria, Slovakia and Poland and are limited to the first period. Overall, the dynamics of the \( FO \) index (and especially of the growth component) provide a picture consistent with the macroeconomic and growth history of Russia in the last two decades, i.e., stagnation or weak growth at the end of the 1990s and after 2008, with higher growth rates in between.

10 Formally, the Shorrocks index is defined as:

\[
M_S(T) = 1 - \frac{I(y_T)}{\sum_{t=1}^{T} \frac{p_t I(y_t)}{H}}
\]

where \( I(y_T) \) is the inequality in T-averaged incomes and \( I(y_t) \) indicates inequality in each period \( t \), computed using the same inequality measure \( I(\cdot) \). The value of the Shorrocks index is sensitive to the inequality index used and the length of the time window \( T \). See Jäntti and Jenkins (2015) (and the references therein) for a discussion on how the length of the accounting period \( T \) may affect income mobility comparisons based on \( M_S(T) \).
5 Results: credit and income mobility

5.1 Baseline results

Table 2 summarizes the OLS-FE and IV-FE estimation results of the baseline income mobility model in Eq. (2) controlling for period fixed effects (columns 1 and 2) and for institutional/policy variables at the country level (columns 3 and 4); the complete results are reported in Table B2 of the Online Appendix.11

Before discussing the parameter estimates, it should be noted that the results of the $F$ test for the significance of the fixed effects and of the Hausman specification tests (reported in the bottom part of Table 2) provide support for the FE approach relative to both the pooled OLS and the random effect estimators in all the specifications. Furthermore, specification tests for the IV-FE estimates (reported in the last rows of columns 2 and 4 of Table 2) clearly point out the endogeneity of access to credit and support the validity of the instrumental variables considered. In this latter respect, the $F$ tests for the joint significance of the instruments in the first-stage regressions lead us to reject the null hypothesis of weak instruments, while the Hansen overidentification tests show that the instruments are uncorrelated with the error term from the second-stage regression; i.e., the instruments are exogenous to income mobility.12

The coefficient estimates reported in columns 1 and 2 of the table clearly indicate that access to credit significantly increases households’ income mobility prospects. However, it is worth remarking that when access to credit is treated as an exogenous regressor, its positive impact on mobility is significant only at the 10% level, and its economic significance is limited. Conversely, when the endogeneity of Credit is properly taken into account, we find that access to credit exerts a positive, statistically (at the 1% level) and economically significant effect on income mobility. In particular, the FE-IV estimates indicate that households with access to credit, ceteris paribus, experience growth in their income over 13% higher than that experienced by those without credit.

To control for the role of institutional or policy variables as additional drivers of household income mobility, in columns 3 and 4 of Table 2, we replace the period dummies with a set of country-level indicators (measured in the initial year). Specifically, we consider government expenditure on education (as a % of GDP) together with indicators of the quality of the legal system and property rights enforcement and of the degree of (de)regulation of the labour market and the business environment (all measured on a continuous scale ranging from 0 to 10, with higher levels corresponding to better legal system or more deregulation). A detailed

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11 In all the estimations, we use standard errors clustered at the household level in order to account for arbitrary correlations within each panel unit. See MacKinnon (2019) and MacKinnon and Webb (2020) for a discussion on the relevance of clustering standard errors when working with panel data.

12 As a preliminary analysis, we also investigated the potential impact of measurement error in and endogeneity of initial incomes. In line with the findings of Woolard and Klasen (2005) the exogeneity of initial income cannot be rejected (with a $p$ value of 0.541). The FE-IV estimates accounting for the potential endogeneity of initial income (using home, durable good and other property ownership dummies as additional instruments) confirm the negative and significant coefficient on the instrumented variable and produce a slightly higher coefficient ($-1.12$) than the FE-OLS estimates (complete results are available upon request). Despite the results of the exogeneity test, measurement error related to the income variable, especially in the tails of the distribution, may persist and should be kept in mind. As a robustness check of our baseline model (available upon request), we estimate models that drop the households in the top and bottom 10% of the income distribution in each initial year; we also replace initial income with its decile. In both cases, the significantly negative sign on initial income is confirmed and the coefficients on the remaining variables are virtually unaltered.
First, and most importantly for the aims of our analysis, the inclusion of these time-varying country-level regressors does not alter the main results related to the impact of access to credit on income mobility, which remains positive and statistically significant, or to the other household-level drivers of mobility. Period dummies are therefore able to account and control for aggregate (i.e., institutional, policy, macroeconomic) aspects not specific to single households or household groups. Furthermore, in line with the literature reviewed in Sections 1 and 2 and with our ex ante expectations, we find that income mobility increases as public expenditure on education increases, as the quality of the legal system improves and as labour market and business deregulation decrease. In particular, a 0.1% increase in public expenditures on education as a percentage of GDP (which oscillates by approximately 3.5% over the whole period) increases average income mobility by approximately 3.7%. Accordingly, a one-point improvement in the quality of the legal system triggers a 14% increase in mobility, while a one-point increase in labour market and business deregulation increases mobility by approximately 17% and 7.5%, respectively.  

\[13\]

Equivalently, these estimates indicate that a one standard deviation increase in the indicators of legal system quality and of labour market and business regulation raises income mobility by 7.53, 9.80 and 6.08 percentage points, respectively.

