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Homeownership, Mobility, and Unemployment: Evidence from Housing Privatization*

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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 documents that the privatization of public housing in Central and Eastern Europe after the fall of the Iron Curtain resulted in a quasi-experimental assignment of homeownership to individual households. This facilitates a new test of the effects of homeownership on mobility and unemployment. We find only weak evidence that homeowners are less willing to move and no evidence of higher unemployment risks relative to renters.

JEL classification: J61, J64, P14, P26.

Keywords: Homeownership, housing privatization, mobility, 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

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2005, for overviews). In a seminal paper, Andrew 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' search effectiveness 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 documenting how housing privatization in the transition countries of Central and Eastern Europe (CEE) created a quasi-experiment in homeownership and by using this quasi-experimental variation to ascertain the causal 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, possibly 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 tradeoff 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; Quigley 2002; Van Ommeren 2008; Winkler 2010), mortgage lock-in effects (Quigley 1987), lock-in effects related to transaction and capital gains taxes (Best and Kleven 2015; Lundborg and Skedinger 1998, 1999; O'Sullivan, Sexton, and Sheffrin 1995; Van Ommeren and Van

[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, Gibas, Boumová, Hájek, and Sunega (2017).

Leuvensteijn 2005), homeowners' higher investments in local social capital (DiPasquale and Glaeser 1999), loss aversion (Engelhardt 2003), as well as mobility expectations and preferences (Lux and Sunega 2012; Rohe and Stewart 1996).

However, empirical evidence concerning the effects of homeownership on unemployment remains mixed (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, Heylen, and Smolders 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, Ma, and Phimister 2008; Coulson and Fisher 2002, 2009; Flatau, Forbes, and Hendershott 2003; Munch, Rosholm, and Svarer 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, Rosholm, and Svarer 2006, 2008; Van Leuvensteijn and Koning 2004), regional price-to-rent ratios at the time of buying a house (Baert, Heylen, and Isebaert 2014), tax deductions of mortgage interest and same-sex siblings (Coulson and Fisher 2009), inheritance of money in young years (Gardner, Pierre, and Oswald 2001), dummies for US states (Green and Hendershott 2001), or homeownership rate in subjects' region of birth and her parents' homeownership status (Munch, Rosholm, and Svarer 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 use 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, 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, Van Nieuwerburg, Vestman, and von Lilienfeld-Toal (2016), who exploit privatization of social housing units in Sweden 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, they do not report results for unemployment. An important aspect in which our paper also differs from Sodini et al. (2016) is that our results are valid for a more representative sample of population. This is because the experiment we study, i.e. housing privatization in Central and Eastern Europe, was not a social housing program, but rather a bulk transfer of homeownership to renters. Our results therefore address more general questions about the role of homeownership in the labor market.

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 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 arguably resulted in an as-good-as-random assignment of homeownership to individuals. This facilitates a new test of the effects of homeownership on individuals' labor market behavior and outcomes.

Our second contribution is that, using housing privatization as a quasi-experimental source of variation in homeownership, we provide new estimates of the causal effects of homeownership on mobility and unemployment. To this end, we use data on ten CEE countries and four Western European "comparator" 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

[2]The CEE countries are: the Czech Republic, the former East Germany, Estonia, Hungary, Latvia, Lithuania, Poland, Romania, Slovakia, and Slovenia. The comparator countries are: Great Britain, Italy, Sweden, and the former West Germany.

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 data is from 2010, by which time 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, Brzeski, and Hamilton 2006; Stephens, Lux, and Sunega 2015). 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.

Our results are summarized in three points. (i) We find only weak evidence that homeowners are less willing to move. (ii) We do not find any evidence that homeownership increases unemployment risks. (iii) Our results suggest that the negative correlation between homeownership and unemployment, usually found in the micro literature, is driven by unobserved heterogeneity.

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 the central specification in the

empirical part of this paper, that is

$$y_i = \beta_1 h_i + \boldsymbol{\beta_2'} \boldsymbol{x_i} + \boldsymbol{\gamma_d} + e_i, \tag{1}$$

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, $\boldsymbol{x_i}$ is a vector of control variables, $\boldsymbol{\gamma_d}$ 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 $\boldsymbol{x_i}$ and $\boldsymbol{\gamma_d}$. 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 home 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 homes 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, approximates such an experiment. This is because it effectively transformed a significant, and arguably exogenously selected, subpopulation of renters into homeowners. Broulíková (2017) provides a detailed account of the housing privatization processes both in transition countries and in Western Europe, and surveys 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

