



Homeownership, mobility, and unemployment: Evidence from housing privatization

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ABSTRACT

Homeownership is believed to cause higher unemployment. This is because homeowners face higher mobility costs that limit their job search to local labor markets. Empirical tests of this prediction have yielded mixed results so far, possibly due to the endogeneity of homeownership. This paper proposes that the privatization of public housing in Central and Eastern Europe after the fall of the Iron Curtain was a substantial policy shock that generated largely exogenous assignment of homeownership to individual households. This facilitates a new test of the effects of homeownership on mobility and unemployment: First, our empirical results do not reject that homeownership reduces mobility. Second, our results are inconsistent with homeownership increasing unemployment.

1. Introduction

Previous research often finds that homeowners, all else equal, are less likely to move residence than renters (see Dietz and Haurin, 2003; Van Ommeren and Van Leuvensteijn, 2005, for overviews). In a seminal paper, Oswald (1996) argues that this should lead to higher unemployment risks, longer unemployment duration, and lower wages among homeowners. This is because lower mobility decreases the value of job offers in distant labor markets, limiting homeowners' job matching opportunities compared to renters.

Since its publication, a large number of contributions have tested various aspects of the "Oswald hypothesis." We add to this literature by studying housing privatization in the transition countries of Central and Eastern Europe (CEE) and using it as a source of exogenous assignment of homeownership to estimate the effects of homeownership on labor mobility and unemployment risks.

Understanding the labor market effects of homeownership is important for a number of reasons, three of which appear crucial to us: (i) People frequently strive to become homeowners, because owning a home may provide long-term security, allowing them to set roots, and

start a family.¹ Homeownership therefore seems to have an intrinsic social value. (ii) However, if homeownership increases unemployment, societies may face a tradeoff between these two policy variables. (iii) Public policies tend to cater to the preference for homeownership but without explicit regard for the labor market repercussions of those policies (see e.g. Laamanen, 2017). Knowledge of the labor market costs of homeownership is therefore required for housing policy to be set optimally and to strike a reasonable balance between the social benefits and costs of homeownership.

Previous literature documents a number of channels linking homeownership to immobility. These include homeowners' higher moving and transaction costs (Dohmen, 2005; Goodman, 1995; Haurin and Chung, 1998; Haurin and Gill, 2002; Oswald, 2019; Quigley, 2002; Van Ommeren, 2008), mortgage lock-in effects (Quigley, 1987), lock-in effects related to transaction and capital gains taxes (Best and Kleven, 2017; Hilber and Lyytikäinen, 2017; Lundborg and Skedinger, 1998; 1999; O'Sullivan et al., 1995; Slemrod et al., 2017; Van Ommeren and Van Leuvensteijn, 2005), homeowners' higher investments in local social capital (DiPasquale and Glaeser, 1999), loss aversion (Engelhardt, 2003), as well as mobility expectations and preferences

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¹ This preference seems to have existed for quite some time. Consider the quote from Talmud stating that "[t]he Torah has thus taught a rule of conduct: that a man should build a house, plant a vineyard and then marry a wife," (Sotah 44a). While few people plant vineyards today, marriage and homeownership are still frequent lifetime choices. The strong preference for homeownership among Czech first-time buyers is analyzed in Lux et al. (2017b).

(Lux and Sunega, 2012; Rohe and Stewart, 1996). Most of this literature finds that homeownership is associated with reduced mobility. However, two recent studies by Hilber and Lyytikäinen (2017) and Slemrod et al. (2017), studying discontinuities in housing transfer taxes, do not find significant distortive lock-in effects that would limit labor market mobility.

The empirical evidence concerning the effects of homeownership on unemployment is even more ambiguous (see Havet and Penot, 2010, for a review). Aggregate-level studies generally find a positive correlation between unemployment and the share of owner-occupied housing, both within and across countries (Blanchflower and Oswald, 2013; Green and Hendershott, 2001; Isebaert et al., 2015; Oswald, 1996). Individual-level studies, by contrast, tend to find that homeowners, if anything, do better on the job market than renters in terms of unemployment risk, its duration, and wages (Barceló, 2006; Battu et al., 2008; Coulson and Fisher, 2002; 2009; Flatau et al., 2003; Munch et al., 2006; 2008; Rouwendal and Nijkamp, 2010; Van Leuvensteijn and Koning, 2004).

An important methodological issue faced by this literature is the endogeneity of homeownership status. It is not clear, for instance, whether individuals become immobile because they acquire homes or whether less mobile individuals are more likely to opt for homeownership. Previous literature has mostly relied on instrumental variable approaches to identify the causal effect of homeownership on unemployment risks. The instruments used in this literature include the regional homeownership rate (DiPasquale and Glaeser, 1999; Munch et al., 2006; 2008; Van Leuvensteijn and Koning, 2004), regional price-to-rent ratios at the time of buying a house (Baert et al., 2014), tax deductions of mortgage interest and same-sex siblings (Coulson and Fisher, 2009), inheritance of money in young years (Gardner et al., 2001), dummies for US states (Green and Hendershott, 2001), or homeownership rate in subjects' region of birth and her parents' homeownership status (Munch et al., 2008). All of these are, however, subject to criticism. For instance, the much used regional homeownership rate has been criticized on account of potentially having a direct impact on labor market outcomes and also potentially being correlated with neighborhood characteristics that are in turn correlated with individual-level labor market outcomes (Blanchflower and Oswald, 2013; Coulson and Fisher, 2009; Laamanen, 2017). Similarly, although the same-sex of first two siblings predicts the presence of a third sibling, and thus can be thought as an exogenous housing demand shifter, it is also likely to affect parents' labor market outcomes through different channels than homeownership, violating the exclusion restriction.

Only few papers explicitly search for quasi-experimental evidence to assess the impact of homeownership on economic behavior. Wolf and Caruana-Galizia (2015) use the allied bombing campaign during the Second World War as an instrument for homeownership, finding a large positive effect on unemployment and a negative effect on mobility. However, one may find it difficult to assume that the economic effects of flattening German cities worked exclusively through homeownership. Laamanen (2017) argues that the deregulation of rental housing markets in Finland in the early 1990s created an exogenous variation in homeownership rates across regions. Higher profits from renting led to increased supply of rental dwellings, lowering homeownership rates in the regions that were subject to the deregulation. He finds that homeownership generates negative externalities in the form of increased unemployment. However, it is not entirely clear whether the identified effects pertain exclusively to homeownership or, instead, to the rental market deregulation itself as it could have spurred economic activity in deregulated regions, or both. Instrumenting homeownership in the former East Germany with pre-1990 homeownership status, Gebhardt (2013) finds that ownership increases asset-specific investment, such as bathroom renovations. He argues that because homeownership under the communist regime was economically meaningless, it was essentially randomly assigned. This observation is also relevant

for the interpretation of our results, as it suggests that endogeneity of homeownership might generally be a lesser issue in the case of transition economies.

The closest paper to ours is a recent study by Sodini et al. (2016), who exploit aborted privatization of public housing units in Stockholm to assess the effects of homeownership on a number of outcomes, including mobility and labor income. They find that homeownership induces households to work harder, increasing their labor income and—in contrast to previous literature—that homeownership increases household mobility. However, their data does not allow for an analysis of the effects of homeownership on unemployment. We complement (Sodini et al., 2016) in that the experiment we study, i.e. housing privatization in Central and Eastern Europe, was a large-scale transfer of homeownership to renters whose move in decisions have taken place before the Fall of the Iron Curtain, that is in the absence of standard housing and rental markets. Therefore, in our case the assignment of homeownership status prior to privatization differs since the main factors driving homeownership versus renting decisions in standard market economies were absent (Gebhardt, 2013).

Our methodological argument, which we consider to be the main innovation of the paper, is the following: Housing privatization in CEE countries was a result of a difficult-to-predict event: the fall of the Iron Curtain. It took the form of transferring property rights over publicly-owned housing units to the sitting tenants at substantially discounted rates (relative to market prices) and was predominantly organized at the central-government level. This led to a situation where (i) upon moving in, renters of housing units that were later privatized could in no way anticipate the privatization of their unit; (ii) decisions regarding which housing units would be designated to be privatized were beyond the direct control of individual tenants; and (iii) individuals had a high incentive to privatize and thus to comply with the “treatment”. Thus, housing privatization approximated an exogenous assignment of homeownership to individuals. This facilitates a new test of the effects of homeownership on individuals' labor market behavior and outcomes.

We, therefore, provide new estimates of the effects of homeownership on mobility and unemployment. To this end, we use data on ten CEE countries from the Life in Transition Survey (LiTS).² In addition, we use the East German sample of the German Socio-Economic Panel covering the post-1990 period. This data allows us to explicitly control for unobserved heterogeneity and tap into the role it may play in explaining the empirical relationship between homeownership and unemployment found in the existing literature. Finally, we extend the evidence concerning the labor-market effects of homeownership, which has been primarily studied in the mature market economies of Western Europe and the United States, to post-socialist countries in Central and Eastern Europe.

Our results are summarized in two points. First, our empirical tests do not reject that homeownership reduces mobility; indeed, the estimated effects tend to be negative and the confidence intervals contain substantively large effects. Second, our results are inconsistent with homeownership increasing unemployment; in fact, the estimated effects are systematically negative, although the confidence intervals contain small positive effects. These results hold across geographic regions, alternative specifications, and across the two estimation strategies in the two datasets we analyze.

2. Identifying the effects of homeownership

2.1. Estimation issues and the ideal experiment

The starting point of our discussion is the standard cross-sectional equation used in studies testing the Oswald hypothesis. This will also be

² The countries are: the Czech Republic, the former East Germany, Estonia, Hungary, Latvia, Lithuania, Poland, Romania, Slovakia, and Slovenia.

the central specification in the empirical part of this paper, that is

$$y_i = \beta_1 h_i + \beta_2' x_i + \gamma_d + e_i, \quad (1)$$

where i is an individual, y_i is the outcome of interest, either an indicator of i 's labor market status or a measure of her labor mobility, h_i is an indicator of homeownership status, which is equal to one if the household owns their home and zero otherwise, x_i is a vector of control variables, γ_d is the full set of country-district effects (including the intercept), and e_i is the residual. Thus, regression (1) compares homeowners and renters within a district. District effects are included to capture the impact of any unobserved region-specific shocks that may be correlated with homeownership. This is also important for the empirical strategy advanced in this paper, as privatization decisions were sometimes delegated to municipalities and therefore could have been affected by district characteristics (see Section 2.2 below).

The coefficient of interest in this regression is β_1 , which captures the partial correlation between homeownership and the respective outcome variable. For the estimates of β_1 to have a causal interpretation—and be a test of the Oswald hypothesis—homeownership has to be (as good as) randomly assigned, conditional on x_i and γ_d . In normal circumstances this is unlikely to be true because people choose whether to become homeowners or renters. Indeed, this is one of the most significant decisions in one's lifetime. Specifically, homeownership is arguably more attractive for individuals who do not plan to move. In addition, homeownership is likely to be more accessible for those with higher productivity and those who face low risk of unemployment.