### Table 2  The drivers of short-run income mobility in Russia (OLS and IV FE estimates, 1994-2018)

|                          | a) Including year dummies | b) Including policy variables |
|--------------------------|---------------------------|-----------------------------|
|                          | (1) FE                    | (2) FE-IV                   | (3) FE | (4) FE-IV |
| **Initial year variables** |                           |                             |        |          |
| Credit                   | 0.013*                    | 0.135***                   | 0.015* | 0.148*** |
|                          | (0.008)                   | (0.048)                    | (0.008) | (0.049) |
| **Policy variables**     |                           |                             |        |          |
| Education exp            |                           |                             |        |          |
|                          | 0.368***                  | 0.373***                   |        |          |
|                          | (0.022)                   | (0.022)                    |        |          |
| Legal system             |                           |                             |        |          |
|                          | 0.133***                  | 0.138***                   |        |          |
|                          | (0.015)                   | (0.014)                    |        |          |
| Labour market reg        |                           |                             |        |          |
|                          | 0.170***                  | 0.169***                   |        |          |
|                          | (0.016)                   | (0.014)                    |        |          |
| Business reg             |                           |                             |        |          |
|                          | 0.075***                  | 0.076***                   |        |          |
|                          | (0.012)                   | (0.012)                    |        |          |
| Other control variables  | Yes                       | Yes                        | Yes    | Yes      |
| Year fixed effects       | Yes                       | Yes                        | No     | No       |
| F test FE vs Pooled [p-value] | [0.000]                | [0.000]                    | [0.000] | [0.000] |
| Hausman test FE vs RE [p-value] | [0.000]               | [0.000]                    | [0.000] | [0.000] |
| Weak instrument F test statistics | 279.32†              | 279.58†                    |        |          |
| Hansen overidentification test [p-value] | [0.241]                  | [0.652]                    |        |          |
| Endogeneity test [p-value] | [0.009]                   | [0.006]                    |        |          |
| N                        | 35762                     | 35762                      | 35762  | 35762    |
| $R^2$                    | 0.5679                    | 0.5642                     | 0.4050  | 0.5362   |

Notes: In columns (2) and (4), the instruments used to address the endogeneity of “Credit” are home ownership, cluster-average levels of credit access by household-head occupation and region, and the regional number of bank branches per capita. Standard errors, clustered at the household level, are reported in parentheses. Complete estimation results are presented in Table B2 of the Online Appendix.

***, ** and * denote significance at the 1, 5 and 10% levels, respectively

\textsuperscript{a} Indicates that the weak instrument F-statistic exceeds the 5% Stock–Yogo critical value for a maximum 10% total relative bias

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With respect to the effects of the other control variables, the results presented in Table B2 of the Online Appendix show that income mobility in Russia is mainly affected by household socio-demographic characteristics. The role of household-head characteristics seems confined to her/his marital status and age: consistent with previous empirical literature (Nissanov 2017; Dang et al. 2020), households with older heads and those whose head is married (or cohabitates with a partner) are characterized by higher income mobility. Focusing on household-level characteristics, we find that household size in the initial year is negatively related to income mobility; in line with Nissanov (2017), we also find that an increase in household size is associated with lower mobility. Given that in our empirical specification we also control for the employment status of the household members (i.e., for the correlation between size and the number of income earners), this result is likely due to the tighter constraints imposed by a higher number of dependent household members. As in Ayala and Sastre (2008) and in Aristei and Perugini (2015a, b), we find that the presence of children in the household (HType 2) is associated to lower mobility compared to the base category (i.e., households composed of adult members only, HType 1); similarly, households composed of elderly individuals only (HType 4) have significantly lower mobility than other types. Higher mobility is associated with the share of household members in better occupations (i.e., managers and professionals) or working in public and foreign firms. The share of unemployed individuals in the initial year is positively associated with income growth, probably as a result of the transition into employment in the following years. The controls for changes in household characteristics over time, when significant, have the expected effects. Last, it is worth noting that initial income levels are negatively related to subsequent growth. This convergence pattern is consistent with the evidence provided in previous studies on Russia (Bogomolova and Tapilina 1999; Lukiyanova and Oshchepkov 2012) and on other western and transition EU countries (Fields et al. 2003; Perugini and Aristei 2015a).

To assess the robustness of the baseline results, we re-estimate the income mobility model on longer-run samples, considering 5- and 7-year time frames.\footnote{In the remainder of the paper and in the Online Appendix, we only report the estimation results of the income mobility models obtained using the FE-IV approach.} The FE-IV estimation results, summarized in Table B3 of the Online Appendix (columns 1 and 5), largely confirm the evidence obtained in the shorter-run analysis, with interesting differences in the magnitude of some of the effects. In particular, compared to the shorter run, the pro-mobility effect of credit increases to 18% and 27% in the 5-year and 7-year mobility samples, respectively. This suggests that either the mechanisms behind the short-run effect continue to play a role or that additional channels come into play. This might be the case for mechanisms that imply more relevant decisions and adjustments (such as a job change or new job search) that materialize over longer time periods. The results presented in Section 6 contribute to shedding some light on these questions.

Table 3 reports the estimation results for the baseline model in which the binary indicator for access to credit has been replaced by alternative proxies for the amount of credit obtained by the household. The outcomes confirm the pro-mobility effect of credit, while the signs and significance of the remaining drivers remain unchanged. Since the amount of credit repaid (columns 1 and 4) and obtained (columns 2 and 5) in the past 30 days are expressed in logs, the corresponding coefficients can be interpreted as elasticities: a 1% increase in their level thus increases income growth prospects by approximately 0.02% and 0.06%, respectively. Similarly, from the estimated coefficients of the share of credit repaid in the last 30 days on
monthly income and its square (columns 3 and 6) we obtain average marginal effects equal to 0.787 and 0.865 in the two specifications, respectively. This means that a 1% increase in the share of credit repaid raises, on average, household income by approximately 0.8 percentage points. However, since the coefficient of the squared term is negative and statistically significant, the magnitude of the effect declines as the amount of debt increases. This could reflect the fact that over-indebtedness is associated with greater economic vulnerability and financial fragility (Anderloni et al. 2012). Although the magnitude of these effects seems to be quite limited, when we restrict the estimation sample to those households with access to credit, we find that a 1% increase in the amount of credit repaid triggers income growth of 0.3% (see column 1 of Table B4 of the Online Appendix). The results for the amount of credit obtained during the last 30 days (column 2 of Table B4) exhibit a remarkable increase in the magnitude