[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. · About half of public housing stock was privatized by 2002. · Prior to privatization, 38% of the rental units were public, 18% were housing cooperatives.	· Sitting tenants. · Housing cooperatives.	· Centrally given Right-to-Buy only for existing coop housing. · Municipalities decided on the privatization scale and terms of most public dwellings.	· Various discounts from the market price usually granted by municipalities.	· Later, coops could mutate into owners' associations, with the owners gaining full property rights to their unit.
East Germany (former)	· Since 1993, culminating between 1993 and 1999. · Late 1990s and 2000s privatization to institutional investors. · Prior to privatization, 25% of homeowners.	· Sitting tenants and members of cooperatives. · Institutional investors.	· Municipalities were centrally ordered to sell at least 15% of public rental housing and coops. · Local-level decision for the remainder of the public housing stock.	· During the first wave, the sale prices were far below market prices. · Federal sales to institutional investors for extremely low prices.	· Initially higher involvement of tenants and coop members than in the former West Germany. · Afterwards the approach to privatization similar to the former West Germany, i.e. privatization to institutional investors.
Estonia	· 1993–2001. · About 85-90% of the public housing stock was privatized. · Prior to privatization, 60% of the rental units were public.	· Sitting tenants.	· Central government. · 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.	· Public capital vouchers (EVPs) distributed according to employment length. · Price set relative to the price of a "standard" dwelling in a prefabricated building · Other discounts applicable.	· 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. · EVPs were tradable. · Most privatizers could buy their units just for their EVPs with no additional payment.
Hungary	· Since 1983. · About 80% of the public stock was privatized by 2003. · Prior to privatization, 23% of the rental units were public.	· Sitting tenants.	· Centrally granted Right-to-Buy for sitting tenants.	· 10–15% of the market price. · No financial assistance by the government.	· 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.
Latvia	· Since 1995, peak around the year 2000. · About 55% of the public housing stock was privatized until 2000. · Prior to privatization, almost 70% of the rental units were public.	· Sitting tenants.	· The Central Housing Privatization Commission was responsible for the privatization process. · Terms (i.e. prices) specified centrally. · Local governments helped with processing the privatization.	· 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. · Vouchers were tradable.
Lithuania	· 1991–1995. · By July 1995 94% of possible flat privatization was accomplished. · Fast and extensive privatization even in comparison with Estonia and Latvia.	· Sitting tenants.	· 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.	· Privatization vouchers (ICs) distributed according to the age of the recipient. · Price set according to the construction characteristics, location etc.	· 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.
Poland	· Since 1994. · Majority took place before 2002. · Prior to privatization, 35% of the rental units were public, 25% were housing coops.	· Sitting tenants. · Housing cooperatives.	· Municipalities decided the privatization scale and terms of most public dwellings. · Right-to-Buy only for tenants in cooperative housing.	· Discount of up to 95% of the market value.	· Majority privatized to tenants or coop members. · Other investors could privatize under centrally specified conditions, but municipalities chose the dwellings to be privatized.
Romania	· 1990s. · Public housing stock almost completely privatized by 1999. · Prior to privatization, 75% of homeowners.	· Sitting tenants.	· Entitled privatizers as well as contract conditions specified centrally.	· 'Symbolic price' depending on the construction year. · Public financial assistance: 25-year loan with 4% interest rate.	· Privatization processed by specialized agencies. · Dwelling cannot be resold before the mortgage has been repaid.
Slovakia	· 1993–2008. · Prior to privatization, 25% of rental units were public, 20% were housing cooperatives.	· Sitting tenants. · Housing cooperatives.	· Entitled privatizers as well as contract conditions specified centrally. · Municipalities had to privatize a dwelling within 2 years if approved by over a half of the tenants.	· Price derived from a comparable unit depending on the construction year. · Discounts between 30 and 80%.	· Free-of-charge transfer to the full ownership of cooperative members.
Slovenia	· 1991–1993. · Prior to privatization, 33% of the rental units were public.	· Sitting tenants.	· Entitled privatizers as well as contract conditions specified centrally.	· Discount over 30%. · Discount increased to 60%, if payment occurred within 60 days.	· Centrally specified conditions for selling restituted property to the sitting tenants.

Note: Table based on Broulíková (2017), available online at https://ssrn.com/abstract=2899928. Information sources for individual countries: Czech Republic: Lux (2003); Struyk (1996); Grabmüllerová (2005). Estonia: Kask, Klettenberg, and Lembit (2005); Kursis (1999); Lux (2003); Struyk (1996). Hungary: Hegedüs et al. (1996); Struyk (1996); Tosics (1987). Latvia: Kursis (1999); Osa (2005); Tsenkova and Turner (2004). Lithuania: Housing, Urbanization, and Development Fund (2005); Jurgatyte (2002); Kursis (1999); Milstead and Miles (2011). Poland: Lis and Zwierzchlewski (2015); Lux (2003); Skiba (2005); Struyk (1996); Tibaijuka (2009); Zawislak (2002). Romania: Dübel et al. (2006); Lux (2003); Palacin and Shelburne (2005). Slovakia: Lux (2003); Struyk (1996); Zapletalova, Antalikova, and Smatanova (2003). Slovenia: Mandic and Clapham (1996); Sendi (1995); Struyk (1996).

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 percent in Hungary, Slovakia, and Romania, to about one third in the Czech Republic, Poland, and Slovenia, 60 percent in Estonia, and 70 percent 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 percent. 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 more representative group of residents in contrast to Sodini et al. (2016), who focus on social housing privatization and thus low-income groups.[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 homes (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.

[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 percent in 1961 and almost 37 percent 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 percent in the former Czechoslovakia and 25 percent in Poland (see first column of Table 1 and Table 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.

Third, although those entitled to privatize their homes 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 percent in Slovakia and from 30 to 60 percent in Slovenia. Hungarian and Polish discounts usually exceeded 80 percent, the latter sometimes reaching 95 percent. 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 tenants (Hegedüs, Tosics, and Mayo 1996; Lux, Cirman, and Sunega 2017).

2.3 Housing Privatization as a Quasi-Experiment

Based on this evidence, Figure 1 presents a stylized time-line of potential homeownership status assignment in CEE countries. During communism, people would naturally self-select to be homeowners or renters.[7] While at this point the assignment of homeownership status

[7]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.

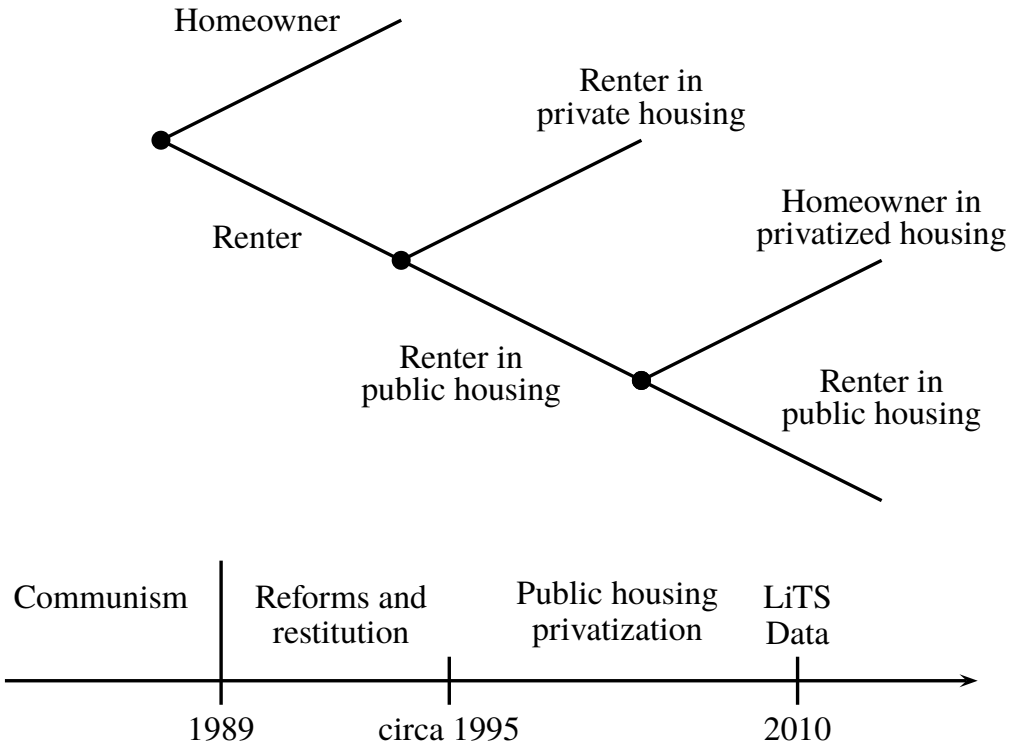

Figure 1: Timeline of potential homeownership status of an individual tenant in Central and Eastern Europe. The initial sorting was an individual's choice; however, it could not have been influenced by the unexpected fall of communism and ensuing reforms that included restitution and the privatization 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.

was decision-based, it was arguably independent of future developments related to the fall of the communist regime in 1989, as those could not have been foreseen.[8]

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

[8]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.

public housing, some would later be able to privatize, while the others would remain renters. This last step of the privatization process provides the randomization 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.