These factors are typically unobserved to the econometrician. If this is the case, a negative correlation between homeownership and mobility could be due to the selection of individuals with a low propensity to move into homeownership (rather than due to homeownership leading to immobility). Similarly, a negative correlation between homeownership and unemployment, which is typically found in micro data, may not imply that the Oswald hypothesis is false. Rather, it may be an artifact of selection, since people with lower unemployment risks may be more likely to become homeowners.

An ideal, though impractical, experiment which would allow us to identify the causal effects of homeownership would therefore involve selecting a random sample from the subpopulation of renters to be assigned to homeownership and then comparing the labor market outcomes of these homeowners and renters. To isolate the effects of homeownership, the experiment would have to guarantee that (i) individuals have no control over homeownership assignment; (ii) individuals who are allocated a dwelling cannot refuse this offer; (iii) any wealth effects arising from the transfer of homeownership are compensated for;³ and (iv) ensure that both homeowners and renters do not trade dwellings after the treatment, so as to prevent self-selection after the experiment.

2.2. Institutional background

We argue that the privatization of public housing in CEE countries, following the fall of the Iron Curtain, may approximate such an experiment. This is because it effectively transformed a significant, and arguably exogenously selected, subpopulation of renters into homeowners. Broulíková and Montag (2020) provide a detailed account of the housing privatization processes both in transition countries and in Western Europe, and survey the extensive but scattered literature on

the topic. Key information pertaining to housing privatization in individual CEE countries is summarized in Table 1. Here we highlight the institutional features that are important for the empirical identification of the effects of homeownership. From that viewpoint, three facts about housing privatization in CEE countries stand out.

First, privatization of public housing in transition countries involved a large share of the housing stock, affecting a significant part of the respective countries' populations. This is because the share of publicly owned housing in these countries was large to begin with as communist regimes eliminated private rental markets and preferred public ownership. To illustrate, it is estimated that at the end of the Soviet era the share of public housing ranged from about 25% in Hungary, Slovakia, and Romania, to about one third in the Czech Republic, Poland, and Slovenia, 60% in Estonia, and 70% in Latvia (see the first column of Table 1 and references therein).⁴

After the fall of the Iron Curtain, new governments quickly began transferring the ownership of publicly owned housing units to private hands. During the first decade of the transition, about one half of the publicly-owned housing stock was privatized in the Czech Republic and Latvia (see first column of Table 1). Privatization rates in Estonia, Hungary, Lithuania, and Romania exceeded 90%. The extent of privatization, in addition to the fact that it was not a social policy project, means that privatization in CEE countries affected a broad group of residents.⁵

Second, the individuals eligible to privatize were the sitting tenants (see the second column of Table 1).⁶ In addition, tenants had no control over the assignment of eligibility to privatize their dwellings (see e.g. Lux, 2003). The housing estates to be privatized and the terms of the offer were mostly specified by the central government, as was the case in Estonia, Hungary, Latvia, Lithuania, Romania, Slovakia, and Slovenia. In the Czech Republic, the former East Germany, and Poland this was done by municipalities (see the third column of Table 1). In other words, eligibility to privatize was not an individual-level decision. However, in countries with decentralized privatization, one may be naturally concerned that municipalities may have been "cherry picking" houses for privatization or that their privatization decisions could have been affected by factors correlated with labor market performance of inhabitants in privatized houses.⁷ It is reassuring that, our estimates are not sensitive to inclusion or exclusion of these countries.

⁴ Even during the communist period, however, a substantial part of the housing stock was privately owned. In the former Czechoslovakia, a substantial share of dwellings (predominantly single-family detached houses) was in private hands: 43.5% in 1961 and almost 37% in 1990, according to census data (Czech Statistical Office, 2003). Another characteristic feature of housing structure in some Eastern Bloc countries was a significant proportion of housing stock owned by cooperatives, e.g. about 20% in the former Czechoslovakia and 25% in Poland (see first column of Tables 1 and 2).

⁵ See Table 2, on p. 18 below, for estimates of the share of households living in privatized dwellings across individual CEE countries as of 2010.

⁶ An exception to this rule is the former East Germany, where a non-negligible part of public housing was also sold to institutional investors.

⁷ Because of the decentralized nature of these privatizations, details are patchy. Skora (2003) states that local governments in the Czech Republic explicitly preferred to privatize buildings in bad condition. Indeed, getting rid of costs associated with ownership and maintenance of historically ill-maintained housing stock is often mentioned as one of the motivations behind housing privatization. On the other hand, Lis and Zwierchlewski (2015) state that Polish municipalities sold mainly the dwellings of a relatively higher standard than the average in the municipal stock. Mikula and Montag (2019) look at housing privatization in Brno, the second largest city in the Czech Republic. They report that the procedure for designation of houses for privatization was not regulated by the law or any city directive and is not documented in public sources. In an attempt to ascertain the exact procedure, we contacted five city officials, including the former Deputy Mayor, who were involved in the decision chain at the time. We met with and interviewed two of them, however they did not recall the details dating from over 20 years ago.

³ For example by reducing treated households' wealth by the net present value of the housing assigned to them, or by imposing *ad valorem* taxes equal to the income stream from the assigned wealth. Alternatively, renters could be compensated by providing them with additional wealth equal to the net present value of their rental home or with a permanent subsidy equal to the income stream from that wealth. Irrespective of which compensation method is chosen, it would have to account for the different liquidity of housing and other wealth.

Table 1
Housing privatization in Central and Eastern European countries.

| | Period and Extent | Privatizers | Decision Level | Sale Price | Details |
|----------------|---|---|--|---|--|
| Czech Republic | • Since 1992. | • Sitting tenants. | • Centrally given Right-to-Buy only for existing coop housing. | • About 80%, relative to the administrative price estimate. | • Later, coops could mutate into owners' associations, with the owners gaining full property rights to their unit. |
| | • About half of public housing stock was privatized by 2002. | • Housing cooperatives. | • Municipalities decided on the privatization scale and terms of most public dwellings. | • Discounts granted by municipalities. | |
| | • Prior to privatization, 38% of the rental units were public, 18% were housing cooperatives. | | | | |
| | • Since 1993, culminating between 1993 and 1999. | • Sitting tenants and members of cooperatives. | • Municipalities were centrally ordered to sell at least 15% of public rental housing and coops. | • During the first wave, the sale prices were far below market prices. | • Initially higher involvement of tenants and coop members than in the former West Germany. |
| Estonia | • Late 1990s and 2000s privatization to institutional investors. | • Institutional investors. | • Local-level decision for the remainder of the public housing stock. | • Federal sales to institutional investors for extremely low prices. | • Afterwards the approach to privatization similar to the former West Germany, i.e. privatization to institutional investors. |
| | • Prior to privatization, 25% of homeowners. | | | | |
| | • 1993–2001. | • Sitting tenants. | • Central government. | • Public capital vouchers (EVPs) distributed according to employment length. | • EVP awarded for one working year was approximately equal to the price of 1 m ² . Consequently, a person working for 40 years could already buy a 2-room apartment. |
| Hungary | • About 85–90% of the public housing stock was privatized. | | • Local authorities could restrict privatization – i.e. to select dwellings not available for privatization—but the pressure from both the central government and tenants was strong and this right was seldom used. | • Price set relative to the price of a "standard" dwelling in a prefabricated building | • EVPs were tradable. |
| | • Prior to privatization, 60% of the rental units were public. | | | | |
| | • Since 1983. | • Sitting tenants. | • Centrally granted Right-to-Buy for sitting tenants. | • Other discounts applicable. | • Most privatizers could buy their units just for their EVPs with no additional payment. |
| Latvia | • About 80% of the public stock was privatized by 2003. | | | • 10–15% of the market price. | • Private housing had similar conditions for public financial assistance as other tenures since 1983. This made private ownership more popular than other tenures. Thus, the importance of the public rental sector decreased even before privatization. |
| | • Prior to privatization, 23% of the rental units were public. | | | | |
| | • Since 1995, peak around the year 2000. | • Sitting tenants. | • The Central Housing Privatization Commission was responsible for the privatization process. | • No financial assistance by the government. | |
| | • About 55% of the public housing stock was privatized until 2000. | | • Terms (i.e. prices) specified centrally. | • Compensation vouchers distributed according to the length of residency/exile in Latvia during the period 1945–1992. | • One voucher equals 0.5 m ² of the residential space. |
| | • Prior to privatization, almost 70% of the rental units were public. | • Local governments helped with processing the privatization. | | | • Vouchers were tradable. |

(continued on next page)

Table 1 (continued)

| | Period and Extent | Privatizers | Decision Level | Sale Price | Details |
|-----------|---|---|--|---|---|
| Lithuania | <ul style="list-style-type: none"> • 1991–1995. • By July 1995 94% of possible flat privatization was accomplished. • Fast and extensive privatization even in comparison with Estonia and Latvia. • Since 1994. • Majority took place before 2002. • Prior to privatization, 35% of the rental units were public, 25% were housing coops. • 1990s. • Public housing stock almost completely privatized by 1999. • Prior to privatization, 75% of homeowners. • 1993–2008. • Prior to privatization, 25% of rental units were public, 20% were housing cooperatives. • 1991–1993. • Prior to privatization, 33% of the rental units were public. | <ul style="list-style-type: none"> • Sitting tenants. | <ul style="list-style-type: none"> • Prepared by the central government that encouraged rapid privatization due to simple administrability. • Local commissions set the price of dwellings but had no incentive to hinder the process. | <ul style="list-style-type: none"> • Privatization vouchers (ICs) distributed according to the age of the recipient. • Price set according to the construction characteristics, location etc. | <ul style="list-style-type: none"> • Trading ICs was not allowed. • Up to 80% of the selling price could be covered by ICs, the remainder had to be paid in cash. • Signatures of family members in the unit, enough ICs, and cash sufficed to carry out the privatization. • Majority privatized to tenants or coop members. • Other investors could privatize under centrally specified conditions, but municipalities chose the dwellings to be privatized. |
| Poland | | <ul style="list-style-type: none"> • Sitting tenants. • Housing cooperatives. | <ul style="list-style-type: none"> • Municipalities decided the privatization scale and terms of most public dwellings. • Right-to-Buy only for tenants in cooperative housing. | <ul style="list-style-type: none"> • Discount of up to 95% of the market value. | <ul style="list-style-type: none"> • Majority privatized to tenants or coop members. • Other investors could privatize under centrally specified conditions, but municipalities chose the dwellings to be privatized. |
| Romania | | <ul style="list-style-type: none"> • Sitting tenants. | <ul style="list-style-type: none"> • Entitled privatizers as well as contract conditions specified centrally. | <ul style="list-style-type: none"> • “Symbolic price” depending on the construction year. • Public financial assistance: 25-year loan with 4% interest rate. | <ul style="list-style-type: none"> • Privatization processed by specialized agencies. • Dwelling cannot be resold before the mortgage has been repaid. |
| Slovakia | | <ul style="list-style-type: none"> • Sitting tenants. | <ul style="list-style-type: none"> • Entitled privatizers as well as contract conditions specified centrally. | <ul style="list-style-type: none"> • Price derived from a comparable unit depending on the construction year. • Discounts between 30 and 80%. | <ul style="list-style-type: none"> • Free-of-charge transfer to the full ownership of cooperative members. |
| Slovenia | | <ul style="list-style-type: none"> • Housing cooperatives. • Sitting tenants. | <ul style="list-style-type: none"> • Municipalities had to privatize a dwelling within 2 years if approved by over a half of the tenants. • Entitled privatizers as well as contract conditions specified centrally. | <ul style="list-style-type: none"> • Discount over 30%. • Discount increased to 60%, if payment occurred within 60 days. | <ul style="list-style-type: none"> • Centrally specified conditions for selling restituted property to the sitting tenants. |

Note: Table based on Broulíková and Montag (2020). Information sources for individual countries: Czech Republic: Lux (2003), Struyk (1996), Grabmüllerová (2005). Estonia: Kask et al. (2005), Kursis (1999), Lux (2003), Struyk (1996). Hungary: Hegedüs et al. (1996), Struyk (1996), Tosics (1987). Latvia: Kursis (1999), Osa (2005), Tsenkova et al. (1996). Lithuania: Housing, Urbanization, and Development Fund (2005), Jurgaityte (2002), Kursis (1999), Milstead and Miles (2011). Poland: Lis and Zwierchlewski (2015), Lux (2003), Skiba (2005); Struyk (1996), Tibajuka (2009), Zawislak (2002). Romania: Dübel, et al. (2006); Lux (2003), Palacin and Shelburne (2005). Slovakia: Lux (2003), Struyk (1996), Zapletalova et al. (2003). Slovenia: Mandič and Clapham (1996); Sendi (1995), Struyk (1996).

