

Economic Transition and Private-Sector Labor: Evidence from Urban China*

Lakshmi Iyer,[†] Xin Meng,[‡] Nancy Qian,[§] Xiaoxue Zhao[¶]

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Abstract

This paper studies the transition of labor from the state sector to the infant private sector in urban China. We examine the impact of the first wave of housing reforms, which untied access to housing in urban areas from working for the state sector. We find that the reform significantly increased private housing ownership and private-sector employment, and reduced private-sector wages. The housing reform had no effect on unemployment or self-employment.

Keywords: Economic Transition, Structural Change, Labor Mobility, Housing Reform, China.

JEL: O12, P2, P26, J23.

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[†]University of Notre Dame; liyer@nd.edu

[‡]Australian National University; xin.meng@anu.rpss.edu.au

[§]Northwestern University, NBER, BREAD, CEPR; nancy.qian@kellogg.northwestern.edu

[¶]Yale University; xiaoxue.zhao@yale.edu

1 Introduction

One of the key economic events of the late twentieth century is the transition of centrally planned socialist economies to market economies. According to the International Monetary Fund, 36 countries have experienced economic transition, affecting over 40% of the world's population, only ten of which had completed the process by the beginning of the new millennium (IMF, 2000). An essential component of economic transition is the movement of labor from state employment to private employment. This has featured prominently in the IMF's policy recommendations to Eastern European transition economies during the early 1990s (Mitra et al., 2002) and in the highly profiled *China 2030* report by the World Bank and the Chinese State Council (World Bank, 2012, p. 88). In this paper, we study the untying of access to urban housing from state-sector employment in China. The main outcomes of interest are private housing ownership, the sector of employment and wages. Did the housing reform increase employment in the private sector? How did it affect wages in the infant private sector? Did it increase grassroots entrepreneurship? The answers to these questions are important for understanding the Chinese transition experience. They are also more generally interesting because most transitioning economies experience similar changes, where state-sector employment is untied from access to important non-wage benefits, amongst which housing is obviously one of the most important.

The Chinese context offers several advantages for understanding the details of economic transition. In contrast to Eastern Europe, the Chinese transition process took place under political and macroeconomic stability, and there was considerable variation in the timing of economic reforms across Chinese provinces and cities, which facilitates econometric identification.¹ The limited population migration across cities in China enables us to treat each city's labor market as relatively isolated. Though there was no comprehensive firm-level survey in the early transition years, we are able to use China's *Urban Household Income and Expenditure Surveys* (UHIES) to measure employment status, housing ownership and wages. Note that private enterprises in this household survey are businesses started by individual entrepreneurs and are distinct from privatized state firms which have been the focus of most other studies of the private sector in China.²

¹Other studies which exploit such variation across provinces include Lin (1992), who examines the impact of the Household Responsibility System.

²For example, see Huang (2003). We do not include large enterprises that were formerly state-owned or grew from state-owned enterprises because such firms typically behaved like state firms with respect to the housing reform. We

The two main empirical challenges to our paper are data availability and causal identification. To address the first challenge, we construct a novel dataset on city-level reform dates using information from province-level newspapers in the National Library archives in Beijing. Our main variable of interest is the date of the first reform which untied access to housing from state-sector employment.³ We combine these data on reform dates with UHIES household survey data for a panel of 27 cities over 1986-2005. Since the reforms were introduced at different times across cities, we can use a difference-in-differences approach to compare labor market choices of household heads or individuals before and after the introduction of the housing reform, between cities that have already introduced it and those that have not. Our baseline specification includes city fixed effects, which control for all time-invariant differences across cities, and year fixed effects, which control for all changes over time that affect cities similarly, such as macroeconomic changes or introduction of national policies (including the national law on urban housing reform enacted in 1994). Since the timing of the reform was determined in part by pre-reform city government revenues, we show that our results are robust to controlling for pre-reform city government revenues interacted with year fixed effects.

We find that the housing reform increased private ownership of housing and had a large and statistically significant impact on the probability of a household head being employed by a private-sector firm. Between 1986 and 2005, employment in private firms increased from zero to 6.7% of the total urban labor force. We find that the housing reform accounts for 30% of the total increase in private-sector employment. It also reduced wages in the infant private sector relative to state-sector wages. A one standard-deviation change in the timing of the reform reduces wages by 0.21 standard deviation for an individual working in the private-sector. As we will discuss later in the paper when we interpret the results on wages, there are likely to be several forces behind the negative effect on relative private-sector wages: a shift of labor into the private sector, the decline of housing subsidies in the state sector, and possible negative selection out of the public sector. We provide suggestive evidence against the decline in housing subsidies driving our estimates. It is beyond the scope of our paper to be more conclusive on this point.