Table 3 Income mobility and credit amounts (IV FE estimates, 1994-2018)

|                  | (1) FE-IV | (2) FE-IV | (3) FE-IV | (4) FE-IV | (5) FE-IV | (6) FE-IV |
|------------------|----------|----------|----------|----------|----------|----------|
| **Initial year variables** |          |          |          |          |          |          |
| Credit repaid (in logs)      | 0.020*** | 0.022*** |          |          |          |          |
|                               | (0.007)  | (0.007)  |          |          |          |          |
| Credit obtained (in logs)    | 0.061*** | 0.069*** |          |          |          |          |
|                               | (0.022)  | (0.023)  |          |          |          |          |
| Share of credit repaid       | 0.789*** | 0.867*** |          |          |          |          |
|                               | (0.296)  | (0.802)  |          |          |          |          |
| Share of credit repaid^2     | -0.015** | -0.016** |          |          |          |          |
|                               | (0.006)  | (0.006)  |          |          |          |          |
| **Policy variables**         |          |          |          |          |          |          |
| Education exp                | 0.326*** | 0.367*** | 0.308*** |          |          |          |
|                               | (0.023)  | (0.027)  | (0.025)  |          |          |          |
| Legal system                 | 0.136*** | 0.154*** | 0.139*** |          |          |          |
|                               | (0.014)  | (0.016)  | (0.014)  |          |          |          |
| Labour market reg            | 0.178*** | 0.164*** | 0.171*** |          |          |          |
|                               | (0.014)  | (0.015)  | (0.015)  |          |          |          |
| Business reg                 | 0.065*** | 0.066*** | 0.060*** |          |          |          |
|                               | (0.013)  | (0.007)  | (0.013)  |          |          |          |
| **Other control variables**  | Yes       | Yes       | Yes       | No        | No        | No        |
| **Year fixed effects**       | Yes       | Yes       | Yes       | No        | No        | No        |
| F test FE vs Pooled [p-value]| [0.000]   | [0.000]   | [0.000]   | [0.000]   | [0.000]   | [0.000]   |
| Hausman test FE vs RE [p-value]| [0.000]   | [0.000]   | [0.000]   | [0.000]   | [0.000]   | [0.000]   |
| Weak instrument F test statistics | 278.85†  | 32.97†   | 18.25†   | 185.53†  | 32.60†   | 18.83†   |
| Hansen overidentification test [p-value]| [0.217]   | [0.200]   | [0.222]   | [0.518]   | [0.880]   | [0.670]   |
| Endogeneity test [p-value]   | [0.025]   | [0.002]   | [0.011]   | [0.042]   | [0.001]   | [0.007]   |
| N                             | 35762     | 35762     | 35762     | 35762     | 35762     | 35762     |
| R^2                           | 0.566     | 0.540     | 0.532     | 0.539     | 0.502     | 0.498     |

Notes: The complete set of control variables includes all the regressors used in the baseline models presented in Table B2 of the Online Appendix. The instruments used to address the endogeneity of the different credit amount variables are home ownership, cluster-average levels of credit access by household-head occupation and region, and the regional number of bank branches per capita. Standard errors, clustered at the household level, are reported in parentheses. Complete estimation results are available upon request

***, ** and * denote significance at the 1, 5 and 10% levels, respectively

a Indicates that the weak instrument F-statistic exceeds the 5% Stock–Yogo critical value for a maximum 10% total relative bias
of its effect on mobility, but the coefficient is not statistically significant, possibly because of the very limited number of households reporting a positive amount of credit obtained. The share of credit repaid still exerts a positive and nonlinear effect on income mobility in the restricted sample, but the magnitude of the effect is significantly larger. From the estimated coefficients (column 3 of Table B4), we obtain an average marginal effect equal to 2.012, which indicate that a 1% change in the share of credit repaid increases mobility, on average, by approximately 2%. Finally, it is worth remarking that the positive and significant effects of credit amount on income mobility are also confirmed in the longer-run samples, and their magnitude is again larger than those obtained in the short-run sample (see Table B3 of the Online Appendix, columns 2–4 and 6–8).

5.2 Heterogeneous effects of credit on mobility over time and across households

In this section, we explore possible heterogeneity in the impact of access to credit on income mobility both over time and by household-head and household characteristics.

Mobility patterns, as discussed in Section 4.2, changed around the mid 2000s and even more significantly in the 2010s. In addition, previous evidence on cross-sectional subsamples (Perugini 2020) suggests that access to credit started to play a significant role in mobility only in the second half of the 2000s, supposedly as a result of institutional developments and policy reforms. In particular, the Russian banking system evolved after the first stage of transition towards a model characterized by an intermediate level of competition and a relatively strong role for the state (see Fungacova et al. 2010; Anzoategui et al. 2012). Specifically, beginning in the early 2000s, Russian banking authorities started to introduce a number of measures aimed, on the one hand, at increasing trust in the banking sector and, on the other hand, at strengthening its solidity. This was meant to address a number of issues that emerged during the 1990s with the first stage of financial liberalization and the privatization of specialized state banks (spetsbanki), which led to a proliferation of financial institutions with obscure ownership structures and operating standards (Schoors 2003). A first important reform concerned the introduction of deposit insurance legislation at the end of 2003, which helped attract a large volume of savings into the system by increasing public confidence in the banking sector. This contributed to private banks being able to compete with state-owned banks in the retail sector in concomitance with a rapidly growing demand for credit by households (Berglof and Lehmann 2009; Yadav 2017) that was triggered by the introduction of a tax exemption for short-term loans in 2004/2005 (Guseva 2008). Other important measures introduced in 2004 were related to the system of prudential supervision by the Central Bank of Russia and the shift from a regime mainly concerned with formal compliance to more substantial control over banks’ health and prospects (Tompson 2004). At the same time, starting in 2004, Russian banks were required to produce financial statements in line with the International Financial Reporting Standards (IFRS), marking a further step towards increasing the quality and availability of information in the sector. This final wave of reforms eventually led to extensive licence withdrawals from private banks, non-bank financial institutions, micro-lenders and other financial market operators. As a result, the number of banks active in Russia decreased significantly during the 2000s, and the decline accelerated remarkably from 2012 onwards (Vernikov 2017). Although various aspects need to be addressed (including the high concentration of banks in core regions), such evolutions eventually led to an overall improvement in the quality of management and conservatism in the commercial policies of banks and contributed to higher operational efficiency (see Solanko 2017; Simanovskiy et al. 2018).
Based on this, we run a set of regressions to explore how the relationship between credit and mobility changed over the period considered. In the first set of regressions, we simply introduce an interaction term between credit and a dummy variable identifying the period from 2004 onwards, which marks the year in which the most significant institutional changes in the banking sector were implemented. As reported in column 1 of Table 4, the interaction term is significant and positive, while the main effect of credit is not significant. Based on these findings, we re-estimate the income mobility model for the two sub-periods, before and after 2004. The results, presented in columns 2–3 of Table 4, confirm that the pro-mobility effect of access to credit is significant in the 2004–2018 period only. With respect to the evidence obtained in the estimation of the whole sample, the magnitude of the coefficient increases from 0.061 to 0.19 in the 2004–2018 period, as the effect is not diluted by the non-significant impact of credit characterizing the first part of the sample.