Third, although those entitled to privatize their dwellings were not obliged to do so, the terms were highly attractive, rendering privatization a rational choice. Above all, the prices were substantially below the market value of the privatized units (see the fourth column of Table 1). To illustrate, the discounts from the estimated market price were in the range from 30 to 80% in Slovakia and from 30 to 60% in Slovenia.⁸ Hungarian and Polish discounts usually exceeded 80%, the latter sometimes reaching 95%. Even in the former East Germany, where the prices reflected restoration work conducted prior to privatization, housing units were sold under their market value. In the Baltic countries, tenants were awarded vouchers that could be used to pay a substantial part of the price of the privatized housing unit. The amount of vouchers that an individual would receive depended either on her age, length of employment, or length of residence in the country. In Estonia, most privatizers could obtain their housing units for vouchers only, without any additional payment. Some governments, such as in Romania, also provided public loan support to help individuals overcome liquidity constraints.

For completeness, it should be noted that CEE countries transferred housing estates to landlords also through restitution. This process returned housing property that was expropriated by communist governments in the Eastern Bloc after the Second World War, mostly in the late 1940s and early 1950s, to the original owners or to their descendants. Compared to housing privatization, restitution affected a smaller part of the housing stock and affected the ownership of whole housing estates, not individual dwellings (Hegedüs et al., 1996; Lux et al., 2017a).

2.3. Housing privatization as an exogenous policy shock

Based on this evidence, Figure 1 presents a stylized timeline of potential homeownership status assignment in CEE countries. During communism, people would naturally self-select to be homeowners or renters.⁹ While at this point the assignment of homeownership status was decision-based, it was arguably independent of future developments related to the fall of the communist regime in 1989, as those

⁸ Systematic evidence on privatization prices in the Czech Republic is lacking. However, three examples may give the reader an idea about the price setting mechanism. In one Czech town (Sokolov), Skora (2003) reports discounts of 80%. In Brno, the purchase price was determined according to the Mayor's Office directive no. 3/1996 as follows (Mikula and Montag, 2019). The baseline administrative price was set based on a directive of the Ministry of Finance (directive no. 178/1994). If the privatizing entity paid upfront, the purchase price was subject to a discount of 34% relative to the administrative price. Alternatively, the estate could be privatized on credit, in which case the purchase price was increased by nine%, relative to the administrative price. 30% of the purchase price would then have to be paid upfront and the remainder in five yearly installments (with no additional interest charged). Importantly, the privatizing entity was entitled to be reimbursed for any investment expenditures into the property, up to 40% of the purchase price. In summary, those paying upfront would effectively receive a discount of 60.4% [$=100 - (100 - 34) \times 0.6$], while those privatizing on credit would receive a discount of 34.6% [$=100 - (100 + 9) \times 0.6$], relative to the administrative price (ignoring the implicit discount due to the prices being set administratively, inflation, and the zero interest rate). Personal experience of one of the authors of this paper (Montag) may serve as a further anecdotal evidence. His apartment building in Prague was privatized in 1996 with an overall discount of 70%, relative to the administrative price estimate (which itself tended to be significantly below the market prices). Moreover, only 20% of the price had to be paid upfront. The remainder was paid over the following 10 years, with no interest charged. The cooperative, established by the tenants for the purpose of the privatization, became a full owner after the whole price was repaid in 2006. About ten years later, the cooperative transformed into an association of owners and individual household became direct owners of their units.

⁹ Although Gebhardt (2013) argues that even this process was essentially random as the difference between owning and renting was much diminished and individuals simply went for whichever form of tenure was available to them in the absence of free housing markets.

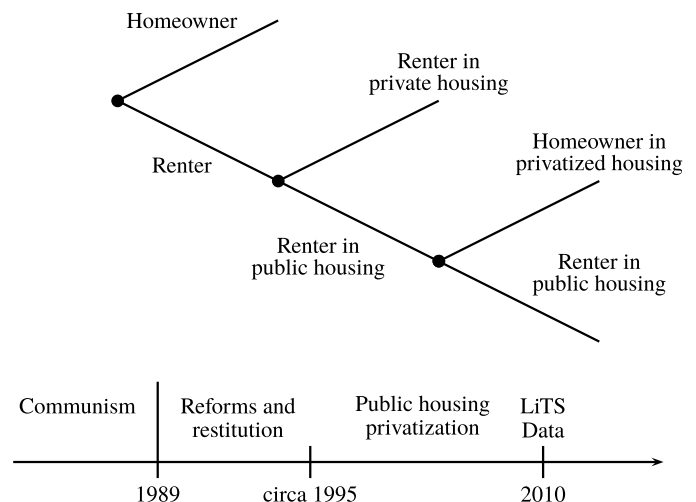


Fig. 1. Timeline of potential homeownership status of an individual tenant in Central and Eastern Europe. The initial sorting was an individual's choice; however, it could not have been influenced by the unexpected fall of communism and ensuing reforms that included restitution and the privatization of public housing. The post-1989 nodes are not decision points, but rather they can be thought of as the exogenous assignment of the respective statuses, conditional on being a renter in 1989.

could not have been foreseen.¹⁰

However, the quasi-experiment we focus on happens within renters in the post-1989 period: At the beginning, essentially all renters would be renters in publicly owned housing. After the reforms, some renters would find themselves in a house that was historically owned by a landlord, which would be returned to her in the restitution. These renters would end up being renters in a privately owned house, with an option to become homeowners through the normal procedure of purchasing or building a home. Of the remaining renters in public housing, some would later be able to privatize, while the others would remain renters. This last step of the privatization process produces exogenous assignment of homeownership that can be used to identify its effects on labor market outcomes.

Relative to the ideal experiment outlined above, there are two identification issues that need to be addressed at this point, one related to privatization and one to the data. Specifically, privatization in the transition economies was often associated with a substantial discount on the price paid for the privatized housing unit, relative to market prices. This has the advantage in that it generated a high compliance rate among tenants who were offered the opportunity to privatize. However, it also has a disadvantage in that the transfer of homeownership implied a transfer of wealth. As a consequence, the effects of the transfer of homeownership cannot be separated from the wealth effect. To the extent financial markets in transition countries were underdeveloped and other factors discussed in below increased privatizers' transaction costs, privatization resulted in a transfer of mainly

¹⁰ One may even argue that it was independent of future labor market outcomes, as the nature of the economic system changed from centrally planned to standard market economies. Socialist economies were characterized by compressed income distributions and the absence of (formal) unemployment. In fact, individuals in these countries were obliged to work and the government was responsible for assigning jobs to people. Skills that may not have generated substantial premiums during communism, such as language skills or entrepreneurial talent became highly valuable after the fall of the Iron Curtain. At the same time, other characteristics like communist party membership could become a disadvantage. On the other hand, there is evidence that former communists often benefited from transition, exploiting their connections (see Petrović, 2001). However, none of these developments could have been foreseen by the individuals prior 1989.

illiquid wealth. This fact may lessen the wealth effect associated with privatization.

Irrespective of this, the wealth effect operates in the same direction as the expectations shaped by the Oswald hypothesis, as wealthier persons have been shown to be less mobile in a number of previous studies (e.g. [Dustmann and Okatenko, 2014](#)) and can also be expected to search less intensively for a job when unemployed (see [Rogerson et al., 2005](#)). Thus wealthier people are likely to have lower mobility and suffer higher unemployment risks, all else equal. In other words, if homeownership lowers mobility and increases unemployment, homeownership due to privatization should amplify these effects. Furthermore, as pointed out by [Sodini et al. \(2016\)](#), while the concurrency of the wealth effect and the effects of homeownership precludes the identification of the pure homeownership effect, this may be a lesser concern if one is interested in the effects of policies promoting homeownership, as all such policies are also associated with a redistribution of wealth in favor of homeowners.

The second issue is related to the fact that our main data is a cross-section from 2010, about a decade since the main privatization took place. This can be a problem as renters and homeowners had enough time to rearrange their homeownership status through normal market transactions. For example, individuals with a high propensity to migrate could have sold their privatized dwellings and renters with a high desire to settle may have bought new dwellings.¹¹

Indeed, by 2010, the countries of Central and Eastern Europe were already normal market economies in most respects ([Shleifer and Treisman, 2014](#)). They had been EU members for six years (except for the former East Germany, which became a member following German reunification in 1990, and Romania, which joined in 2007), members of the OECD for at least three but often more than 15 years, and some had adopted the euro.

However, housing markets in these transition countries differed from those of the countries analyzed in the literature so far. In particular, transaction costs for housing in the CEE economies were substantially larger than in most developed market economies and, despite rather high homeownership rates, the share of mortgage finance has been rather low ([Bloze, 2009](#); [Dübel et al., 2006](#); [Stephens et al., 2015](#)). To this day, housing market liquidity and the supply of housing loans in most of these countries remain relatively low, as evidenced by the low share of mortgage-financed housing in these countries (see [Table 2](#) on p. 18 below). These factors should amplify the negative effects of homeownership on mobility, suggesting that transition countries are particularly well suited for examining the labor market effects of homeownership.

In addition, privatization often resulted in ownership burdened by increased transaction costs that limited further resales. In Romania, for instance, a privatized unit could not be sold before the mortgage had been repaid ([Lux, 2003](#)). In Latvia, ownership titles to individual apartments were transferred only after the entire multi-family building was privatized ([Osa, 2005](#)). In the Czech Republic and Poland, the tenants would often have to form a cooperative, which would privatize the housing estate and own all of the individual apartments therein.¹² The tenants would be members of these coops. Although it was possible

to sell the coop membership, banks in the Czech Republic would not provide mortgages for coop-owned dwellings. As a result, individuals faced increased transaction costs and lower selling prices if they were to sell a privatized apartment. Only in the second step could coops transform themselves into owner associations and transfer the property rights to the individual tenants, a process that could take a year or more ([Lis and Zwierchlewski, 2015](#); [Lux, 2003](#)). In Poland, coop members were given the right to transform their memberships into full ownership starting from 2001 ([Zawislak, 2002](#)).