Interestingly, we do not find any increase in the probability that the household head owns his/her own business – i.e., is an entrepreneur or self-employed. Together with the results on private-sector

discuss this in more detail in Section 4.

³These reforms have been enacted in at least 50 Chinese cities, affecting more than 90 million people, making it the largest urban property reform in history.

labor supply, these results are consistent with the belief that untying job-specific benefits increases worker mobility, but cannot spur entrepreneurship without other changes such as the relaxation of credit constraints. To further investigate whether the main results are driven by the untying of benefits from state-sector employment, or income effects from the monetization of private housing, we control for the introduction of a second reform, which enabled households to buy and sell housing. Our main results are unchanged by this control, which again supports our interpretation that untying access to housing from state-sector employment is the main driving force.

The main caveat for interpreting the reduced form estimates is that the housing reform may be confounded with concurrent reforms or factors that increased private-sector labor demand. We conduct a large number of robustness exercises to show that our estimates are unlikely to be biased by such factors. These include controlling for the fraction of state-owned firms in the city (as a proxy for concurrent SOE restructuring reforms), province-level GDP and employment growth trends, and pre-reform city-level employment growth trends interacted with year fixed effects. We also examine two placebo outcomes which can be interpreted as indicators of economic growth – re-employment and unemployment. We find that the housing reform had no effect on either outcome.

The main results show that the housing reform had a statistically significant and economically important impact on private housing ownership, the sector of employment and private-sector wages. Another way to examine the economic significance of the housing reform is to conduct a scaling exercise. With the reduced form estimates of the housing reform on private-sector labor supply and wages, we can ask: how much did the change in labor supply due to the housing reform affect wages? We construct a 2SLS estimate of private-sector labor supply on wages, where we use the housing reform as an instrument for labor supply.⁴ The estimates show that when the housing reform causes a 1% increase in the private-sector labor force, private-sector wages decline by approximately 0.29%. This supports the interpretation that the housing reform had economically important effects on the private sector.

Our findings contribute to a large literature on economic transition and structural change. Existing empirical studies in this literature have mostly been descriptive or have focused on cross-country comparisons.⁵ We add to these by using micro data and a policy experiment to provide detailed

⁴It is important to note that the goal of our 2SLS is not to establish the causal effect of labor supply on wages, which would require additional assumptions about the exclusion restriction. See section 6 for a more detailed discussion.

⁵Please see Guriev and Megginson (1997), Megginson and Netter (2001) and Roland (2002) for overviews of this

and well-identified evidence that untying benefits from state-sector employment can be an effective policy for gradually releasing labor into the private sector. Our general finding that the housing reform had a large effect on urban labor market choices is consistent with the work of Wang (2012), which finds that housing reforms increased entrepreneurship and attributes this finding to relaxed credit constraints. We differ from her study in focusing more on general equilibrium effects. Wang (2012) compares individuals that were previously employed in the state sector to those that were not, before and after 1994, the date of the national housing reform law. However, we find that most of the cities in our data had already enacted the housing reform prior to the national law. Our empirical strategy therefore compares the effects of reforms across cities before and after implementation, using city-specific reform dates that began in 1988 and ended in 1997. The different strategies may explain why we find no effects on entrepreneurship.

The evidence that the increase in labor mobility reduced private-sector wages adds to studies that investigate why privatization policies are unpopular despite their obvious benefits to the economy at large (e.g., Denisova et al., 2009; Fernandez and Rodrik, 1991; Zhuravskaya, 2007). Since the increased movement to the private sector caused by the housing reform was gradual and voluntary, our results provide some support for gradual transition (e.g., Aghion, 1993; Roland, 1994). The empirical findings are also consistent with the theoretical study by Aghion and Blanchard (1994), which argues that to avoid unemployment and wage reductions, private-sector job creation should take priority over state-sector restructuring even if shedding labor from the state sector will increase productivity.⁶

This study is organized as follows. Section 2 discusses the background on privatization and housing reforms. Section 3 discusses our identification strategy and Section 4 describes the data. Section 5 presents the main reduced form results of the effect of the housing reform on housing ownership, employment decisions and wages, as well as the robustness exercises. Section 6 examines the economic significance of the housing reform through labor-supply channels. Section 7 offers concluding remarks.

literature.

⁶Kikeri (1998) provides a review of what happens to labor when governments divest from firms.