This wealth effect, however, should work 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, Shimer, and Wright 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 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 homes.

However, this caveat is likely to be of a lesser concern in data from CEE countries, relative to established market economies, because mortgage markets in these countries only started to develop in the 2000s. 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).[9]

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.[10] The tenants would be members of these coops. Although it was possible to sell the coop membership, provided the other members agreed by a vote, banks in the Czech Republic would not provide mortgages for coop-owned dwellings. As a result, individuals faced increased transaction costs if they were to buy 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 Zwierzchlewski 2015; Lux 2003). In Poland, coop members were given the right to transform their memberships into full ownership starting from 2001 (Zawislak 2002).

[9]See also "Key figures 2012," European Mortgage Federation, Online, at http://www.hypo.org/Content/default.asp?PageID=414 (last accessed on January 14, 2017) and Bloze (2009); Dübel, Brzeski, and Hamilton (2006); and Sunega and Lux (2007).

[10]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.

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 (2017), using the 2006 and 2010 LiTS data, finds that in eight out of the ten CEE countries we focus on the shares of households living in privatized units were either similar or higher in 2010 compared to 2006.[11]

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, the survey asks the respondents whether they became homeowners through privatization.[12]

The survey was conducted in 28 transition countries in Central and Eastern Europe and the former Soviet Union, plus five Western European comparator 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.

[11]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 percent. However, this change is likely an artifact of misinterpretation of the ownership question in 2006 by some respondents.

[12]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 November 15, 2017). 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.

Subsequently, 20 households were randomly chosen within each PSU for interviews. For each country, the database therefore contains a sample of about 1,000 or 1,500 households.

Interviewers had to visit each selected household at least three times before replacing it with another one. In 79 percent 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 percent 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.[13]

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. In order to facilitate comparison with the existing literature, we also report results for four Western European comparator countries, Great Britain, Italy, Sweden, and the former West Germany.[14] Bulgaria and France are dropped from the data as housing privatization in these countries was too limited and the LiTS data does not contain any homeowners who privatized their dwellings. All LiTS respondents are at least 18 years old, and we restrict the sample to respondents who are at most 65 years old and drop households for which the homeownership variable is missing (2.2 percent of the observations).[15] Our analysis dataset contains 7,544 individuals from the ten CEE countries and 3,333 from individuals

[13]For more details about the surveying process and outcomes see http://www.ebrd.com/news/publications/special-reports/life-in-transition-survey-ii.html (last accessed on November 15, 2017).

[14]We use the regional identifiers in the data to split Germany into East and West. In this separation Berlin was assigned to West Germany because the population of West Berlin in 1990 was almost twice the size of East Berlin. We note, and report below, that our results were unaffected when we dropped Berlin as a robustness check.

[15]We do not restrict the data to individuals in the active labor force, since labor force status is not exogenously assigned. As reported below, results are unaffected if the sample is restricted to individuals active in the labor force.

the Western European comparator 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.[16] (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.

Table 2 summarizes the homeownership structure in the CEE countries as well as the comparator countries in the LiTS data. 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 than any of the four comparator countries. Despite this, the shares of mortgage-financed housing in CEE countries, except for Hungary, are always substantially lower than in the Western European comparator 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 limited rental and housing markets during the communist era when people often built their homes by themselves, either as private individuals or members of cooperatives, as this was the most accessible way of becoming a

[16]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 2008).

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.

homeowner (Stephens, Lux, and Sunega 2015). A second probable factor is represented by the relatively underdeveloped financial markets, particularly mortgage markets, as of 2010.

Table 3 presents the descriptive statistics of individuals' willingness to move and unemployment, as well as a number of demographic and household characteristics reported in the survey, split by the geographic regions and broken down by homeownership status. In general, people in CEE countries are less willing to move than individuals living in Western Europe. The data also confirms that renters are significantly more willing to move relative to homeowners—privatizers as well as those that did not privatize. Note also that renters' willingness to move is very similar across the two geographic regions. The difference in the willingness to move between CEE and comparator countries is therefore driven by homeowners. These patterns are consistent with the previous research on homeownership and mobility and with the high levels of homeownership in CEE countries.

At the same time, the share of unemployed is almost the same across the two geographic regions. In the CEE countries, the share of unemployed is the same among privatizers and

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

	Central and Eastern Europe						Western Europe					
	Means				*t*-tests (*p*-values)		Means				*t*-tests (*p*-values)	
	Full Sample	Renters	Privatizers	Homeowners Non-Privatizers	Privatizers v. Renters	Homeowners Non-Privatizers v. Renters	Full Sample	Renters	Privatizers	Homeowners Non-Privatizers	Privatizers v. Renters	Homeowners Non-Privatizers v. Renters
Willing to move for job (=1)	0.37	0.53	0.37	0.33	< 0.01	< 0.01	0.46	0.52	0.40	0.42	< 0.01	< 0.01
Unemployed (=1)	0.10	0.12	0.12	0.10	0.76	0.01	0.09	0.14	0.08	0.05	< 0.01	< 0.01
Female (=1)	0.58	0.57	0.65	0.57			0.57	0.57	0.54	0.58		
Age	42.16	35.89	45.22	42.99			43.67	40.00	46.05	45.91		
Married (=1)	0.56	0.35	0.52	0.61			0.52	0.34	0.60	0.64		
Household size	2.69	2.24	2.55	2.82			2.46	2.21	2.53	2.63		
Number of children	0.53	0.51	0.41	0.56			0.51	0.49	0.38	0.56		
Years of education	13.21	13.25	13.42	13.16			13.48	13.12	13.62	13.72		
Willing to take risks (1–10)	4.79	5.17	4.64	4.74			4.99	5.07	4.96	4.93		
Communist before 1989 (=1)	0.04	0.02	0.06	0.05			0.01	0.01	0.01	0.01		
Parents were communists (=1)	0.11	0.10	0.11	0.11			0.02	0.03	0.03	0.01		
Foreign language speaker (=1)	0.05	0.05	0.13	0.04			0.01	0.01	0.02	0.01		
Observations	7544	1200	1017	5327			3333	1274	413	1646		

Data source: Life in Transition Survey 2010.

renters, while homeowners who did not obtain their dwelling in privatization exhibit about a two percentage point lower unemployment rate. In Western European comparator countries, the homeowner non-privatizers have the lowest unemployment risk by far, followed by privatizers, while renters face the highest unemployment risk.