These factors are likely to reduce the impact of sorting of homeowners and renters after privatization. They should also push results in the direction favorable for the Oswald hypothesis, as they lead to higher transaction costs and thus should have further reduced mobility. Consistent with these expectations, [Broulíková and Montag \(2020\)](#), using the 2006 and 2010 LiTS data, find that in eight out of the ten CEE countries we focus on the shares of households who state they are living in privatized units were either similar or higher in 2010 compared to 2006.¹³

3. Cross-country evidence from transition economies

3.1. The 2010 life in transition survey

To advance the analysis of the effects of homeownership on mobility and unemployment, we first use data from the 2010 wave of the Life in Transition Survey (LiTS) conducted by the European Bank for Reconstruction and Development. The 2010 LiTS data has the advantage that it asks respondents questions about their mobility, employment, as well as homeownership. Most importantly for us (and unlike in other surveys), the respondents were asked whether they became homeowners through privatization. LiTS 2010 is thus the best available dataset for studying the effects of homeownership in transition economies.¹⁴

The survey was conducted in 28 transition countries in Central and Eastern Europe and the former Soviet Union, plus five Western European countries (Germany, France, Italy, Sweden, and the United Kingdom), as well as Mongolia and Turkey. For most countries, 50 Primary Sampling Units (PSUs) were randomly selected from the local electoral units, with the probability of selection proportional to PSU size. For Russia, Ukraine, Uzbekistan, Serbia, Poland, and the UK, the sample consists of 75 PSUs. Subsequently, 20 households were randomly chosen within each PSU for interviews. For each country, the database therefore contains a sample of about 1000 or 1500 households.

Interviewers had to visit each selected household at least three times before replacing it with another one. In 79% of cases, however, the interviews were completed on the first visit. The questionnaire consists of two parts: a household roster and expenses form, answered by the head of the household, and the main part answered by the principal respondent, a randomly selected member of the household above the age of 18. In 61% of the cases, the household head and the principal respondent were the same person, while in the remainder two different interviews were conducted in the same household.¹⁵

¹¹ A reviewer pointed to us that this factor in itself would make our sample of homeowners who privatized their unit selected in favor of the stayer types. If that were the case, we should be more likely to find homeownership to worsen mobility and unemployment.

¹² In the Czech Republic, municipalities could also privatize directly to individual tenants. There is no data which would enable us to specify more precisely the relative importance of these two approaches, i.e. privatization directly to individual tenants and to coops. From the municipalities' viewpoint, however, privatizing to coops was likely to be administratively less burdensome than dealing with individual tenants. This should make privatization to coops more attractive, particularly when a large number of housing units was involved.

¹³ The former East Germany was not included in the 2006 survey. Only data from Slovakia show a decrease in the number of privatizers, from 22 to 2%. However, this change is likely an artifact of misinterpretation of the ownership question in 2006 by some respondents.

¹⁴ The earlier wave of the LiTS survey from 2006 neither contains questions about mobility nor about employment and thus was not suitable for this paper. The 2016 LiTS wave does not ask homeowners how they acquired their dwelling so that privatizers cannot be identified. For more details and access to the LiTS data see the <http://www.ebrd.com/what-we-do/economic-research-and-data/data/lits.html> (last accessed on June 29, 2020).

¹⁵ For more details about the surveying process and outcomes see <http://www.ebrd.com/news/publications/special-reports/life-in-transition-survey-ii>.

Table 2
Homeownership structure by country (proportions).

| | | Ownership Type | | | | | |
|----------------------------|--------|----------------|-----------------|------------------|-------------|-----------|--------------|
| | Rented | Privatized | Bought or Built | | Cooperative | Inherited | Observations |
| | | | With Mortgage | Without Mortgage | | | |
| Central and Eastern Europe | | | | | | | |
| Czech Republic | 0.21 | 0.05 | 0.15 | 0.24 | 0.12 | 0.24 | 897 |
| East Germany | 0.73 | 0.06 | 0.10 | 0.02 | 0.00 | 0.09 | 139 |
| Estonia | 0.16 | 0.31 | 0.15 | 0.24 | 0.01 | 0.13 | 661 |
| Hungary | 0.14 | 0.01 | 0.38 | 0.32 | 0.04 | 0.11 | 731 |
| Latvia | 0.33 | 0.45 | 0.05 | 0.09 | 0.02 | 0.07 | 689 |
| Lithuania | 0.10 | 0.27 | 0.10 | 0.27 | 0.02 | 0.24 | 667 |
| Poland | 0.12 | 0.10 | 0.12 | 0.32 | 0.13 | 0.21 | 1241 |
| Romania | 0.06 | 0.08 | 0.17 | 0.44 | 0.01 | 0.25 | 758 |
| Slovakia | 0.12 | 0.02 | 0.15 | 0.38 | 0.11 | 0.23 | 922 |
| Slovenia | 0.13 | 0.08 | 0.10 | 0.49 | 0.01 | 0.19 | 839 |
| Western Europe | | | | | | | |
| Great Britain | 0.43 | 0.03 | 0.48 | 0.05 | 0.00 | 0.01 | 1084 |
| Italy | 0.24 | 0.25 | 0.26 | 0.09 | 0.01 | 0.15 | 874 |
| Sweden | 0.31 | 0.14 | 0.48 | 0.05 | 0.00 | 0.02 | 676 |
| West Germany | 0.56 | 0.10 | 0.21 | 0.03 | 0.00 | 0.10 | 699 |

Data source: Life in Transition Survey 2010.

3.2. Estimation sample and summary statistics

Our main focus is on ten CEE countries for which we were able to collect sufficient information about their housing privatization process: the Czech Republic, the former East Germany, Estonia, Hungary, Latvia, Lithuania, Poland, Romania, Slovakia, and Slovenia.¹⁶ For completeness, we also provide estimates for former Soviet Union (FSU) countries and the Balkans. We restrict the sample to respondents who were 18 years or older in 1990, and thus could have in principle participated in privatization, and were at most 65 years old in 2010, the year of the survey. We drop households for which the homeownership variable is missing (2.2% of the observations).¹⁷ Our analysis dataset contains 4525 individuals from the ten CEE countries. Sampling weights are provided and we use them in our regressions.

Three variables are central to our analysis: (i) LiTS respondents were asked, in two separate questions, whether they would be willing to move elsewhere in the country or abroad for employment reasons. From these two questions we form our measure of labor mobility, an indicator equal to one if either answer is affirmative.¹⁸ (ii) With regard to job market status, we define as unemployed those individuals who do not work and are actively looking for a job. (iii) Lastly, respondents were asked whether they are renters or homeowners. Homeowners were then asked how they acquired ownership of their dwelling, which could be through a purchase, building a house themselves, via inheritance, as a co-operative, or through privatization. We call “privatizers” those who state they became homeowners via privatization.

Table 2 summarizes the homeownership structure in the CEE countries. For comparison and context, we also provide estimates for

four Western European countries surveyed in LiTS. In line with the literature on housing markets in transition countries cited above, the data shows that post-communist countries, except for the former East Germany and Latvia, tend to have higher homeownership rates (lower rentership rates) than any of the four Western European countries. Despite this, the shares of mortgage-financed housing in CEE countries, except for Hungary, are always substantially lower than in the Western European countries, supporting the assumption of illiquid housing markets in transition countries.

However, privatization does not seem to be the main factor behind the high incidence of homeownership. Rather, high ownership rates in CEE countries appear to be due to the high share of households living in inherited houses and houses bought or built without mortgages. This is likely an artifact of the practically nonexistent rental and housing markets during the communist era when people often built their homes by themselves, either as private individuals or members of co-operatives, as this was the most accessible way of becoming a homeowner (Stephens et al., 2015). A second probable factor is represented by the relatively underdeveloped financial markets, particularly mortgage markets, as of 2010, as apparent from Table 2.

Table 3 presents the descriptive statistics of individuals' willingness to move and unemployment in our estimation sample, as well as a number of demographic and household characteristics reported in the survey, broken down by homeownership status. About 27% of people in CEE countries are willing to move for job reasons. The data confirms that renters are significantly more willing to move relative to homeowners—privatizers as well as those that did not privatize. These patterns are consistent with the previous research on homeownership and mobility and with the high levels of homeownership in CEE countries.

About nine percent of individuals in our estimation sample are unemployed. The share of unemployed is, actually, slightly higher among renters than privatizers and homeowners who did not obtain their dwelling in privatization, by two and four percentage points, respectively (although the first comparison is not statistically significant).¹⁹ Taken at face value, these patterns are consistent with empirical results found in many of the previous individual-level studies, while being rather inconsistent with the idea that homeownership increases unemployment.

However, the summary statistics in Table 3 also suggest that these

(footnote continued)

html (last accessed on June 29, 2020).

¹⁶ We use the regional identifiers in the data to split Germany into East and West. In this separation Berlin was dropped because the population of West Berlin in 1990 was almost twice the size of East Berlin. We also drop Bulgaria as the LiTS data does not contain any homeowners who privatized their dwellings.

¹⁷ We do not restrict the data to individuals in the active labor force, since labor force status is not exogenous. As reported below, results are unaffected if the sample is restricted to individuals active in the labor force.

¹⁸ Our mobility indicator is therefore one of willingness to migrate rather than of actual migration. Although not all stated migration and return intentions can be expected to be realized, such data has previously been shown to accurately capture the determinants of migration behavior (see van Dalen and Henkens, 2013).

¹⁹ Throughout the paper, we use the five percent threshold in order to determine statistical significance.

Table 3
Summary statistics: Central and Eastern Europe (unweighted sample means).

| | Means | | | | t-tests (p-values) | |
|-------------------------------|-------------|---------|------------------------|----------------------------|------------------------|---------------------------------------|
| | Full Sample | Renters | Homeowners Privatizers | Homeowners Non-Privatizers | Privatizers v. Renters | Homeowners Non-Privatizers v. Renters |
| Willing to move for job (= 1) | 0.27 | 0.36 | 0.28 | 0.26 | 0.01 | < 0.01 |
| Unemployed (= 1) | 0.09 | 0.12 | 0.10 | 0.08 | 0.15 | 0.01 |
| Female (= 1) | 0.59 | 0.56 | 0.67 | 0.58 | < 0.01 | 0.37 |
| Age | 51.49 | 49.97 | 52.63 | 51.45 | < 0.01 | < 0.01 |
| Years of education | 13.09 | 13.06 | 13.46 | 13.02 | < 0.01 | < 0.01 |
| Married (= 1) | 0.64 | 0.47 | 0.58 | 0.68 | < 0.01 | < 0.01 |
| Household size | 2.56 | 2.24 | 2.32 | 2.65 | 0.24 | < 0.01 |
| Number of children | 0.41 | 0.44 | 0.31 | 0.42 | < 0.01 | 0.66 |
| Willing to take risks (1–10) | 4.31 | 4.29 | 4.28 | 4.31 | 0.97 | 0.86 |
| Communist before 1989 (= 1) | 0.07 | 0.05 | 0.08 | 0.07 | 0.03 | 0.05 |
| Parents were communists (= 1) | 0.13 | 0.14 | 0.12 | 0.13 | 0.39 | 0.43 |
| Foreigner (= 1) | 0.06 | 0.04 | 0.15 | 0.04 | < 0.01 | 0.44 |
| Observations | 4525 | 458 | 720 | 3347 | | |

Data source: Life in Transition Survey 2010.

subpopulations are not identical, although the differences appear substantively small: Privatizers, compared to renters, tend to be more often females, are somewhat older, slightly more educated, more often married, have fewer children, and were more likely to have had been communist party members. Individuals speaking a foreign language at home have also been over-represented among privatizers relative to both other homeowners and renters in CEE countries; however, this difference is mainly driven by the Baltic countries with high privatization rates and large Russian minorities. On the other hand, privatizers are not different from renters and other homeowners in terms of the sizes of their households, their willingness to take risks, and their parents' communist background.