2 Private Enterprise and Housing Reform in Urban China

2.1 Urban Housing Reforms

Until the post-Mao economic reforms that began in 1978, almost all urban workers in China worked for state-owned enterprises (SOEs). Leaving state employment was extremely costly as it was tied to a range of benefits, the most important of which was employer-provided housing. Historically, urban housing was allocated by SOEs to workers at a highly subsidized rent. The subsidies were financed by the local and upper levels of government.⁷

Housing reforms began in the mid-1980s, as part of the general movement towards a market economy and also because city governments wanted to be relieved of the responsibilities of providing housing.⁸ It was administratively cumbersome to allocate housing and the gap between the maintenance cost of state-owned housing and the nominal rents paid by the residents grew over time (Duda et al., 2005). The rising costs of urban housing provision for city budgets became increasingly problematic as city government revenues from SOEs declined during the early part of the reform era. In the late 1980s, cities with the most pressing budgetary problems began to push for reforms that would include selling off part of the old state-owned housing to employees at a subsidized price (Yuan, 2000; Pan, 2000). New workers in the state sector would no longer be eligible for state housing but workers who left their state jobs could remain in the same housing if they had purchased it. This effectively untied access to housing from state-sector employment.

The pricing of housing purchases was decided by city governments with inputs from SOEs. Prices took into account the age, size, location and quality of the housing.⁹ In addition, workers were offered “concessions” (price reductions) based on their characteristics. The most important was based on job tenure: the longer the tenure at the work unit, the higher the concession (Wang and Murie, 1999). In 1994, the central government codified the housing reform as a national law called the *Urban Real Estate Administration Act* to be officially “enacted” in 1995. Since most cities

⁷Some firms and work units were owned by the central government, some were owned by the provincial government, others were owned by the city governments. Finance of housing and any other spending on welfare were the responsibilities of the respective governments.

⁸A few locations were used to experiment with housing reforms in the early 1980s. They are not in our sample and therefore we do not discuss them for the sake of brevity.

⁹The basic guideline was that the price of a new apartment should not be higher than three times the average household annual income in a city. If buying an older house, the price would be adjusted according to a depreciation formula that fully depreciated the house over 75 years (Wang and Murie, 1999).

in our sample had already implemented the reform by then, we do not use 1994 as a reform date. Instead, our empirical strategy will use the city-specific introduction dates.

The wealth gains from the reform are unclear *ex ante*. We should note that households enjoyed considerable rental subsidies when living in state-owned housing. According to our data, during 2001-2005, a person who rented from the state paid on average 271 RMB (26 RMB per square meter) per month, while a person who rented from a private owner paid on average 1,362 RMB (71 RMB per square meter). Wang (2012) uses price and rent subsidy data from the China Household Income Project and calculates that the average price subsidy for purchase was approximately 24,462 RMB while the present discounted value of rental subsidies was approximately 20,223 RMB. She concludes from this that the subsidized purchase price offered in the housing reforms did little to increase overall household wealth.

The gains in wealth, moreover, were difficult to realize in practice. Privately owned housing could not be sold until the implementation of a second reform that allowed the trading of housing on the market. We collected data on the introduction of this second policy and include it as an additional control in our robustness checks, to verify that our results are not driven by wealth gains for households. Furthermore, we should note that state-owned banks did not make loans to households until very recently. Similarly, it was difficult for creditors to repossess housing used as collateral in the case of a default. Therefore, the ability of new property owners to use their newly acquired housing as collateral for business or other types of loans was very limited.

2.2 Urban Economic Transition

As with many other transitioning economies, the Chinese government's official position during the early period of transition was to tolerate and not interfere with the emerging market economy, rather than to proactively assist it. Private enterprises therefore faced several barriers to growth during the period of our study, 1986-2005, including limited access to formal credit and legal contract enforcement. For example, in 2003, even with the most expansive definition of what constitutes a private enterprise, the private sector accounted for only 1.75% of loans from all formal financial institutions in China.¹⁰ With the encouragement of the China Development Bank, banks began to

¹⁰Data from *China Industrial Statistical Year Book* 2004 and the *Almanac of China's Finance and Banking* 2005.

make loans to small and medium enterprises starting in 2004.¹¹ The stock market was also of little use to new enterprises since a firm needs to reach a certain size before it can become public. Of the 976 companies listed on the Shanghai and Shenzhen stock exchanges in 1999, only eleven were non-state firms (Huang, 2003). It is widely believed that new enterprises mostly relied on their own savings and informal credit (McMillan and Woodruff, 2001). Private enterprises also lacked access to legal contract enforcement.¹² The presence of credit constraints and limited contract enforcement is important to keep in mind for interpreting the results since they can limit firm growth. Despite the lack of contract enforcement, it is widely believed that Chinese business owners faced little government predation and a low risk of expropriation, which may have encouraged private-sector growth relative to other low- and middle-income countries.¹³