With the same logic, we focus on the period 2004–2018 and test for the possibility that in the last available years, when the last wave of banking system reforms was introduced, the magnitude of the relationship could have increased further. Columns 4–6 of Table 4 confirm that the impact of credit is stronger after 2012, climbing to 0.25. As a further robustness check for the relationship between credit and mobility in the most recent years of the sample, we consider the dummy variable Credit 12 m (indicating access to credit in the last 12 months),
|                  | (1) CRE-IV | (2) FE-IV | (3) FE-IV | (4) FE-IV | (5) FE-IV | (6) FE-IV | (7) FE-IV |
|------------------|------------|-----------|-----------|-----------|-----------|-----------|-----------|
| Credit           | 0.238***   | 0.143*    | -0.689*** | 0.330***  | 0.458***  | 0.205***  | -0.463**  |
|                  | (0.059)    | (0.077)   | (0.210)   | (0.110)   | (0.118)   | (0.058)   | (0.233)   |
| Credit x Male    | -0.102***  |           |           |           |           |           |           |
|                  | (0.036)    |           |           |           |           |           |           |
| Male             | 0.028***   |           |           |           |           |           |           |
|                  | (0.009)    |           |           |           |           |           |           |
| Credit x Sec Edu | -0.040     |           |           |           |           |           |           |
|                  | (0.070)    |           |           |           |           |           |           |
| Credit x Ter Edu |           | 0.053     |           |           |           |           |           |
|                  |           | (0.101)   |           |           |           |           |           |
| Credit x Age     |           |           | 0.018***  |           |           |           |           |
|                  |           |           | (0.004)   |           |           |           |           |
| Credit x Married |           |           |           | -0.214**  |           |           |           |
|                  |           |           |           | (0.107)   |           |           |           |
| Credit x Size    |           |           |           |           | -0.073*** |           |           |
|                  |           |           |           |           | (0.022)   |           |           |
| Credit x HType 2 |           |           |           |           |           | 0.135**   |           |
|                  |           |           |           |           |           | (0.068)   |           |
| Credit x HType 3 |           |           |           |           |           | -0.075**  |           |
|                  |           |           |           |           |           | (0.086)   |           |
| Credit x HType 4 |           |           |           |           |           | -0.149**  | -0.121**  |
|                  |           |           |           |           |           | (0.061)   | (0.069)   |

Other control variables: Yes
Year fixed effects: Yes
$F$ test FE vs Pooled [p-value]: [0.000]
Hausman test FE vs RE [p-value]: [0.000]
Weak instrument $F$ test statistics: 51.51†
Hansen overidentification test [p-value]: [0.301]
Endogeneity test [p-value]: [0.001]
N: 35762
$R^2$: 0.559

Notes: The complete set of control variables includes all the regressors used in the baseline models presented in Table B2 of the Online Appendix. The instruments used to address the endogeneity of "Credit" are home ownership, cluster-average levels of credit access by household-head occupation and region, and the regional number of bank branches per capita. Standard errors, clustered at the household level, are reported in parentheses. Complete estimation results are available upon request.

***, ** and * denote significance at the 1, 5 and 10% levels, respectively.

Indicates that the weak instrument $F$-statistic exceeds the 5% Stock–Yogo critical value for a maximum 10% total relative bias.
which is available in the RLMS starting only in 2006. The results are summarized in Table B5 of the Online Appendix and confirm that the magnitude of the pro-mobility effect of access to credit is larger after 2012.

Overall, our results highlight a concomitance between stronger effects of credit and the timing of reforms. This corroborates the idea that the channels of transmission from credit to income mobility are activated by better functioning and more efficient banking and financial sectors, which contribute to allocating credit to borrowers who are more likely to use financial resources to increase their income prospects (by adjusting their labour supply decisions and/or activating capital income sources) rather than to feeding current consumption only.

A second dimension of heterogeneity in the impact of credit on mobility pertains to household and household-head characteristics. To this end, we have augmented the baseline mobility model by interacting the credit dummy with a subset of explanatory variables. The estimation results are reported in Table 5.

We first focus on the heterogeneity related to the household head’s gender. To assess the main effect of gender on mobility, we estimate the model by means of a correlated random effect (CRE) IV estimator, which allows for the inclusion of time-invariant covariates. This estimator also allows individual heterogeneity to be correlated with time-varying explanatory variables and accounts for the potential endogeneity of regressors with respect to time-varying idiosyncratic errors (see Joshi and Wooldridge 2019). The empirical results (column 1 of Table 5) show that income mobility is 2.8% higher for households with a male head. By contrast, access to credit exerts a significantly more beneficial effect on income growth for female-headed households and contributes to the alleviation of this dimension of the gender gap. In our interpretative framework, this could be explained by the fact that access to credit provides resources to women (particularly those with full household responsibilities), enabling them to participate more in employment (for example, by being able to resort to market-based family care services), therefore increasing their earned income.