3.3. The effects of homeownership on mobility and unemployment

Table 4 reports alternative specifications of regression (1) for respondents' willingness to move. To facilitate a comparison with the existing literature, we report results for the full sample of all homeowners and renters as our baseline estimates. However, β_1 estimated in the full sample may be biased due to the endogeneity of homeownership.

Our preferred models are estimated on the sample restricted to renters and privatizers only. As already discussed in Section 2, this subsample consists of regular renters and former renters who have privatized their housing units. Because of the nature of housing privatization in CEE countries, this results in a more exogenous homeownership status within the population of renters and privatizers. These restricted-sample estimates are therefore likely to yield a more accurate portrayal of the causal effects of homeownership.

For each subsample, we estimate and report three specifications of Eq. (1) using OLS, to which we report as “short”, “long”, and “flexible” for brevity.²⁰ All reported estimates use “federalist” survey weights, supplied in the LiTS data, which account for different sample sizes and populations of individual countries; that is, all countries have equal weight in the results we report.²¹

The short specifications control only for respondents' age and country-district effects. Controlling for age is necessary as individuals'

job market experience, wealth, and possibly preferences vary over the life cycle. Controlling for district effects is necessary because in some countries privatization was implemented by municipalities and could have been affected by region-specific characteristics and region-specific economic shocks. For the same reason, the reported standard errors are corrected to account for clustering at the regional level. The long specifications control for all characteristics reported in Table 3 above, interacted with gender when relevant. Finally, in the flexible specifications, we replace age, years of education, and household composition (number of children and number of adults) with the full set of dummies for each value of these variables. Because most of these characteristics are not exogenously assigned, we are agnostic as to whether the estimates from the long or short specification are preferable, leaving this judgment to the reader.

Our baseline full-sample estimates in columns (1) through (3) of Table 4 indicate that homeowners in the CEE countries are robustly less mobile than renters. This is consistent with the findings in the previous literature on developed market economies and suggests that these patterns are more general, extending to post-socialist countries. Furthermore, the magnitudes of the coefficients are also substantively significant; comparing the full-sample estimates in Table 4 with the sample averages in Table 3 suggests that, after controlling for regional effects and respondents age, homeowners' willingness to move is nearly one fifth lower than that of renters.

Shifting attention to our preferred models in columns (4) through (6), where we compare renters and privatizers, we see that the willingness to move of privatizers is still estimated to be negative in the short specification, although the magnitude is smaller by about one half. However, when the differences in characteristics are controlled for in long and flexible specifications, the point estimates approach zero. We interpret these estimates as the absence of evidence that that homeownership makes people less mobile. However, the estimates are rather imprecise, the 95% confidence interval (CI) for the point estimate in column (6) ranges from – 0.091 to 0.113, so that substantively important positive or negative effects cannot be ruled out.

Our key results for unemployment are reported in Table 5. The full sample estimates in columns (1) through (3) suggest that homeowners in CEE countries face smaller unemployment risk than renters, however these differences are not statistically significant and decline (in absolute terms) as controls are added.

Shifting attention to our preferred specifications comparing privatizers and renters, reported in columns (4) through (6), homeownership is associated with four to five percentage points lower unemployment risk coefficients, which are substantively large but imprecise estimates (95% CI for the estimate in column 6 ranges from – 0.094 to 0.020), so that one cannot rule out zero or substantively small positive effects.

²⁰ All our regressions contain country-district dummies, a dummy per 20 observations or less. We therefore estimate linear probability models in order to avoid the biases (of estimated coefficients as well as standard errors) due to the incidental parameters problem in nonlinear models (Greene, 2004).

²¹ Because privatization rates differ widely country-by-country, we have corrected the weights for regressions estimated in the sample of privatizers and renters to maintain equal weight of all countries. As reported below, our results did not change when we re-estimated our regressions without weights.

Table 4
Homeownership and mobility.

| Sample: | Full Sample | | | Privatizers and Renters | | |
|-------------------------------|---------------------------------|---------------------------------|---------------------------------|-------------------------|---------------------------------|---------------------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Homeowner (= 1) | − 0.069 ⁺ (0.028) | − 0.057 ⁺ (0.028) | − 0.062 ⁺ (0.028) | − 0.032 (0.047) | − 0.002 (0.048) | 0.011 (0.052) |
| Age | − 0.017 (0.013) | − 0.024 (0.013) | | − 0.053 (0.036) | − 0.045 (0.030) | |
| Age ² /100 | 0.008 (0.013) | 0.016 (0.012) | | 0.043 (0.034) | 0.038 (0.028) | |
| Years of education | | 0.012* (0.004) | | | 0.027* (0.009) | |
| Willing to take risks (1–10) | | 0.038* (0.003) | 0.037* (0.003) | | 0.058* (0.007) | 0.058* (0.007) |
| Female (= 1) | | − 0.025 (0.042) | − 0.042 (0.041) | | − 0.111 (0.079) | − 0.098 (0.079) |
| Married (= 1) | | − 0.072* (0.026) | − 0.088* (0.028) | | − 0.098 ⁺ (0.050) | − 0.150* (0.052) |
| Number of children | | − 0.019 (0.015) | | | 0.072 (0.040) | |
| Number of adults | | 0.007 (0.013) | | | − 0.027 (0.034) | |
| Foreigner (= 1) | | − 0.018 (0.040) | − 0.011 (0.039) | | 0.007 (0.075) | 0.008 (0.085) |
| Communist before 1989 (= 1) | | 0.027 (0.034) | 0.027 (0.033) | | − 0.045 (0.057) | − 0.048 (0.050) |
| Parents were communists (= 1) | | 0.022 (0.020) | 0.021 (0.020) | | − 0.0002 (0.046) | 0.006 (0.045) |
| Married × Female | | − 0.021 (0.030) | − 0.005 (0.032) | | − 0.019 (0.080) | 0.030 (0.076) |
| Number of children × Female | | − 0.013 (0.019) | − 0.023 (0.020) | | − 0.097 ⁺ (0.045) | − 0.124 ⁺ (0.055) |
| Number of adults × Female | | 0.0004 (0.018) | 0.002 (0.018) | | 0.039 (0.044) | 0.024 (0.043) |
| Age dummies | − | − | Yes | − | − | Yes |
| Education dummies | − | − | Yes | − | − | Yes |
| Household composition dummies | − | − | Yes | − | − | Yes |
| Country-district dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 4525 | 4525 | 4525 | 1178 | 1178 | 1178 |
| Adjusted R ² | 0.071 | 0.122 | 0.127 | 0.101 | 0.212 | 0.240 |

Note: The outcome variable is an indicator coded as one if the respondent states he or she would be willing to move within the home country or abroad for employment reasons. Individual models are estimated by OLS with observations weighted using “federalist” sampling weights supplied with the LiTS data so that all countries have equal weight. Standard errors clustered at the country-district level are in parentheses: ⁺ $p < 0.05$, * $p < 0.01$. Data source: Life in Transition Survey 2010.

In summary, our main results do not reveal homeownership to be causing higher risks of unemployment amongst homeowners. These results are consistent with the findings in the existing literature from developed market economies.

3.4. Robustness checks and evidence from the FSU and the balkans

Table 6 presents alternative specifications of our preferred estimates (i.e. those comparing privatizers and renters) from Tables 4 and 5. First, because of their lower mobility, the Oswald hypothesis also implies that homeowners should face longer unemployment spells. We therefore replace the unemployment indicator in our regressions with a long-term unemployment indicator. This variable is equal to one if the respondent did not have any job, was actively looking for one, and was without job for more than one year at the time of the interview. The results are statistically very similar to our main estimates. Privatizers thus did not face longer unemployment spells than renters.

The second row addresses the concern that the decentralized nature of privatization in the Czech Republic, East Germany, and Poland is less likely to approximate experimental setting, as municipalities may have been “cherry picking” houses for privatization or their privatization decisions could have been affected by factors correlated with labor market performance of inhabitants in privatized houses. We thus re-estimate our main models for the subset of countries where housing privatization was centralized at the government level. However, the

resulting estimates are qualitatively as well as quantitatively very similar to our main estimates in Tables 4 and 5.

The third row addresses the concern stemming from the time lag between our data and the main privatization episodes occurring during the 1990s and early 2000s. During that time individuals could have changed their residence, resulting in selected sample. We therefore re-estimate our regressions for the subset of countries where post-privatization transaction costs were arguably higher, as discussed in Section 2.3, namely, the Czech Republic, Poland, and Romania. We note that doing this results in sample size reduced by about 70% and thus increases the imprecision of our estimates. The results suggest that homeownership is associated with lower willingness to move as well as unemployment. The estimates are substantively larger (in absolute terms) than our baseline estimates, however the associated standard errors make these results very imprecise.

In a next robustness check, we drop all three Baltic countries, as they may be different from other Central European countries, particularly due to their previously belonging to the Soviet Union, but also due to somewhat different privatization methods (see Table 1) and high privatization rates (see Table 2). As a result, the estimated effect of homeownership on mobility increases and becomes positive in long and flexible specifications, although the estimates are never statistically significant. Importantly, the estimated effect on unemployment remains negative and is larger in magnitude, although it remains not significant statistically.