Taken at face value, these patterns are consistent with empirical results found in many of the previous micro studies, while being rather inconsistent with the idea that homeownership increases unemployment. However, the summary statistics in Table 3 also suggest that these subpopulations are not directly comparable: In the CEE countries, the share of females among privatizers is higher than the share of females among renters, but gender shares are very similar among homeowner non-privatizers and renters. Privatizers as well as other homeowners tend to be older, more often married, and have larger households than renters in both geographic regions. The age difference between renters and privatizers amounts to almost ten years in the CEE countries. In both geographic regions, the number of children living in a household is higher among homeowners who did not privatize than among renters. However, there are fewer children in privatizers' households compared to renters, possibly due to the age differences. Privatizers also tend to be more educated than renters in both geographic regions. Renters in the CEE countries are more willing to take risks than privatizers and other homeowners. Perhaps not surprisingly, as they are older, privatizers in CEE countries are significantly more likely to have been communist party members than renters. Finally, individuals speaking a foreign language at home seem to have 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.

3.3 Homeownership and Mobility

Table 4 reports alternative specifications of regression (1) for respondents' willingness to move, estimated separately for CEE and Western European comparator countries. 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 quasi-experimental assignment of homeownership 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 a short and long specification of equation (1) by OLS.[17] The short specification controls 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. At the same time, older respondents are obviously more likely to have had the opportunity to privatize their home. 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. 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. 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.[18]

[17] 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).

[18] 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

Region:	Central and Eastern Europe				Western Europe			
Sample:	Full Sample		Privatizers and Renters		Full Sample		Privatizers and Renters	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Homeowner (=1)	−0.098*	−0.080*	−0.057	−0.034	−0.072*	−0.077*	−0.073*	−0.090*
	(0.017)	(0.018)	(0.030)	(0.033)	(0.018)	(0.019)	(0.027)	(0.029)
Age	−0.022*	−0.011*	−0.035*	−0.025*	−0.024*	−0.017*	−0.026*	−0.019+
	(0.003)	(0.003)	(0.007)	(0.007)	(0.005)	(0.004)	(0.008)	(0.007)
$Age^2/100$	0.014*	0.003	0.027*	0.018+	0.018*	0.012+	0.022+	0.016
	(0.004)	(0.003)	(0.008)	(0.008)	(0.006)	(0.005)	(0.010)	(0.009)
Willing to take risks (1–10)		0.040*		0.052*		0.044*		0.042*
		(0.003)		(0.005)		(0.005)		(0.006)
Years of education		0.009*		0.016+		0.032*		0.033*
		(0.003)		(0.008)		(0.006)		(0.008)
Female (=1)		−0.053		−0.155*		−0.0005		−0.026
		(0.033)		(0.052)		(0.037)		(0.061)
Married (=1)		−0.077*		−0.083+		−0.059+		−0.063
		(0.022)		(0.040)		(0.029)		(0.040)
Number of children		−0.032*		0.019		−0.028		−0.037
		(0.010)		(0.027)		(0.020)		(0.025)
Number of adults		−0.007		−0.044		0.029+		0.042
		(0.010)		(0.024)		(0.015)		(0.023)
Foreigner (=1)		−0.044		−0.069		−0.018		0.008
		(0.035)		(0.051)		(0.075)		(0.138)
Communist before 1989 (=1)		0.010		−0.084		0.062		0.183
		(0.032)		(0.061)		(0.100)		(0.117)
Parents were communists (=1)		0.022		0.011		−0.084		−0.197*
		(0.018)		(0.038)		(0.061)		(0.071)
Married × Female		−0.018		0.009		−0.034		−0.031
		(0.023)		(0.048)		(0.040)		(0.052)
Number of children × Female		−0.004		−0.075+		−0.005		0.009
		(0.011)		(0.035)		(0.023)		(0.029)
Number of adults × Female		0.016		0.061+		−0.025		−0.020
		(0.012)		(0.026)		(0.017)		(0.032)
Country-district dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,544	7,544	2,217	2,217	3,333	3,333	1,687	1,687
Adjusted R^2	0.135	0.187	0.157	0.237	0.099	0.170	0.109	0.179

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.

Our baseline full-sample estimates in columns (1), (2), (5), and (6) of Table 4 indicate that homeowners in the CEE countries and the Western European comparator 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 (3) and (4), where we compare renters and privatizers, we see that the willingness to move of privatizers in the CEE countries is still estimated to be negative. However, the coefficients are about one half smaller than the corresponding full-sample estimates and are not statistically significant. Homeowners who privatized their home in the Western European countries also have lower willingness to move relative to renters, and the coefficients in columns (7) and (8) are comparable to the corresponding full-sample estimates. Noting that we put more weight on the estimates for CEE countries, since this is where privatization provides a quasi-experiment in homeownership, we interpret these results as only weakly suggesting that homeownership makes people less mobile.