We then account for potential heterogeneity in the impact of credit on mobility related to other time-varying household and household-head characteristics. To this aim, we first include interaction terms one at a time and then consider all the interaction effects simultaneously to jointly account for all sources of heterogeneity and assess their overall significance. Results show that, while the household head’s education level does not play any significant role (column 2 of Table 5), her/his age has significant direct and indirect effects on income mobility (column 3). In particular, the interaction between credit and age has a significant positive coefficient. This might reflect the fact that older household heads are more likely to drive the allocation of credit taken into channels that increase income prospects rather than those that expand current consumption (Schooley and Worden 2010). Being married (or cohabitating with a partner) decreases the pro-mobility effect of access to credit (column 4). This result might depend on the fact that single household heads are more likely to need to increase their labour supply to repay the credit they have taken out than are those who can rely on the financial support of their spouse/stable partner. Larger households and households with children (HType 2) are characterized by relatively lower pro-mobility effects from credit (columns 5 and 6, respectively). This might be related to the heavier family workload due to the presence of children and inactive members in the household, which imposes tighter constraints on the labour supplied in the market by those in employment. Another possible explanation is that in the presence of children, households are more willing to borrow for current consumption to avoid deprivation or consumption restrictions for the children. The effect of credit on mobility is also lower for households composed of elderly individuals only.
(HType 4). This evidence also corroborates our conjectured channels of transmission, as this household type relies mainly on pension income, and we do not expect major labour market reactions to having obtained new credit. Lastly, it is worth noticing that when we include all the interaction terms simultaneously (column 7), results obtained by adding interactions one at a time are largely confirmed. Only the interaction effect related to HType 2 becomes statistically insignificant, suggesting that the heterogeneity due to household types is partly captured by the effect of household size.15

6 Disentangling the transmission channels from credit to mobility

The results presented so far indicate that credit is significantly associated with higher income growth in the short term and that its beneficial effects are confirmed over a longer run. We now attempt to disentangle the mechanisms behind this effect based on the three research hypotheses developed in Section 2. In particular, we test the idea that the effect of access to credit on income mobility unfolds through labour market mechanisms (labour supply and employment mismatch) and capital income. As explained in Section 3.2, we first test the effects of credit on the relevant channel variables, and then we link these effects to income mobility.

Labour supply at the household level can evolve due to changes at either the extensive or intensive margin. For the extensive margin, we consider the number of household members currently working, while the intensive margin of labour supply is measured in terms of total hours worked per week by the household members (see Section A.3. of the Online Appendix for further details). For the aims of our analysis, we regress changes in labour supply on the dummy variable $\text{Credit}$, controlling for the level of labour supply in the initial year and for the same household-head and household characteristics used to assess income mobility (see Eq. 3). The potential endogeneity of access to credit is again addressed by means of an FE-IV approach, as in the income mobility regressions. Columns 1–2 and 5–6 of Table 6 summarize the estimation results for the whole sample (1994–2018) and for the period 2004–2018, respectively.

In both samples, access to credit is positively associated with an increase in the number of total hours worked by the household in the following 3 years. The magnitude of the impact is not negligible, as ceteris paribus, households with access to credit work on average 9.2 hours per week longer than households with no credit (12.4 for the 2004–2018 sample).16 Access to credit does not trigger any significant change in household labour supply at the extensive margin in the short term. However, if we extend the length of the time intervals considered, access to credit is also found to drive a significant increase at the extensive margin (see Table 7), and the magnitude of the effect of credit on the number of household members currently working strongly increases when we consider 7-year intervals. Our evidence is consistent with the literature on the positive link between household indebtedness and labour supply (see Bottazzi et al. 2007; Del Boca and Lusardi 2003; Belkar et al. 2007; Butrica and Karamcheva 2014). However, in the short run, credit triggers an increase at the intensive

15 This interpretation is supported by the fact that, when we add to the model in column 6 the interaction for household size, the effect of HType 2 is again not statistically significant. Results of this additional regression are available upon request.

16 We obtain similar results by using the number of hours worked by the household head as the dependent variable, with an estimated coefficient equal to approximately 7 for both samples. The results are available upon request.
Table 6  Impact of credit on labour supply, overeducation and capital incomes (1994-2018 and 2004-2018)

|                | a) Whole sample (1994-2018) |           |           |           |           |           |           |
|----------------|-------------------------------|-----------|-----------|-----------|-----------|-----------|-----------|
|                | (1)                          | (2)       | (3)       | (4)       | (5)       | (6)       | (7)       |
| Dependent variable: | FE-IV | FE-IV | FE-IV | FE-IV | FE-IV | FE-IV | FE-IV |
| ΔN. workers \(_t\) | 0.025 \(0.065\) | 9.228** \(3.686\) | 0.016 \(0.034\) | -0.038 \(0.092\) | 0.035 \(0.081\) | 12.426*** \(4.574\) | 0.012 \(0.047\) | 0.034 \(0.131\) |
| ΔH. worked \(_t\) | -0.710*** \(0.011\) | -0.803*** \(0.011\) | -0.919*** \(0.016\) | -1.074*** \(0.021\) | -0.761*** \(0.013\) | -0.897*** \(0.013\) | -0.995*** \(0.014\) | -1.117*** \(0.023\) |
| ΔN. overeduc \(_t\) | -0.016 \(0.034\) | -0.021 \(0.034\) | -0.038 \(0.092\) | -0.038 \(0.092\) | -0.016 \(0.034\) | -0.021 \(0.034\) | -0.038 \(0.092\) | -0.038 \(0.092\) |
| ΔR/C income \(_t\) | 0.035 \(0.081\) | 12.426*** \(4.574\) | 0.012 \(0.047\) | 0.034 \(0.131\) | 0.035 \(0.081\) | 12.426*** \(4.574\) | 0.012 \(0.047\) | 0.034 \(0.131\) |

|                | b) 2004-2018 period |           |           |           |           |           |           |
|----------------|----------------------|-----------|-----------|-----------|-----------|-----------|-----------|
|                | (5)       | (6)       | (7)       | (8)       | FE-IV | FE-IV | FE-IV |
| Dependent variable: | FE-IV | FE-IV | FE-IV | FE-IV |
| ΔN. workers \(_t\) | 0.025 \(0.065\) | 9.228** \(3.686\) | 0.016 \(0.034\) | -0.038 \(0.092\) | 0.035 \(0.081\) | 12.426*** \(4.574\) | 0.012 \(0.047\) | 0.034 \(0.131\) |
| ΔH. worked \(_t\) | -0.710*** \(0.011\) | -0.803*** \(0.011\) | -0.919*** \(0.016\) | -1.074*** \(0.021\) | -0.761*** \(0.013\) | -0.897*** \(0.013\) | -0.995*** \(0.014\) | -1.117*** \(0.023\) |
| ΔN. overeduc \(_t\) | -0.016 \(0.034\) | -0.021 \(0.034\) | -0.038 \(0.092\) | -0.038 \(0.092\) | -0.016 \(0.034\) | -0.021 \(0.034\) | -0.038 \(0.092\) | -0.038 \(0.092\) |
| ΔR/C income \(_t\) | 0.035 \(0.081\) | 12.426*** \(4.574\) | 0.012 \(0.047\) | 0.034 \(0.131\) | 0.035 \(0.081\) | 12.426*** \(4.574\) | 0.012 \(0.047\) | 0.034 \(0.131\) |