Table 5
Homeownership and unemployment.

| Sample: | Full Sample | | | Privatizers and Renters | | |
|-------------------------------|---------------------------------|---------------------------------|---------------------------------|-------------------------|---------------------------------|---------------------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Homeowner (= 1) | − 0.032 ⁺ (0.015) | − 0.018 (0.016) | − 0.014 (0.015) | − 0.052 (0.027) | − 0.046 (0.029) | − 0.037 (0.029) |
| Age | 0.027 [*] (0.008) | 0.027 [*] (0.008) | | 0.024 (0.019) | 0.012 (0.022) | |
| Age ² /100 | − 0.030 [*] (0.008) | − 0.030 [*] (0.008) | | − 0.026 (0.018) | − 0.016 (0.020) | |
| Years of education | | − 0.012 [*] (0.003) | | | − 0.010 (0.007) | |
| Willing to take risks (1–10) | | − 0.001 (0.002) | − 0.001 (0.002) | | 0.001 (0.005) | 0.001 (0.005) |
| Female (= 1) | | − 0.077 [*] (0.026) | − 0.076 [*] (0.026) | | − 0.055 (0.089) | − 0.063 (0.089) |
| Married (= 1) | | − 0.080 [*] (0.019) | − 0.094 [*] (0.021) | | − 0.151 [*] (0.042) | − 0.177 [*] (0.053) |
| Number of children | | − 0.002 (0.008) | | | − 0.019 (0.030) | |
| Number of adults | | − 0.014 (0.008) | | | 0.020 (0.042) | |
| Foreigner (= 1) | | 0.040 ⁺ (0.020) | 0.041 (0.021) | | 0.036 (0.032) | 0.040 (0.037) |
| Communist before 1989 (= 1) | | − 0.021 (0.020) | − 0.016 (0.020) | | − 0.079 (0.048) | − 0.083 (0.052) |
| Parents were communists (= 1) | | 0.018 (0.016) | 0.016 (0.016) | | − 0.034 (0.037) | − 0.032 (0.037) |
| Married × Female | | 0.059 [*] (0.021) | 0.063 [*] (0.022) | | 0.147 [*] (0.047) | 0.184 [*] (0.048) |
| Number of children × Female | | − 0.007 (0.012) | − 0.007 (0.013) | | 0.001 (0.036) | − 0.024 (0.040) |
| Number of adults × Female | | 0.010 (0.010) | 0.009 (0.010) | | − 0.036 (0.041) | − 0.043 (0.041) |
| Age dummies | − | − | Yes | − | − | Yes |
| Education dummies | − | − | Yes | − | − | Yes |
| Household composition dummies | − | − | Yes | − | − | Yes |
| Country-district dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 4525 | 4525 | 4525 | 1178 | 1178 | 1178 |
| Adjusted R ² | 0.043 | 0.058 | 0.057 | 0.027 | 0.045 | 0.038 |

Note: The outcome variable is an indicator coded as one if the respondent does not work and states he or she is actively looking for a job. Individual models are estimated by OLS with observations weighted using “federalist” sampling weights supplied with the LiTS data so that all countries have equal weight. Standard errors clustered at the country-district level are in parentheses: ⁺*p* < 0.05, ^{*}*p* < 0.01. Data source: Life in Transition Survey 2010.

In the last three robustness checks, we first look only at households living in apartments, as households of renters and privatizers in this subsample possibly live in more homogeneous housing arrangements.

Then we restrict our sample to individuals who are in the active labor force. Third, we re-estimate our regressions without sample weights. All three checks yield results that are qualitatively and quantitatively

Table 6
Robustness checks: Alternative specifications, Privatizers and Renters.

| Outcome: | Willing to Move (= 1) | | | Unemployed (= 1) | | |
|-----------------------------------|-----------------------|--------------------|--------------------|---------------------------------|--------------------|--------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Outcome: Unemployed > year | | | | − 0.028 (0.022) | − 0.028 (0.023) | − 0.021 (0.024) |
| Privatization centralized | − 0.049 (0.061) | − 0.017 (0.063) | − 0.009 (0.060) | − 0.052 (0.036) | − 0.048 (0.041) | − 0.039 (0.040) |
| High post-priv. transaction costs | − 0.150 (0.118) | − 0.093 (0.101) | − 0.101 (0.131) | − 0.130 (0.097) | − 0.113 (0.083) | − 0.134 (0.089) |
| Baltic states dropped | − 0.003 (0.075) | 0.045 (0.079) | 0.050 (0.098) | − 0.074 (0.042) | − 0.065 (0.043) | − 0.076 (0.045) |
| Only HHs living in apartments | − 0.006 (0.060) | 0.018 (0.057) | 0.027 (0.058) | − 0.056 (0.037) | − 0.054 (0.036) | − 0.038 (0.030) |
| Active in labor force subsample | − 0.051 (0.053) | − 0.017 (0.057) | 0.006 (0.062) | − 0.057 (0.033) | − 0.034 (0.035) | − 0.029 (0.036) |
| Unweighted regressions | − 0.029 (0.039) | − 0.014 (0.042) | − 0.003 (0.046) | − 0.047 ⁺ (0.021) | − 0.034 (0.024) | − 0.024 (0.024) |

Note: Reported estimates are the coefficients on the homeownership variable estimated in a subsample of privatizers and renters. Regressions correspond to models (4), (5), and (6) reported in Tables 4 and 5, respectively. Individual models are estimated by OLS with observations weighted using “federalist” sampling weights supplied with the LiTS data so that all countries have equal weight. Standard errors clustered at the country-district level are in parentheses: ⁺*p* < 0.05, ^{*}*p* < 0.01. Data source: Life in Transition Survey 2010.

similar to our main estimates reported in Tables 4 and 5.

To ascertain potential sensitivity of our results with respect to individual country, we have also re-estimated our preferred regressions from Tables 4 and 5 without individual countries, dropping one at a time. The results are reported in Table A.1 in the Appendix. For mobility, the results are similar to our main estimates. Notably, for unemployment the estimates remarkably stable; all coefficient estimates are negative and are within one standard error from the corresponding estimates in Table 5. These results increase our confidence in the baseline estimates.

In summary, our ambiguous effects of homeownership on mobility and negative effects of homeownership on unemployment are consistent across alternative specifications and robust to country composition.

Finally, because LiTS data also contains information about transition countries in the regions of the Balkans and the former Soviet Union, we have re-estimated our regressions in these samples in order to check whether our results also hold in these countries. The results are reported in Tables A.2 and A.3 in the Appendix (we omit the long specifications for brevity). The estimated effect of homeownership on mobility is now systematically negative, in both FSU and Balkan countries. Notably, the estimates for unemployment are very similar to our findings for CEE countries. Although our knowledge about housing privatization in these countries is more limited (for some details see Broulíková and Montag, 2020), this finding suggests that our results for unemployment may be more general, extending to other post-socialist countries in the Balkans and the Former Soviet Union.

4. The role of unobserved heterogeneity

Our results from transition countries rely on cross-sectional regressions and while housing privatization arguably brings us closer to the experimental setup, one may still question the extent to which we have a true experiment in the data. We see two potential criticisms: (i) Privatizers may possess unobserved characteristics correlated with their productivity and employment prospects. (ii) The respective samples of privatizers and renters are self-selected as the bulk of housing privatization occurred in the 1990s and early 2000s but our data is from 2010. During this period, individuals' housing status may have changed.²²

4.1. The German Socio-Economic panel data

In order to address these criticisms more directly, we use the German Socio-Economic Panel (GSOEP) data that allow us to estimate the effect of homeownership on mobility and employment while controlling for any unobserved productivity characteristics. The GSOEP data is a representative longitudinal survey of German households. We focus on the subsample covering households in the former East Germany during the main privatization period from 1990 until 2000 (see Table 1).

One drawback of the GSOEP data is that it does not contain a question about privatization. We therefore construct an identifier of “quasi-privatizer,” which is defined at the household level and captures households that, during a four-year time window, (i) declared being a renter during the first two years of the time window; (ii) declared being homeowner in the third and fourth year of the time window; (iii) importantly, did not change residence; and (iv) at the same time they did not inherit the housing unit.²³ For individuals from households that

satisfy these criteria, the quasi-privatizer indicator is switched on from the third year of the time window (the year of becoming homeowners) and stays on ever after, in all other cases it is switched off.

Two types of households may fall in this quasi-privatizer category: renters in public housing who bought their dwellings (i.e. privatizers) and renters in private housing who bought the housing from the landlord. Although we have no information about the relative weights of these two types of homeowners, we believe that this quasi-privatizer category is a useful group to study the effects of homeownership. First, it covers privatizers. Second, renters who later buy their housing units from their landlords are probably of a lesser importance.

4.2. Summary statistics

Table 7 presents the summary statistics of the former East German GSOEP sample, broken down by quasi-privatizer status with the unit of observation being an individual in a year. The rate of homeownership is substantially higher among quasi-privatizers, which is not surprising as these are individuals who are homeowners at some point in time, whereas among non-privatizers are also renters who never became homeowners.

In addition, quasi-privatizers seem to be somewhat less mobile, with the probability of changing residence in any given year being about one third compared to non-privatizers. None of the 100 quasi-privatizers moved between German states. Notwithstanding their higher homeownership rates and lower mobility, quasi-privatizers seem to face the same risk of unemployment and its length, as non-privatizers. However, quasi-privatizers tend to be more often active in the labor force, more often self-employed, and seem to work more hours per year.

4.3. Empirical approach

In order to study the effect of homeownership on mobility and unemployment more directly, we estimate fixed effects regressions in the form

$$y_{it} = \beta h_{it} + \phi_i + \tau' \text{year}_t \otimes \text{age}_{it} + e_{it}, \quad (2)$$

where y_{it} is the outcome of interest for an individual i in year t , h_{it} is i 's homeownership status in year t , ϕ_i is her fixed effect that controls for any unobserved time-invariant characteristics that may be correlated with individuals' homeownership status as well as labor market outcomes, year_t is the full set of year effects that remove common shocks, such those due to economic reforms or transfers from the former West Germany to the new federal states following German reunification, age_{it} is the full set of age dummies that control for life-cycle regularities. The age effects are interacted with year effects, allowing for year-specific life-cycle shocks.²⁴ Finally, e_{it} is the residual.

The coefficient of interest is β , it captures the partial correlation between homeownership and the outcome of interest. We report results for two alternative definitions of h_{it} : (i) a simple homeownership indicator that is switched on when the household owns their dwelling and switched off if they are renters and (ii) the quasi-privatizer indicator that is switched on once the household declares they own the dwelling they were previously renting. Once the quasi-privatizer indicator switches on, it remains so ever after. The idea is that we are interested in the full effect of homeownership that may also include the household leaving their dwelling and buying a new one or becoming renters, as found by Sodini et al. (2016).

We note that in regressions with a simple homeownership indicator,

(footnote continued)

to eliminate such errors.

²⁴ The results with age and year effects only, i.e. without the interactions, are qualitatively as well as quantitatively very similar to those without the interactions. We prefer to report the more flexible specifications.

²² See Koar et al. (2019) for an exhaustive study of factors affecting residential mobility.