3.4 Homeownership and Unemployment

Our key results for unemployment are reported in Table 5. When comparing the full-sample estimates in columns (1) and (2) with columns (5) and (6), the results for CEE countries and Western European comparator countries differ. For CEE countries, the estimates suggest that homeowners face essentially the same unemployment risk as renters. For Western European comparator countries, we obtain the standard result that homeowners are less likely to be unemployed. Comparing with the sample means in Table 3, estimates for comparator countries are both statistically and substantively significant,

Table 5: Homeownership and unemployment

Region:	Central and Eastern Europe				Western Europe			
Sample:	Full Sample		Privatizers and Renters		Full Sample		Privatizers and Renters	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Homeowner (=1)	0.003	0.011	0.004	0.003	−0.061*	−0.052*	−0.021	−0.017
	(0.013)	(0.013)	(0.018)	(0.019)	(0.016)	(0.016)	(0.024)	(0.024)
Age	0.001	0.006+	0.001	0.003	−0.001	0.0004	−0.0002	0.001
	(0.002)	(0.002)	(0.004)	(0.005)	(0.003)	(0.003)	(0.004)	(0.004)
$Age^2/100$	−0.004	−0.009*	−0.004	−0.006	−0.001	−0.003	−0.002	−0.003
	(0.002)	(0.003)	(0.005)	(0.006)	(0.003)	(0.003)	(0.004)	(0.004)
Willing to take risks (1–10)		−0.0004		0.001		0.001		−0.002
		(0.002)		(0.003)		(0.002)		(0.003)
Years of education		−0.014*		−0.011+		−0.006+		−0.007
		(0.002)		(0.005)		(0.003)		(0.005)
Female (=1)		−0.031		−0.013		−0.049		−0.042
		(0.019)		(0.047)		(0.030)		(0.045)
Married (=1)		−0.071*		−0.069+		−0.066*		−0.095*
		(0.015)		(0.027)		(0.018)		(0.025)
Number of children		−0.004		−0.020		0.003		0.018
		(0.008)		(0.019)		(0.010)		(0.015)
Number of adults		0.001		0.008		−0.001		0.002
		(0.006)		(0.017)		(0.011)		(0.017)
Foreigner (=1)		0.018		0.030		0.019		0.160
		(0.026)		(0.051)		(0.056)		(0.100)
Communist before 1989 (=1)		−0.029		−0.107		0.031		−0.027
		(0.020)		(0.061)		(0.056)		(0.063)
Parents were communists (=1)		0.012		−0.045		0.036		0.065
		(0.014)		(0.026)		(0.043)		(0.060)
Married × Female		0.023		0.021		0.058+		0.096+
		(0.018)		(0.033)		(0.029)		(0.041)
Number of children × Female		0.010		0.050+		0.005		−0.001
		(0.010)		(0.024)		(0.012)		(0.019)
Number of adults × Female		0.006		−0.003		0.007		0.001
		(0.008)		(0.021)		(0.017)		(0.027)
Country-district dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,544	7,544	2,217	2,217	3,333	3,333	1,687	1,687
Adjusted R^2	0.041	0.054	0.060	0.074	0.048	0.052	0.047	0.054

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.

suggesting that homeowners in these countries face unemployment risk that is about half the size of the risk faced by renters.

Shifting attention to our preferred specifications comparing privatizers and renters in CEE countries, reported in columns (3) and (4), we obtain virtually identical coefficients as for the corresponding full-sample estimates in columns (1) and (2), i.e. a zero effect. For the Western European comparator countries, the results in columns (7) and (8) suggest that privatizers appear to have lower unemployment than renters. However, the estimates are about one third of those in the corresponding full-sample models in columns (5) and (6) and are not statistically significant.

In summary, our main results do not reveal that homeownership causes higher risks of unemployment amongst homeowners. Additionally, the absence of a correlation between homeownership and unemployment in CEE countries, particularly when looking at our preferred estimates for renters and privatizers, suggests that the negative correlation found in previous literature may be due to factors driving selection into homeownership that are related to individuals' job market outcomes, but are unobservable for the econometrician.

3.5 Robustness Checks

To check the robustness of our results for renters and privatizers, we first examine whether they may be driven, or excessively influenced, by an individual country. We therefore re-estimate our preferred regressions from Tables 4 and 5 dropping individual countries, one at a time. The results, reported in Table A1 in the Appendix, are remarkably stable, particularly for the CEE countries. The coefficients are always within one standard error from the corresponding estimates in Tables 4 and 5. Note also that the estimates on the willingness to move, although almost never statistically significant in the CEE sample, are always negative. These results increase our confidence in the baseline estimates.

Table 6 presents alternative specifications of our preferred estimates 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

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

Region:	Central and Eastern Europe				Western Europe			
Outcome:	Willing to Move (=1)		Unemployed (=1)		Willing to Move (=1)		Unemployed (=1)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome: Unemployed > year			0.008	0.006			−0.016	−0.010
			(0.015)	(0.016)			(0.019)	(0.020)
Only subjects 40-plus years old	−0.010	0.013	−0.039	−0.035	−0.117*	−0.145*	−0.067*	−0.066*
	(0.046)	(0.047)	(0.023)	(0.027)	(0.037)	(0.037)	(0.020)	(0.024)
Only HHs living in apartments	−0.013	0.006	0.005	−0.001	−0.092+	−0.105*	−0.014	−0.023
	(0.035)	(0.035)	(0.022)	(0.023)	(0.038)	(0.039)	(0.029)	(0.032)
Labor force subsample	−0.072+	−0.048	−0.003	−0.001	−0.050	−0.056	−0.016	−0.008
	(0.035)	(0.038)	(0.026)	(0.024)	(0.036)	(0.037)	(0.030)	(0.031)
Berlin dropped					−0.074*	−0.091*	−0.020	−0.016
					(0.027)	(0.029)	(0.024)	(0.024)
Baltic states dropped	−0.054	−0.023	−0.021	−0.014				
	(0.045)	(0.047)	(0.024)	(0.024)				
Unweighted regressions	−0.057+	−0.043	−0.002	−0.00000	−0.066*	−0.082*	−0.028	−0.021
	(0.028)	(0.034)	(0.019)	(0.019)	(0.025)	(0.029)	(0.024)	(0.023)

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.

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, neither in the CEE countries nor in the Western European comparator countries.

Next, to illustrate the potential influence of selection into and out of homeownership, we restrict the samples to individuals older than 40 years. These are people whose first decisions about homeownership have been plausibly dated before 1989. Taken at face value, homeownership seems to reduce unemployment, and in the case of Western Europe it is also statistically significant. While this result is again in the opposite direction than the Oswald hypothesis would predict, we are cautious with this interpretation. The renters in this subsample are likely to be nonrepresentative: If individuals prefer to own a home and more productive individuals are more likely to afford one and become homeowners, they drop out of the sample of renters as time progresses. Because all subsequent cohorts of renters are dropped from this subsample, the sample of renters becomes negatively selected. This result thus points to the role of selection bias as a potential explanation for the finding of a negative correlation between homeownership and unemployment in previous micro studies.