Notes: The complete set of control variables includes all the regressors used in the baseline models presented in Table B2 of the Online Appendix. The instruments used to address the endogeneity of “Credit” are home ownership, cluster-average levels of credit access by household-head occupation and region, and the regional number of bank branches per capita. Standard errors, clustered at the household level, are reported in parentheses. Complete estimation results are available upon request.

***, ** and * denote significance at the 1, 5 and 10% levels, respectively.

a Indicates that the weak instrument F-statistic exceeds the 5% Stock–Yogo critical value for a maximum 10% total relative bias.
margin only, while it is able to push inactive (or unemployed) household members into employment only over longer periods of time. A possible explanation might be related to the peculiarities of the Russian labour market, which is characterized by remarkable flexibility in working hours and general stability in employment levels (Gurvich and Vakulenko 2017; Gimpelson 2019). Hence, adjustments at the extensive margin might need more time to materialize. In addition, if the amount of credit taken is not high, an increase in the hours worked by the family members might be sufficient to make debt servicing sustainable. To test for this possibility, we replaced the credit dummy with measures of credit amount, but we did not find any corroborative evidence (results are available upon request). The positive effect of credit amount on hours worked is confirmed, and we again do not find any effect on the extensive margin. Accordingly, the effect of a binary variable identifying households with high indebtedness (top 25% of the credit amount distribution) on the number of household members currently working is always statistically non-significant.

The second possible channel of transmission we hypothesized is better matching between jobs and workers’ skills/education. To test this conjecture, we constructed a measure of overeducation based on the so-called statistical approach (Fernández and Ortega 2008; Jauhiainen 2011; Ortiz and Kucel 2008), which compares individual educational attainment to the average of a reference group. Specifically, we define as overeducated those workers with a number of years of education exceeding the average value in the corresponding sector/occupation group by one standard deviation (see Section A.3 in the Online Appendix for further details). Compared to other methods (normative, self-assessment, income-ratio), the statistical approach has the advantage of being objective and relatively easy to apply if the breakdown of occupations/industries is sufficiently detailed, as in our case (see ILO 2014, for a comparative review of these methods). Columns 3 and 7 of Table 6 show that access to credit does not exert a significant influence on the number of overeducated workers in a household, and this result is confirmed for longer-run samples (see Table 7). Hence, our research hypothesis H2 does not receive empirical support, not even allowing for adjustments over a longer time horizon. Again, the explanation may lie in the characteristics of the labour market described above, which favour marginal adjustments rather than more radical employment changes.

The third channel of transmission hypothesized in Section 2 is related to capital income. To this end, we constructed a rent/capital income variable as the sum of the amounts received as rent, interest, dividends and insurance payments in the last 30 days (see Section A.3 in the Online Appendix for additional details). A descriptive analysis reveals that over the whole period only approximately 2.5% of the households reported positive capital/rent income. This is consistent with the evidence from wealth data: as discussed in Novokmet et al. (2018), the very low level of recorded financial assets owned by Russian households could be due to the fact that a limited subset of households owns very substantial unrecorded assets in offshore centres. As these households sit at the top of the income distribution, it is likely that our data only partially account for this income source, and this should be born in mind when interpreting our outcomes. The results presented in columns 4 and 8 of Table 6 indicate that having access to credit does not change the probability of activating market income sources other than labour income, and the results are confirmed in the longer-run mobility analysis (Table 7). Hence, the third research hypothesis H3 is not corroborated by our empirical evidence. Understanding the extent to which this depends on the limited information available on capital incomes remains an open question to be explored in future research.
### Table 7: Impact of credit on labour supply, overeducation and capital incomes (five- and seven-year time intervals)

|                  | a) 5-year time intervals | b) 7-year time intervals |
|------------------|--------------------------|--------------------------|
|                  | (1) FE-IV                | (2) FE-IV                | (3) FE-IV                | (4) FE-IV                | (5) FE-IV                | (6) FE-IV                | (7) FE-IV                | (8) FE-IV                |
| **Dependent variable:** | ΔN. workers<sub>t</sub> | ΔH. worked<sub>t</sub> | ΔN. overeducated<sub>t</sub> | ΔR/C income<sub>t</sub> | ΔN. workers<sub>t</sub> | ΔH. worked<sub>t</sub> | ΔN. overeducated<sub>t</sub> | ΔR/C income<sub>t</sub> |
| Credit           | 0.227**                 | 9.545**                 | 0.017                     | 0.024                    | 0.413***                | 32.219***               | 0.012                     | 0.026*                   |
|                  | (0.091)                 | (3.800)                 | (0.051)                   | (0.092)                  | (0.141)                 | (6.507)                 | (0.081)                   | (0.015)                  |
| Number of workers| -0.894***               | -0.998***               | -1.157***                 | -1.262***                | -1.153***               | -1.210***               | -1.405***                 | -1.405***                |
|                  | (0.017)                 | (0.026)                 | (0.025)                   | (0.028)                  | (0.020)                 | (0.025)                 | (0.054)                   | (0.054)                  |
| Rent/Capital Income | Yes                     | Yes                     | Yes                       | Yes                      | Yes                     | Yes                     | Yes                       | Yes                      |
| Other control variables | Yes                     | Yes                     | Yes                       | Yes                      | Yes                     | Yes                     | Yes                       | Yes                      |
| Year fixed effects | Yes                     | Yes                     | Yes                       | Yes                      | Yes                     | Yes                     | Yes                       | Yes                      |
| F test FE vs Pooled [p-value] | [0.000]                 | [0.000]                 | [0.000]                   | [0.000]                  | [0.000]                 | [0.000]                 | [0.000]                   | [0.000]                  |
| Hausman test FE vs RE [p-value] | [0.000]                 | [0.000]                 | [0.000]                   | [0.000]                  | [0.000]                 | [0.000]                 | [0.000]                   | [0.000]                  |
| Weak instrument F test statistics | 127.04†                 | 1290.84†                | 133.05†                   | 131.91†                  | 63.53†                  | 48.26†                  | 47.85†                    | 49.09                    |
| Hansen overidentification test [p-value] | [0.029]                 | [0.581]                 | [0.804]                   | [0.918]                  | [0.275]                 | [0.467]                 | [0.194]                   | [0.678]                  |
| Endogeneity test [p-value] | [0.009]                 | [0.018]                 | [0.789]                   | [0.517]                  | [0.005]                 | [0.000]                 | [0.808]                   | [0.115]                  |
| N                | 12220                   | 12220                   | 12220                     | 12220                    | 5380                    | 5380                    | 5380                      | 5380                     |
| R²               | 0.643                   | 0.690                   | 0.579                     | 0.625                    | 0.681                   | 0.736                   | 0.650                     | 0.719                    |