²³ There are instances in which a household systematically reports being a renter, then for one year reported being a homeowner, and later again reports that they are renters (or vice versa), while remaining in the same residence. We classify such cases as coding errors. The four year window was chosen in order

Table 7

Summary statistics: Former East German sample of the GSOEP data (1990–2000).

| | Non-Privatizers | | Quasi Privatizers | |
|--------------------------------------|-----------------|----------|-------------------|----------|
| | Mean | St. Dev. | Mean | St. Dev. |
| Moved residence (= 1) | 0.058 | 0.234 | 0.017 | 0.128 |
| Moved to another federal state (= 1) | 0.005 | 0.068 | 0 | 0 |
| Unemployed (= 1) | 0.108 | 0.31 | 0.109 | 0.312 |
| Months unemployed | 0.558 | 3.413 | 0.603 | 3.604 |
| Active in labor force (= 1) | 0.701 | 0.458 | 0.798 | 0.402 |
| Yearly hours worked | 1237.609 | 1138.925 | 1475.03 | 1152.813 |
| Self-employed (= 1) | 0.042 | 0.201 | 0.07 | 0.255 |
| Homeowner (= 1) | 0.363 | 0.481 | 0.517 | 0.5 |
| Renter (= 1) | 0.606 | 0.489 | 0.472 | 0.5 |
| Potential privatizer (= 1) | 0 | 0 | 0.5 | 0.5 |
| Year of quasi privatization | – | – | 1995.236 | 2.563 |
| Age | 43.996 | 16.72 | 39.784 | 14.048 |
| Female (= 1) | 0.524 | 0.499 | 0.462 | 0.499 |
| Number of children | 0.66 | 0.903 | 0.927 | 0.987 |
| Years of education | 11.61 | 2.909 | 11.655 | 3.478 |
| Number of observations | 44,163 | | 758 | |
| Number of individuals | 8273 | | 100 | |

Data source: German Socio-Economic Panel 1990–2000.

β may pick up individual-level labor market shocks. However, the estimate of β is unaffected by selection on (fixed over time) unobservables as long as fixed effects are included. In order gauge the role of this unobserved heterogeneity, we also report estimates without fixed effects, which are akin to the standard estimates in cross-sectional data. For instance, if the estimate of β in the model with fixed effects would be larger than in the model without fixed effects, this would suggest that individuals who face lower unemployment risk self-select into homeownership.

4.4. Results

We again begin by looking at the relationship between homeownership and mobility. Rather than the willingness to move, GSOEP data allows us to measure the actual mobility. We therefore look at two mobility measures: a change in the residence and mobility across the 16 German states. The results are reported in Table 8.

The estimates in columns (1) and (2) suggest that homeowners are about six to seven percentage points less likely to change residence in a given year than renters, which is both a substantively and statistically significant effect. Similar patterns are found in columns (5) and (6),

where the outcome is mobility across the German states, although the magnitudes are much smaller reflecting low baseline cross-state mobility rate of 0.005 reported in Table 7.

The pattern of results is similar when we look at quasi-privatizers. In columns (3) and (7), there is an apparent decrease in mobility when quasi-privatizers become owners of their dwelling, but the estimate for within-state mobility is somewhat smaller (in absolute terms) than the corresponding estimates on the homeownership dummy (the difference is statistically significant, using two-sample *t*-test). Once fixed effects are included, the reduction in mobility tends to drop further and is zero (with 95% CI ranging from – 0.002 to 0.002) for cross-state mobility in column (8).

With respect to unemployment, the results reported in Table 9 show a slightly different story to our cross-sectional results for transition countries in the LiTS data. The coefficient on homeownership dummy in column (1) suggests that homeowners face about two percentage points lower unemployment, which is a substantively significant difference when we consider that the sample average is about 11% (see Table 7). This result matches the standard finding in the individual-level studies that homeownership is associated with better labor market outcomes. However, this result entirely disappears once we control for fixed effects, as can be seen in column (2). The difference between the two coefficient estimates is statistically significant (using two-sample *t*-test). This suggests that the negative relationship between homeownership and unemployment is driven by homeowners' characteristics, rather than homeownership itself. We find similar result when unemployment status is replaced with the number of months in unemployment (although the difference between coefficients is not statistically significant, using two-sample *t*-test).

Our key results are in columns (3) and (4). We see that quasi-privatizers tend to be slightly less likely to be unemployed after they become homeowners, but this result is not statistically significant and is substantively small. However, with the 95% CI ranging from – 0.059 to 0.051, moderately-sized positive or negative effects cannot be ruled out. After individual fixed effects are included, the estimate approaches zero (95% CI ranging from – 0.050 to 0.052), suggesting that composition effects play little role. When unemployment status is replaced with the number of months in unemployment, as reported in columns (7) and (8), the estimated effects are again small and statistically not significant, with the 95% CI for the estimate in column (8) ranging from – 0.53 to 0.45. In summary, these estimates do not support the idea that homeownership increases unemployment risk and lengthens unemployment spells.

Table 10 delves further into the relationship between homeownership and labor market activity. One may hypothesize that when homeowners experience unemployment, they may be more likely to drop out of the labor force or become self-employed. This would

Table 8

Homeownership and mobility in the former East Germany: Pooled OLS and Fixed Effects estimates.

| | Moved residence (= 1) | | | | Moved to a different federal state (= 1) | | | |
|----------------------------|-----------------------|---------------------|---------------------|---------------------|--|---------------------|---------------------|---------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Homeowner (= 1) | – 0.074* (0.002) | – 0.057* (0.005) | | | – 0.005* (0.001) | – 0.010* (0.002) | | |
| Quasi-privatized (= 1) | | | – 0.059* (0.005) | – 0.039* (0.011) | | | – 0.005* (0.001) | – 0.0002 (0.001) |
| Individual fixed effects | – | Yes | – | Yes | – | Yes | – | Yes |
| Year effects × Age effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 36,281 | 36,281 | 36,281 | 36,281 | 36,643 | 36,643 | 36,643 | 36,643 |
| Adjusted R ² | 0.052 | 0.091 | 0.029 | 0.089 | 0.006 | 0.205 | 0.005 | 0.204 |

Note: Standard errors clustered at the individual level are in parentheses: **p* < 0.05, ***p* < 0.01. Data source: German Socio-Economic Panel 1990–2000.

Table 9
Homeownership and unemployment in the former East Germany: Pooled OLS and Fixed Effects estimates.

| | Unemployed (= 1) | | | | Number of months in unemployment | | | |
|----------------------------|------------------|---------|---------|---------|----------------------------------|---------|---------|---------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Homeowner (= 1) | − 0.019* | − 0.001 | | | − 0.222* | − 0.082 | | |
| | (0.005) | (0.007) | | | (0.052) | (0.071) | | |
| Quasi-privatized (= 1) | | | − 0.004 | 0.001 | | | − 0.009 | − 0.044 |
| | | | (0.028) | (0.026) | | | (0.334) | (0.250) |
| Individual fixed effects | – | Yes | – | Yes | – | Yes | – | Yes |
| Year effects × Age effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 44,919 | 44,919 | 44,919 | 44,919 | 40,766 | 40,766 | 40,766 | 40,766 |
| Adjusted R ² | 0.059 | 0.265 | 0.058 | 0.265 | 0.319 | 0.551 | 0.318 | 0.551 |

Note: Standard errors clustered at the individual level are in parentheses: [†]*p* < 0.05, **p* < 0.01. Data source: German Socio-Economic Panel 1990–2000.

mitigate the unemployment effects of homeownership. We find the opposite, homeowners as well as quasi-privatizers are similarly likely to be in the active labor force as renters. Examining hours worked, we find in column (5) that homeowners tend to work significantly more hours per year (by 67 h, more than 0.4 of a month-worth of a full-time job). However, the estimated effect drops close to zero once individual fixed effects are included in column (6), the difference between coefficients being statistically significant. Quasi-privatizers seem to increase their hours worked, as reported in columns (7) and (8), but these results are not statistically significant. Finally, the higher self-employment rate of homeowners, reported in column (9), is explained away by composition effects, comparing with the result in column (10), the difference between the two coefficients is statistically significant. The positive and substantively significant effects for quasi-privatizers, reported in columns (11) and (12), are not statistically significant. To summarize, these results do not suggest that homeownership would reduce economic activity.

5. Conclusion

This paper argues that housing privatization in Central and Eastern European countries provides an external policy shock to homeownership that may be used to learn about its effects on individual-level mobility and the risk of unemployment. In particular, we take advantage of the fact that housing privatization in CEE countries took the form of transferring property rights to sitting tenants at substantially discounted rates, sometimes even as giveaways. Furthermore, housing privatization was the result of the fall of the Iron Curtain, an event that could not have been anticipated by individuals. This led to a situation in which (i) upon moving in, renters could not anticipate whether or not they would later become entitled to privatize their home; (ii) the

decision as to which of the dwelling was to be privatized was beyond the control of individual tenants; and (iii) individuals who received an offer for privatization had a high incentive to accept. We exploit the randomization provided by these characteristics of housing privatization in CEE countries, to obtain new estimates of the effects of homeownership on individual mobility and unemployment risks.

Previous research, the nature of housing and financial markets, and some aspects of housing privatization in transition countries suggest that homeowners in these countries faced relatively high transaction costs compared to established market economies, enhancing the potential for adverse labor-market effects of homeownership. While we cannot reject that homeownership limits people's mobility, we find no evidence that homeownership has a detrimental impact on individuals' unemployment risks—the estimated effects are systematically negative (although the 95% confidence intervals contain small positive effects). We perform a number of robustness checks and find these results to be very stable. Our findings are further corroborated when we re-estimate our models in data for the Balkans and the former Soviet Union countries.

Our preferred estimates of the effects of homeownership on unemployment in CEE countries are systematically negative. This is consistent with the existing individual-level studies. Our analysis of former East German sample of the GSOEP data allows us to explicitly control for unobserved heterogeneity. The results indeed suggest that unobserved heterogeneity may be an important factor behind the negative relationship between homeownership and unemployment typically found in the previous literature. Controlling for these factors, we obtain estimates that are close to zero.

We interpret our results as inconsistent with the idea that homeownership increases unemployment. Homeownership is unlikely to be a major force that would generate unemployment through the decreased

Table 10
Homeownership and labor force participation in the former East Germany: Pooled OLS and Fixed Effects estimates.

| | In labor force (= 1) | | | | Yearly hours worked | | | | Self-employed (= 1) | | | |
|----------------------------|----------------------|---------|---------|---------|---------------------|----------|----------|----------|---------------------|---------|---------|---------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Homeowner (= 1) | 0.010 | 0.007 | | | 67.173* | 9.166 | | | 0.036* | 0.007 | | |
| | (0.005) | (0.008) | | | (15.917) | (21.409) | | | (0.005) | (0.006) | | |
| Quasi-privatized (= 1) | | | 0.013 | − 0.003 | | | 105.840 | 29.305 | | | 0.050 | 0.027 |
| | | | (0.032) | (0.026) | | | (98.426) | (72.564) | | | (0.035) | (0.023) |
| Individual fixed effects | – | Yes | – | Yes | – | Yes | – | Yes | – | Yes | – | Yes |
| Year effects × Age effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 44,919 | 44,919 | 44,919 | 44,919 | 44,919 | 44,919 | 44,919 | 44,919 | 44,919 | 44,919 | 44,919 | 44,919 |
| Adjusted R ² | 0.532 | 0.695 | 0.532 | 0.695 | 0.467 | 0.699 | 0.467 | 0.699 | 0.025 | 0.628 | 0.018 | 0.628 |

Note: Standard errors clustered at the individual level are in parentheses: [†]*p* < 0.05, **p* < 0.01. Data source: German Socio-Economic Panel 1990–2000.

mobility of homeowners. However, this finding is limited to the direct channel and thus we cannot rule out possible effects through external channels suggested by [Blanchflower and Oswald \(2013\)](#). We believe that future research studying housing privatization processes in these regions and their labor-market effects may yield further insights about the effects of homeownership on the labor market.