Then we look only at households living in apartments, as households of renters and privatizers in this subsample possibly live in more homogeneous housing arrangements. However, we find the same results as before. In the next robustness check, we restrict our sample to individuals who are in the active labor force, and again obtain quantitatively very similar results to our main regressions. Then we drop Berlin from the sample of Western European comparator countries with no effect on the results. We also 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 coefficients on homeownership increase in their absolute value in the

negative direction, but are never statistically significant. As a last robustness check, we re-estimate our regressions without sample weights and find that the results do not change. In summary, our findings of limited negative effects of homeownership on mobility and no effects of homeownership on unemployment are consistent across alternative specifications and robust to country composition.

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, reported in Tables A2 and A3 in the Appendix, are very similar to our findings for CEE countries. We again find weak evidence of the negative effect of homeownership on mobility, but no relationship between unemployment and homeownership, neither in full-sample estimates, nor in estimates on restricted sample of renters and privatizers. Although our knowledge about housing privatization in these countries is more limited (for some details see Broulíková 2017), this finding suggests that the results in this paper may be more general, extending to other post-socialist countries in the Balkans and the Former Soviet Union.

3.6 Interpreting Results for Transition Economies

Regarding homeownership and unemployment, the pattern of our findings for CEE countries reported in Table 5, as well as results for the Balkans and FSU, is somewhat different from our expectations. If homeowners are positively selected, we would expect the coefficients on homeownership in the sample restricted to privatizers and renters to be higher than our full-sample estimates, i.e. we expected a similar pattern as we obtained for Western European comparator countries. However, we note again that we are cautious when interpreting results for the comparator countries in the first place as we do not believe privatization therein provides a clear enough quasi-experiment.

A possible explanation for the results for CEE countries may lie in the nature of their housing and financial markets, together with their historical development outlined in

Section 2. Because housing markets and rental markets in these countries practically did not exist before 1989, a substantial part of homeowners in CEE countries are individuals who built on their own home or inherited their house and remained there, resulting in high levels of homeownership. Together with the fact that a substantial part of 2010 housing stock had been built before 1989, this may suggest that homeowners in CEE countries may be less selected than their counterparts in Western Europe, where individuals and housing markets have been naturally interacting for decades. This interpretation is also consistent with Gebhardt's (2013) more radical claim that homeownership during communism was inconsequential and therefore essentially randomly assigned. All in all, these observations together suggest that, due to path dependency, the bulk of homeownership observed in CEE countries in 2010 might be the result of random factors. This may explain why the results for the full sample estimates do not differ from our results that use privatization as a quasi-experiment in homeownership. This interpretation is also consistent with our finding that our full-sample results do not depend on whether or not we control for individuals' characteristics.

However, to the extent that the housing markets, the nature of privatization, and the less developed mortgage markets in transition countries appear to increase homeowners' transaction costs, we would expect the possibly detrimental effects of homeownership on labor mobility and unemployment to be amplified in the case of the CEE transition economies. Instead, we find the opposite and therefore interpret our results as suggesting that homeownership may have only a limited impact on mobility and no causal impact on unemployment. In addition, the results suggest that housing privatization as a policy does not have substantial detrimental effects on the labor markets as far as individual outcomes are concerned.

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 but our data is from 2010. During this period, individuals' housing status may have changed.

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 starting in 1990.

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 fist 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.[19] 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.

[19]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, while remaining in the same residence. We classify such cases as coding errors. The four year window was chosen in order to eliminate such errors.

Two types of households may fall in this quasi-privatizer category: renters in public housing who bought their housing units (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, one can argue that the homeownership status of renters who later buy their housing units from their landlords is probably more exogenous than the homeownership acquired in the housing market. This is because it essentially requires a mutual coincidence of wants between the landlord and the renter that occurs during an existing rental contract, together with overlapping reservation prices. The occurrence of such a coincidence may be thought of as rather random.

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. More interestingly, quasi-privatizers are not different from the rest of the population in terms of age, education, gender, or number of children.

In addition, quasi-privatizers seem to be somewhat less mobile, with the probability of changing residence being about one third compared to non-privatizers. Notwithstanding their higher homeownership rates and lower mobility, quasi-privatizers are not more often unemployed. If anything, their unemployment spells are shorter. However, they tend to be slightly more often self-employed and seem to work more hours per year.

Table 7: Summary statistics: Former East German sample of the GSOEP data (1990–2015)

	Non-Privatizers		Quasi Privatizers	
	Mean	St. Dev.	Mean	St. Dev.
Moved residence (=1)	0.057	0.231	0.021	0.143
Moved to another federal state (=1)	0.007	0.081	0.003	0.059
Moved between East and West (=1)	0.005	0.071	0.002	0.042
Unemployed (=1)	0.104	0.305	0.089	0.285
Months unemployed	1.224	3.25	0.988	2.949
Active in labor force (=1)	0.657	0.475	0.657	0.475
Yearly hours worked	1146.267	1126.916	1223.421	1154.565
Self-employed (=1)	0.051	0.22	0.078	0.269
Homeowner (=1)	0.418	0.493	0.592	0.492
Renter (=1)	0.556	0.497	0.393	0.488
Potential privatizer (=1)	0	0	0.524	0.499
Year of quasi privatization	–	–	2002.176	6.968
Age	46.869	17.516	46.268	17.398
Female (=1)	0.527	0.499	0.503	0.5
Number of children	0.575	0.919	0.608	0.88
Years of education	11.92	3.188	11.885	3.288
Number of observations	118769		3087	
Number of individuals	14881		226	

Data source: German Socio-Economic Panel 1990–2015.

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

$$y_{it} = \beta h_{it} + \phi_i + \boldsymbol{\tau}' \boldsymbol{year_t} \otimes \boldsymbol{age_{it}} + e_{it}, \tag{2}$$

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, $\boldsymbol{year_t}$ 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, $\boldsymbol{age_{it}}$ 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.[20] 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 housing unit and switched off if they are renters and (ii) the quasi-privatizer indicator that is switched on once the household declares they own the unit 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 unit 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, β may pick up individual-level labor market shocks. However, the estimate of β is unaffected by selection on 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 three mobility measures: a change in the residence, mobility across the 16 German states, and moves from the former East Germany to the former West Germany. The results are reported in Table 8.

[20]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.