Notes: Each column of the table reports estimation results for the three-way recursive system in Eq. (4) referring to one transmission channel. The complete set of control variables includes all the regressors used in the baseline models presented in Table B2 of the Online Appendix. The instruments used to address the endogeneity of “Credit” are home ownership, cluster-average levels of credit access by household-head occupation and region and the regional number of bank branches per capita. Standard errors, clustered at the household level, are reported in parentheses. Complete estimation results are available upon request.

* Indicates that the weak instrument F-statistic exceeds the 5% Stock–Yogo critical value for a maximum 10% total relative bias

***, ** and * denote significance at the 1, 5 and 10% levels, respectively
The empirical findings presented and discussed so far support the idea that access to credit could exert its beneficial effects on mobility by triggering an increase in labour supply. Our results confirm for Russia what has been found in previous studies in other contexts (see Bottazzi et al. 2007; Del Boca and Lusardi 2003; Belka et al. 2007; Belkare et al. 2007; Butrica and Karamcheva 2014). Although an increase in labour supply can be expected to translate into higher income, empirical evidence on the whole chain of effects (i.e., the link between credit-induced labour supply and income growth) is, to the best of our knowledge, not available. Thus, to move a step forward in this direction, we estimate the recursive system of trivariate fixed-effects equations illustrated in Eq. (4). The estimation results are summarized in Table 8.17 In particular, Panel A of the table reports the impact of credit on the change in each of the channel variables considered (labour supply, overeducation and capital income), while Panel B illustrates the impact of each channel on income mobility.

The empirical results further corroborate our first research hypothesis H1, pointing out that when access to credit impacts labour supply at the intensive margin, the latter increases income mobility (columns 2 and 6). Conversely, we do not find evidence of a similar effect on the extensive margin of labour supply, overeducation or capital income: increases in these variables (Panel B of Table 8) have the expected impact on mobility, but they are unaffected by credit (Panel A).

Interesting insights emerge from the estimated coefficients on the “direct” effect of credit in the income mobility equation (Panel B). As explained in Section 3.2, since we are analysing the channels one at a time, the credit access variable is included in the third equation to control for the remaining potential channels of transmission from credit to mobility. In the whole sample (1994–2018), when the intensive labour supply margin is the channel of transmission considered (column 2), the estimated coefficient on credit in Panel B is insignificant, indicating that this channel captures the totality (or the majority) of the effect of credit on mobility. 18 When the other channels are estimated (columns 3 and 4), the credit variable in Panel B is positive and significant and has a coefficient close to that in the baseline model. This coefficient describes the impact of those channels that are at work (i.e., changes in the number of hours worked, according to our results) but that are not explicitly taken into account in the specific estimations. Interestingly, when analysing the results for the 2004–2018 period (columns 5–8), all the coefficients on credit in the third equation (Panel B) are statistically significant, even in the model that takes the intensive margin of labour supply into account (column 6). This suggests that in the 2004–2018 period, the channels of transmission are not exhausted by changes in labour supply but that there is still a residual transmission mechanism that we were not able to identify.

To assess the robustness of the results obtained from the recursive trivariate models, we estimate a six-equation recursive system, considering all the transmission channels simultaneously. The results, presented in Table B6 of the Online Appendix, support the evidence obtained from the three-equation models, confirming that the intensive margin of labour supply is the main channel of transmission. For the whole sample, we find that access to

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17 The simultaneous system of three equations is estimated with maximum likelihood techniques using the methods proposed by Roodman (2011).

18 The results in column 1 of Table 8 show that the coefficient on credit in the income mobility equation is not statistically significant (Panel B), despite the fact that the extensive margin of labour supply does not exert any significant effect on mobility. This is probably because changes in the number of workers partly capture the effect of changes in total hours worked (i.e., the intensive margin of labour supply). The correlation between changes in these two variables is indeed high (approximately 65%) and statistically significant.
Table 8  Channels of transmission from credit to income mobility, three-way recursive system (1994-2018 and 2004-2018)