From the policy perspective, our findings suggest that housing privatization, which further increased the already high levels of homeownership in transition economies, probably did not cause the relatively higher unemployment rates found in some of these countries. As a corollary, our results also suggest that policies promoting homeownership have neither detrimental nor beneficial effects on labor markets.

CRedit authorship contribution statement

Hana M. Broulíková: Investigation. **Peter Huber:** Conceptualization, Methodology, Data curation, Writing - original draft. **Josef Montag:** Supervision, Conceptualization, Methodology, Data curation, Writing - original draft. **Petr Sunega:** Writing - review & editing, Investigation.

Appendix A

Table A1

Robustness checks: Dropping individual countries, Privatizers and Renters.

| Outcome: | Willing to Move (= 1) | | | Unemployed (= 1) | | |
|----------------|-----------------------|--------------------|--------------------|--------------------------------|--------------------------------|--------------------------------|
| | (1) | (2) | (3) | (4) | | |
| Czech Republic | −0.050 (0.051) | −0.018 (0.053) | −0.0002 (0.057) | −0.053 (0.030) | −0.045 (0.033) | −0.035 (0.034) |
| Estonia | −0.028 (0.051) | 0.012 (0.052) | 0.021 (0.057) | −0.055 (0.029) | −0.047 (0.032) | −0.038 (0.032) |
| East Germany | −0.024 (0.053) | 0.001 (0.053) | 0.007 (0.053) | −0.050 (0.029) | −0.048 (0.032) | −0.041 (0.032) |
| Hungary | −0.040 (0.048) | −0.010 (0.049) | 0.012 (0.052) | −0.052 (0.027) | −0.048 (0.030) | −0.038 (0.029) |
| Latvia | −0.031 (0.057) | 0.001 (0.058) | 0.019 (0.065) | −0.051 (0.033) | −0.048 (0.035) | −0.035 (0.036) |
| Lithuania | −0.020 (0.052) | 0.007 (0.055) | 0.022 (0.058) | −0.064 ⁺ (0.028) | −0.060 ⁺ (0.030) | −0.067 ⁺ (0.028) |
| Poland | −0.035 (0.049) | −0.0004 (0.051) | 0.011 (0.055) | −0.053 (0.028) | −0.049 (0.031) | −0.039 (0.030) |
| Romania | 0.013 (0.044) | 0.024 (0.047) | 0.037 (0.051) | −0.034 (0.019) | −0.021 (0.023) | −0.004 (0.023) |
| Slovakia | −0.039 (0.049) | −0.013 (0.049) | 0.0004 (0.053) | −0.049 (0.027) | −0.040 (0.030) | −0.029 (0.028) |
| Slovenia | −0.075 (0.043) | −0.030 (0.041) | −0.019 (0.047) | −0.059 (0.030) | −0.051 (0.032) | −0.038 (0.033) |

Note: Reported estimates are the coefficients on the homeownership variable estimated in a subsample of privatizers and renters. Regressions correspond to models (3), (4), (7), and (8) reported in [Tables 4](#) and [5](#). Individual models are estimated by OLS with observations weighted using “federalist” sampling weights supplied with the LiTS data so that all countries have equal weight. Standard errors clustered at the country-district level are in parentheses: ⁺*p* < 0.05, **p* < 0.01. Data source: Life in Transition Survey 2010.

Table A2

Homeownership and mobility: The Balkans and the former Soviet Union.

| Region: | The Balkans | | | | Former Soviet Union (except Baltic countries) | | | |
|-----------------|--------------------|--------------------------------|-------------------------|-------------------|---|-------------------|-------------------------|-------------------|
| | Full Sample | | Privatizers and Renters | | Full Sample | | Privatizers and Renters | |
| Sample: | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Homeowner (= 1) | −0.083* (0.032) | −0.069 ⁺ (0.031) | −0.065 (0.068) | −0.116 (0.060) | −0.048 (0.039) | −0.050 (0.037) | −0.069 (0.050) | −0.059 (0.043) |
| Age | 0.027 (0.015) | | 0.002 (0.033) | | 0.005 (0.011) | | 0.011 (0.019) | |

(continued on next page)

Table A2 (continued)

| Region: | The Balkans | | | | Former Soviet Union (except Baltic countries) | | | |
|-------------------------------|---------------------------------|--------------------|-------------------------|-------------------------------|---|---------------------|-------------------------|--------------------|
| Sample: | Full Sample | | Privatizers and Renters | | Full Sample | | Privatizers and Renters | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Age ² /100 | − 0.036 ⁺ (0.015) | | − 0.013 (0.032) | | − 0.015 (0.011) | | − 0.020 (0.018) | |
| Willing to take risks (1–10) | | 0.025* (0.004) | | 0.021* (0.008) | | 0.023* (0.003) | | 0.025* (0.005) |
| Female (= 1) | | − 0.045 (0.054) | | − 0.068 (0.100) | | − 0.063 (0.041) | | − 0.105 (0.074) |
| Married (= 1) | | 0.030 (0.034) | | 0.285 ⁺ (0.133) | | − 0.025 (0.034) | | − 0.043 (0.052) |
| Foreigner (= 1) | | 0.011 (0.053) | | 0.163 (0.106) | | 0.031 (0.039) | | − 0.067 (0.062) |
| Communist before 1989 (= 1) | | 0.099* (0.037) | | 0.142 (0.086) | | 0.014 (0.026) | | 0.059 (0.044) |
| Parents were communists (= 1) | | 0.050 (0.026) | | − 0.058 (0.057) | | 0.019 (0.019) | | − 0.025 (0.037) |
| Married × Female | | − 0.075 (0.039) | | − 0.130 (0.180) | | 0.013 (0.041) | | 0.088 (0.063) |
| Number of children × Female | | − 0.004 (0.016) | | 0.054 (0.072) | | − 0.037* (0.012) | | − 0.023 (0.025) |
| Number of adults × Female | | 0.019 (0.020) | | 0.053 (0.045) | | 0.007 (0.013) | | 0.009 (0.021) |
| Age dummies | − | Yes | − | Yes | − | Yes | − | Yes |
| Education dummies | − | Yes | − | Yes | − | Yes | − | Yes |
| Household composition dummies | − | Yes | − | Yes | − | Yes | − | Yes |
| Country-district dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 3497 | 3497 | 593 | 593 | 5151 | 5151 | 1963 | 1963 |
| Adjusted R ² | 0.081 | 0.122 | 0.128 | 0.215 | 0.096 | 0.123 | 0.116 | 0.152 |

Note: The outcome variable is an indicator coded as one if the respondent does not work and states he or she is actively looking for a job. Balkan countries in the data: Albania, Bosnia and Herzegovina, Croatia, Kosovo, Macedonia, Montenegro, Serbia. FSU countries in the data: Armenia, Azerbaijan, Belarus, Georgia, Kazakhstan, Kyrgyzstan, Moldova, Russia, Tajikistan, Ukraine, and Uzbekistan. Individual models are estimated by OLS with observations weighted using “federalist” sampling weights supplied with the LiTS data so that all countries have equal weight. Standard errors clustered at the country-district level are in parentheses: ⁺*p* < 0.05, ^{*}*p* < 0.01. Data source: Life in Transition Survey 2010.

Table A3

Homeownership and unemployment: The Balkans and the former Soviet Union.

| Region: | The Balkans | | | | Former Soviet Union (except Baltic countries) | | | |
|-------------------------------|---------------------------------|---------------------|-------------------------|---------------------------------|---|-------------------------------|-------------------------|--------------------|
| Sample: | Full Sample | | Privatizers and Renters | | Full Sample | | Privatizers and Renters | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Homeowner (= 1) | − 0.043 (0.024) | − 0.034 (0.027) | − 0.042 (0.037) | − 0.031 (0.046) | − 0.047 (0.027) | − 0.040 (0.028) | − 0.043 (0.036) | − 0.043 (0.036) |
| Age | 0.018 (0.011) | | 0.057* (0.019) | | 0.013 (0.008) | | 0.024 (0.016) | |
| Age ² /100 | − 0.023 ⁺ (0.010) | | − 0.059* (0.019) | | − 0.017 ⁺ (0.008) | | − 0.027 (0.015) | |
| Willing to take risks (1–10) | | 0.001 (0.003) | | − 0.001 (0.009) | | 0.004 (0.002) | | 0.005 (0.003) |
| Female (= 1) | | − 0.052 (0.042) | | − 0.279 ⁺ (0.129) | | − 0.043 (0.031) | | 0.021 (0.070) |
| Married (= 1) | | − 0.099* (0.036) | | − 0.039 (0.105) | | − 0.020 (0.030) | | 0.006 (0.063) |
| Foreigner (= 1) | | 0.013 (0.024) | | 0.043 (0.063) | | − 0.012 (0.026) | | 0.007 (0.044) |
| Communist before 1989 (= 1) | | 0.016 (0.023) | | 0.029 (0.089) | | − 0.009 (0.020) | | − 0.002 (0.037) |
| Parents were communists (= 1) | | 0.021 (0.018) | | − 0.003 (0.049) | | 0.029 ⁺ (0.012) | | 0.027 (0.023) |
| Married × Female | | 0.132* (0.044) | | 0.177 (0.115) | | 0.022 (0.028) | | 0.013 (0.056) |
| Number of children × Female | | − 0.022 (0.016) | | 0.078 (0.062) | | − 0.010 (0.010) | | − 0.032 (0.021) |
| Number of adults × Female | | − 0.019 (0.013) | | 0.047 (0.045) | | 0.002 (0.010) | | − 0.012 (0.025) |
| Age dummies | − | Yes | − | Yes | − | Yes | − | Yes |
| Education dummies | − | Yes | − | Yes | − | Yes | − | Yes |

(continued on next page)

Table A3 (continued)

| Region: | The Balkans | | | | Former Soviet Union (except Baltic countries) | | | |
|-------------------------------|-------------|-------|-------------------------|-------|---|-------|-------------------------|-------|
| | Full Sample | | Privatizers and Renters | | Full Sample | | Privatizers and Renters | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Household composition dummies | – | Yes | – | Yes | – | Yes | – | Yes |
| Country-district dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 3497 | 3497 | 593 | 593 | 5151 | 5151 | 1963 | 1963 |
| Adjusted R ² | 0.047 | 0.056 | 0.017 | 0.100 | 0.135 | 0.141 | 0.156 | 0.186 |

Note: The outcome variable is an indicator coded as one if the respondent does not work and states he or she is actively looking for a job. Balkan countries in the data: Albania, Bosnia and Herzegovina, Croatia, Kosovo, Macedonia, Montenegro, Serbia. FSU countries in the data: Armenia, Azerbaijan, Belarus, Georgia, Kazakhstan, Kyrgyzstan, Moldova, Russia, Tajikistan, Ukraine, and Uzbekistan. Individual models are estimated by OLS with observations weighted using “federalist” sampling weights supplied with the LiTS data so that all countries have equal weight. Standard errors clustered at the country-district level are in parentheses: * $p < 0.05$, ** $p < 0.01$. Data source: Life in Transition Survey 2010.

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