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)				Moved from East to West (=1)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Homeowner (=1)	−0.078*	−0.074*			−0.008*	−0.016*			−0.007*	−0.011*		
	(0.001)	(0.004)			(0.0005)	(0.002)			(0.0004)	(0.001)		
Quasi-privatized (=1)			−0.045*	−0.015			−0.006*	−0.004			−0.005*	−0.001
			(0.003)	(0.008)			(0.0005)	(0.003)			(0.0004)	(0.002)
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	106,499	106,499	106,499	106,499	107,322	107,322	107,322	107,322	107,323	107,323	107,323	107,323
Adjusted R^2	0.078	0.145	0.051	0.140	0.020	0.175	0.018	0.174	0.018	0.247	0.016	0.246

Note: Standard errors clustered at the individual level are in parentheses: $^{+}p < 0.05$, $^{*}p < 0.01$. Data source: German Socio-Economic Panel 1990–2015.

The estimates in columns (1) and (2) suggest that homeowners are about 7.5 percentage points less likely to change residence in a given year, which is both a substantively and statistically significant effect, considering that the sample average is six percent (see Table 7). Interestingly, the result does not depend on whether or not fixed effects are controlled for. Similar patterns are found in columns (5) and (6), where the outcome is mobility across the German states, as well as in columns (9) and (10), where the outcome is mobility across the former East-West divide. If anything, the effect of homeownership on mobility increases when fixed effects are controlled for; the differences in coefficients between fixed effects and pooled models are statistically significant for the two cross-state mobility measures. This is inconsistent with the idea that less mobile individuals become homeowners. Rather, this suggests that either homeownership reduces mobility or that individuals become homeowners when their mobility (or need for it) decreases.

The pattern of results is different when we look at quasi-privatizers. In columns (3), (7), and (11), there is an apparent decrease in mobility once they become owners of their housing unit, although the estimates are substantially smaller than the corresponding estimates on the homeownership dummy and these differences are statistically significant. Once fixed effects are included, the reduction in mobility tends to drop further and loses statistical significance for all three measures used in columns (4), (8), and (12). The differences between the estimates with and without fixed effects are statistically significant for two out of the three mobility measures.

Altogether, the results in Table 8 suggest that the bulk of the observed lower mobility of homeowners is probably an artifact of individual-specific shocks (e.g. obtaining a stable job), rather than homeownership itself. This interpretation is consistent with our results for CEE countries above and the finding by Sodini et al. (2016) that homeownership increases mobility.

With respect to unemployment, the results reported in Table 9 show a story similar 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 5.6 percentage

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)	−0.056*	0.004			−0.798*	−0.038		
	(0.004)	(0.005)			(0.048)	(0.052)		
Quasi-privatized (=1)			−0.020	−0.018			−0.271	−0.349
			(0.013)	(0.015)			(0.182)	(0.189)
Individual fixed effects	–	Yes	–	Yes	–	Yes	–	Yes
Year effects × Age effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	121,854	121,854	121,854	121,854	100,371	100,371	100,371	100,371
Adjusted R^2	0.053	0.346	0.045	0.346	0.063	0.504	0.049	0.504

Note: Standard errors clustered at the individual level are in parentheses: $^{+}p < 0.05$, $^{*}p < 0.01$. Data source: German Socio-Economic Panel 1990–2015.

point lower unemployment, which is a significant difference when we consider that the sample average is about ten percent (see Table 7). This result matches the standard finding in the micro 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). This suggests that the negative relationship between homeownership and unemployment is driven by homeowners' characteristics, rather than homeownership itself. We find the same result when unemployment status is replaced with the number of months in unemployment.

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, although this result is not statistically significant. The estimate remains the same after individual fixed effects are included, suggesting that composition effects play no role. The same result is obtained if unemployment status is replaced with the number of months in unemployment, as reported in columns (7) and (8). Thus, our results are inconsistent with 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 mitigate the unemployment effects of homeownership. We find instead that homeowners are more likely to be in the active labor force. However, this correlation is entirely explained away by composition effects (comparing columns 1 and 2), suggesting the higher labor force activity is not due to homeownership. Consistent with this, we do not find evidence that quasi-privatizers would become more active in the labor force once they become homeowners (columns 3 and 4). Examining hours worked, we find in column (5) that homeowners tend to work significantly more hours per year, more than one month-worth of a full-time job. However, the estimated effect becomes negative and substantively close to zero once individual fixed effects are included in column (6). Quasi-privatizers seem to increase their hours worked by one to 1.5 weeks, as reported in columns (7) and (8),

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)	0.019*	−0.003			174.000*	−16.300			0.035*	0.008		
	(0.004)	(0.006)			(12.700)	(15.900)			(0.004)	(0.004)		
Quasi-privatized (=1)			−0.008	0.011			60.300	41.500			0.026	0.013
			(0.020)	(0.022)			(61.000)	(49.800)			(0.018)	(0.019)
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	121,854	121,854	121,854	121,854	121,854	121,854	121,854	121,854	121,854	121,854	121,854	121,854
Adjusted R^2	0.533	0.692	0.533	0.692	0.424	0.714	0.418	0.714	0.030	0.655	0.024	0.655

Note: Standard errors clustered at the individual level are in parentheses: $^{+}p < 0.05$, $^{*}p < 0.01$. Data source: German Socio-Economic Panel 1990–2015.

but this result is not statistically significant. Finally, the higher self-employment rate of homeowners, reported in column (9), is also explained away by composition effects, comparing with the result in column (10). The positive 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 created a quasi-experiment in homeownership that may be used to learn about its causal 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 privatizations were 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 housing unit 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. Yet we find no evidence that homeownership has a detrimental impact on individuals' unemployment risks and only weak evidence that homeownership limits people's mobility. 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 consistently around zero, suggesting that the standard negative relationship between unemployment and homeownership found in micro studies may be due to the endogeneity of homeownership status. In particular, it may be that individuals with low unemployment risks are more likely to become homeowners. In order to tap into this issue, we use the former East German sample of the GSOEP data that allows us to explicitly control for such unobserved heterogeneity. The results indeed suggest that unobserved heterogeneity is the driving force behind the negative relationship between homeownership and unemployment typically found in the previous literature.

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 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 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.