|                      | (1)        | (2)         | (3)     | (4)     | (5)        | (6)         | (7)     | (8)     |
|----------------------|------------|-------------|---------|---------|------------|-------------|---------|---------|
| A) Credit/Mobility-channels equation |            |             |         |         |            |             |         |         |
| Dependent variable:  |            |             |         |         |            |             |         |         |
| ∆N. workerst         | 0.004      | 8.306**     | -0.0131 | -0.042  | -0.022     | 12.908***   | -0.016  | 0.004   |
|                     | (0.065)    | (3.645)     | (0.0329)| (0.088) | (0.076)    | (4.389)     | (0.045) | (0.124) |
| B) Income mobility equation |            |             |         |         |            |             |         |         |
| Dependent variable:  |            |             |         |         |            |             |         |         |
| ∆ln yit              | 0.008      | 0.009       | 0.128***| 0.135***| 0.072*     | 0.015*      | 0.177***| 0.185***|
|                     | (0.045)    | (0.008)     | (0.046) | (0.046) | (0.043)    | (0.009)     | (0.044) | (0.044) |
| ∆Number of workers   | 0.208***   |             |         |         |            |             |         |         |
|                     | (0.007)    |             |         |         |            |             |         |         |
| ∆Total hours worked | 0.003***   |             |         |         |            |             |         |         |
|                     | (0.000)    |             |         |         |            |             |         |         |
| ∆Number of overeducated workers | -0.075*** |             |         |         |            |             |         |         |
|                     | (0.012)    |             |         |         |            |             |         |         |
| ∆Rent/Capital Income |            |             |         |         | 0.025***   |             |         | 0.024***|
|                     |            |             |         |         | (0.003)    |             |         | (0.004) |
| Other control variables | Yes       | Yes         | Yes     | Yes     | Yes        | Yes         | Yes     | Yes     |
| Year fixed effects   | Yes        | Yes         | Yes     | Yes     | Yes        | Yes         | Yes     | Yes     |
| N                    | 35762      | 35762       | 35762   | 35762   | 24099      | 24099       | 24099   | 24099   |

Notes: Each column of the Table reports estimation results of the three-way recursive system in Eq. (4) referred to one transmission channel. The complete set of control variables includes all the regressors used in the baseline models presented in Table B2 of the Appendix. The instruments used to address the endogeneity of “Credit” are home ownership, cluster-average levels of credit access by household-head occupation and region and the regional number of bank branches per capita. Standard errors, clustered at the household level, are reported in parentheses. Complete estimation results are available upon request.

***, ** and * denote significance at the 1, 5 and 10% levels, respectively.
credit has a positive and statistically significant effect only on the number of hours worked, whereas its impact on mobility is entirely captured by changes in labour supply intensity and in the other channel variables. Furthermore, in the 2004–2018 period, we find a positive impact of access to credit on the capital income channel, which is significant at the 10% level. However, the low statistical significance of this coefficient suggests caution in interpreting this evidence as supportive of our research hypothesis H3.

7 Conclusions

In this paper, using household-level data from the Russian Longitudinal Monitoring Survey (RLMS), we have investigated the microeconomic drivers of short-term income mobility in Russia for the period 1994–2018.

The main contribution of our analysis to the existing literature lies in the analysis of the role of access to credit in triggering short-run household income growth. Although aspects of finance and credit markets have been extensively investigated among the drivers of long-run patterns of income inequality at the aggregate level, the possible impact on individual, short-run income growth has received little attention. In this regard, Russia represents a particularly interesting case, as over the period considered the credit and banking sectors evolved significantly amidst major crises and extensive reforms.

Descriptive evidence shows that short-term income mobility in Russia declined over time and more markedly from the mid 2000s when the ‘transfer’ component of mobility became dominant. Simultaneously, income movements increasingly shaped permanent changes in the income distribution. Households with access to credit also experienced higher average mobility and a higher probability of moving upwards in the income distribution.

Our empirical analysis on the drivers of income mobility relies on panel econometric approaches applied to a longitudinal sample of households observed repeatedly over three-year time intervals. The possibility of including fixed effects contributes to properly addressing endogeneity and omitted variable biases; the use of instrumental variable approaches accounts for the possible endogeneity of our main variable with respect to income growth. The empirical results clearly indicate that access to credit contributes significantly to boosting upwards income mobility, even when other household-level factors and institutional/policy dimensions are controlled for. The results are also robust to the definition of access to credit considered and to the shift to longer-run mobility. Assessing the heterogeneity in the pro-mobility effect of credit over time, we highlight that the significance of this effect is mainly driven by the impact of access to credit on income growth during the 2004–2018 period. In view of the institutional evolution implemented in Russia starting in the early 2000s, this might indicate that access to credit is beneficial for income growth in the presence of better functioning and more efficient banking and financial sectors. Looking at the heterogeneity across household types and characteristics, we find that the effect of credit is weaker for larger households and in the presence of children; conversely, its magnitude increases when the household head is older, not married/cohabitating and female.

Complementary analyses on what lies behind the pro-mobility effect of credit suggest that the main channel is an increase in labour supply at the intensive margin (i.e., hours worked). We also find evidence corroborating the idea that a similar effect materializes at the extensive margin but only for longer-run mobility. In contrast, our results do not support the conjecture of credit fostering better matching between jobs and workers’ skills/education or activating
capital incomes. Further research is therefore needed to identify the existence of transmission mechanisms other than labour supply that, especially in the most recent years, cannot be excluded. One avenue to explore is related to changes in wage rates as a consequence of a change in employer, in light of the literature uncovering remarkable heterogeneity in labour remunerations between firms with different productivity and innovation rates (see Abowd et al. 1999; Card et al. 2018). Investigating these mechanisms was not possible here due to the limited information about employers provided by RLMS; future empirical research based on matched employer/employee data might shed light on such questions.

Supplementary Information The online version contains supplementary material available at https://doi.org/10.1007/s10888-022-09525-x.

Acknowledgements A preliminary version of this paper was developed while Cristiano Perugini was a visiting researcher at the Bank of Finland Institute for Economies in Transition (BOFIT). Comments and suggestions by Zuzana Fungáčová, Mikko Mäkinen, Risto Herrala, Laura Solanko, Olga Kuzina and the seminars participants at BOFIT and at the Higher School of Economics (Moscow) are gratefully acknowledged. We are thankful to the editor, Garcia-Peñalosa, and three anonymous reviewers for providing very valuable comments and suggestions on previous versions of the manuscript.

Funding Open access funding provided by Università degli Studi di Perugia within the CRUI-CARE Agreement.

Data Availability The data used in this study are part of the Russian Longitudinal Monitoring Survey (RLMS) and are publicly accessible, upon acceptance of terms and conditions of use, at: https://data.cpc.unc.edu/projects/3/view#public_li

Declarations

Conflict of interests/competing interests The authors have no conflicts of interest to declare that are relevant to the content of this article.
The authors have no relevant financial or non-financial interests to disclose.

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