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Appendix

Table A1: Robustness checks: Dropping individual countries, Privatizers and Renters

Outcome:	Willing to Move (=1)		Unemployed (=1)	
	(1)	(2)	(3)	(4)
	Central and Eastern Europe			
Czech Republic	−0.066^{+}	−0.033	0.001	0.0004
	(0.033)	(0.037)	(0.020)	(0.021)
East Germany	−0.050	−0.030	0.009	0.004
	(0.031)	(0.034)	(0.020)	(0.020)
Estonia	−0.062	−0.033	−0.009	−0.004
	(0.034)	(0.037)	(0.019)	(0.020)
Hungary	−0.053	−0.038	0.009	0.008
	(0.030)	(0.034)	(0.019)	(0.020)
Latvia	−0.060	−0.039	0.013	0.011
	(0.034)	(0.037)	(0.021)	(0.022)
Lithuania	−0.045	−0.023	−0.008	−0.008
	(0.031)	(0.035)	(0.019)	(0.020)
Poland	−0.051	−0.027	0.011	0.009
	(0.032)	(0.036)	(0.019)	(0.020)
Romania	−0.050	−0.037	0.009	0.011
	(0.028)	(0.034)	(0.017)	(0.017)
Slovakia	−0.063^{+}	−0.043	0.005	0.005
	(0.031)	(0.035)	(0.019)	(0.020)
Slovenia	−0.066^{+}	−0.040	0.003	−0.001
	(0.031)	(0.031)	(0.021)	(0.022)
	Western Europe			
Great Britain	−0.076*	−0.095*	−0.015	−0.013
	(0.028)	(0.031)	(0.025)	(0.026)
Italy	−0.102*	−0.112*	−0.036	−0.029
	(0.035)	(0.037)	(0.034)	(0.034)
Sweden	−0.053	−0.071^{+}	−0.047	−0.042
	(0.029)	(0.036)	(0.026)	(0.025)
West Germany	−0.067^{+}	−0.084^{+}	0.008	0.012
	(0.031)	(0.034)	(0.026)	(0.026)

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)			
Sample:	Full Sample		Privatizers and Renters		Full Sample		Privatizers and Renters	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Homeowner (=1)	−0.095*	−0.093*	−0.042	−0.038	−0.081*	−0.078*	−0.076+	−0.064+
	(0.024)	(0.024)	(0.039)	(0.042)	(0.022)	(0.021)	(0.030)	(0.028)
Age	−0.006	0.001	−0.003	0.006	−0.003	0.002	−0.004	0.0002
	(0.003)	(0.003)	(0.009)	(0.008)	(0.002)	(0.002)	(0.003)	(0.004)
Age^2/100	−0.005	−0.011*	−0.007	−0.017	−0.006+	−0.011*	−0.004	−0.008
	(0.004)	(0.004)	(0.011)	(0.010)	(0.003)	(0.003)	(0.004)	(0.004)
Willing to take risks (1–10)		0.029*		0.031*		0.025*		0.022*
		(0.004)		(0.006)		(0.003)		(0.005)
Years of education		0.013*		0.014		0.016*		0.024*
		(0.004)		(0.013)		(0.004)		(0.006)
Female (=1)		−0.009		0.002		−0.035		−0.026
		(0.038)		(0.078)		(0.026)		(0.050)
Married (=1)		−0.033		−0.011		−0.074*		−0.069
		(0.019)		(0.053)		(0.017)		(0.039)
Number of children		−0.004		−0.086*		0.014		0.014
		(0.009)		(0.030)		(0.009)		(0.014)
Number of adults		0.006		−0.010		0.011		0.001
		(0.009)		(0.026)		(0.007)		(0.009)
Foreigner (=1)		−0.021		0.012		0.040		−0.006
		(0.041)		(0.071)		(0.029)		(0.044)
Communist before 1989 (=1)		0.094*		0.128		0.003		0.050
		(0.035)		(0.072)		(0.026)		(0.043)
Parents were communists (=1)		0.042		0.030		0.030		−0.028
		(0.025)		(0.040)		(0.017)		(0.023)
Married × Female		−0.018		−0.013		0.012		0.038
		(0.025)		(0.073)		(0.022)		(0.055)
Number of children × Female		−0.016		0.051		−0.034*		−0.036+
		(0.010)		(0.044)		(0.010)		(0.018)
Number of adults × Female		−0.008		−0.017		−0.012		−0.015
		(0.012)		(0.033)		(0.007)		(0.010)
Country-district dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,587	6,587	1,200	1,200	9,930	9,930	3,823	3,823
Adjusted R^2	0.122	0.158	0.139	0.179	0.105	0.145	0.144	0.175

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)	0.008	0.007	−0.004	−0.010	−0.009	−0.016	−0.018	−0.034
	(0.018)	(0.018)	(0.042)	(0.049)	(0.017)	(0.018)	(0.022)	(0.023)
Age	0.0001	0.003	0.006	0.008	0.002	0.007*	0.002	0.009+
	(0.002)	(0.003)	(0.005)	(0.006)	(0.002)	(0.003)	(0.004)	(0.004)
$Age^2/100$	−0.006+	−0.010*	−0.011	−0.013	−0.006+	−0.011*	−0.005	−0.013*
	(0.003)	(0.003)	(0.006)	(0.007)	(0.003)	(0.003)	(0.005)	(0.005)
Willing to take risks (1–10)		−0.003		−0.002		0.003		0.004
		(0.004)		(0.007)		(0.002)		(0.003)
Years of education		−0.007		−0.016		−0.012*		−0.014+
		(0.004)		(0.009)		(0.004)		(0.006)
Female (=1)		−0.007		−0.109		−0.035		0.004
		(0.036)		(0.081)		(0.022)		(0.034)
Married (=1)		−0.036		−0.062		−0.076*		−0.099*
		(0.019)		(0.050)		(0.016)		(0.025)
Number of children		−0.009		−0.025		0.013+		0.023
		(0.007)		(0.018)		(0.006)		(0.014)
Number of adults		0.004		0.011		0.019*		0.030*
		(0.007)		(0.029)		(0.006)		(0.010)
Foreigner (=1)		−0.001		0.042		0.006		0.026
		(0.028)		(0.054)		(0.020)		(0.032)
Communist before 1989 (=1)		−0.002		−0.049		0.001		0.001
		(0.020)		(0.049)		(0.020)		(0.035)
Parents were communists (=1)		0.018		0.045		0.018		−0.007
		(0.015)		(0.039)		(0.011)		(0.018)
Married × Female		0.035		0.096		0.054*		0.057+
		(0.020)		(0.068)		(0.020)		(0.029)
Number of children × Female		−0.005		0.014		−0.014+		−0.022
		(0.011)		(0.033)		(0.007)		(0.015)
Number of adults × Female		−0.002		0.017		−0.011		−0.019
		(0.010)		(0.038)		(0.007)		(0.010)
Country-district dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,587	6,587	1,200	1,200	9,930	9,930	3,823	3,823
Adjusted R^2	0.060	0.062	0.032	0.038	0.102	0.112	0.117	0.130

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.