Large-scale restructuring of the state sector mostly occurred during 1993-1998. These reforms divided state-owned enterprises (SOEs) into three groups: firms that continued to be directly owned and financially supported by the state, firms that were allowed to sell equity to non-government entities to raise capital (e.g., other SOEs and private entities) and firms that were shut down. The second group of “mixed-ownership” firms are among those classified as “other ownership” in our data, though the state continued to have a leading role in the control of such firms. For example, data from the *1998 Annual Survey of Above-scale Industrial Firms Panel (ASIFP)* show that the state is the majority equity holder in the average manufacturing firm of mixed ownership; less than 10% of equity is held by individuals.¹⁴ With political stability as the ultimate objective, the government was cautious in ensuring that enterprise reform did not alter urban public goods and social security, which were mostly provided by SOEs (e.g., Bai et al., 2001; Bai et al., 2006). Thus, when ownership changed from being state to mixed, the new mixed ownership enterprise continued to provide similar benefits to workers as the state enterprise did previously.

It is important to consider the effect of enterprise restructuring on unemployment. The Chinese

¹¹Interviews by authors with officials in the China Development Bank, who described government policies about bank loans to private enterprises.

¹²The details of the legal reforms are described on the Chinese government website <http://www.law110.com/law/guowuyuan.htm> (in Chinese). See Meng (2000) for a detailed description of Chinese labor market reforms.

¹³For example, Bai et al. (1999) provide evidence that one mechanism for avoiding predation was to provide urban residents with anonymous bank accounts. See McMillan and Woodruff (2002) for a comparison of the environment faced by Chinese entrepreneurs relative to those in Vietnam and Eastern European countries. Note that their discussion mostly focuses on the early parts of the Chinese transition, which was mostly driven by rural Town and Village Enterprises.

¹⁴The ASIFP is often referred to as “the Census of Manufacturing Firms”.

government was committed to keeping urban unemployment rates low during transition and past studies have found that enterprise reforms did not reduce total employment (Bai et al., 2009). However, workers of both state and mixed ownership firms were often forced to take early retirement, before the official retirement age of 55 for women and 60 for men (Giles et al., 2006). These workers are not counted as being in the labor force and thus do not contribute to unemployment statistics. Some of these workers, who might be re-employed after official retirement, are listed as “retired and re-employed” in the UHIES data. In our empirical analysis, we examine both “unemployment” and “retired and re-employed” as separate labor market indicators, and verify that neither is correlated with the timing of urban housing reforms. We will discuss this in more detail in the Data section.

3 Empirical Strategy

To identify the effects of the housing policy on housing ownership and occupational choice (sector of employment), we examine the effect of the housing reforms on household level outcomes using a difference-in-differences specification, which compares household outcomes before and after the housing reform is implemented in a city:

$$y_{hjt} = \theta H_{jt} + \psi X_{hjt} + \gamma_j + \delta_t + \varepsilon_{hjt}, \quad (1)$$

where y_{hjt} is the outcome (e.g., housing status, household head’s labor market choice) for household h in city j during year t . This is modeled as a function of a dummy variable that equals one if the urban housing reform had taken place in that city, H_{jt} ; a vector of individual controls that includes sex, age and the years of education of the household head, X_{hjt} ; a vector of city dummy variables, γ_j ; and a vector of year dummy variables, δ_t . Our coefficient of interest is θ , which captures the effect of the urban housing reform. To adjust for the fact that households in the same city might face correlated shocks in the same year, we cluster the standard errors at the city level. Since clustering may produce downward biased standard errors when the number of clusters is not large (we have 27 clusters), we also implement wild bootstrapped clustered standard errors (Cameron et al., 2008). Both sets of p-values are reported in the tables for the main results.

Causal identification assumes that the outcome of interest and the introduction of the reform are not joint outcomes of an omitted variable. For example, one obvious concern is that private-sector

employment and the housing reform might both be driven by a poorly performing state sector. This could push workers from the state sector into the private sector and cause budget-constrained city governments, which rely partly on state-owned enterprises for revenues, to privatize housing. However, the main determinant of the timing of the reforms, city government income prior to the reform, is already controlled for by the city fixed effects, along with all other time-invariant differences across cities. Nevertheless, to account for the possibility that cities with lower pre-reform revenues were on a different trajectory, we estimate specifications controlling for pre-reform city revenues interacted with year dummies. We also conduct a placebo test by examining the probability that an individual is “retired and re-employed” or unemployed. We discuss these in more detail in the Sections on Data and Robustness.

Similarly, any changes over time that affect all cities similarly are controlled for by the year fixed effects. This includes nationwide macroeconomic changes as well as other policy changes such as the introduction of a central government urban housing reform in 1994.

The main caveat for interpreting our estimates as the causal effects of the housing reform is the possibility of other reforms or changes that coincide with the introduction of the housing reform. We address this in detail after we present the main results in Section 5.5.

In terms of mechanisms that might drive the relationship between housing reforms and movement towards the private sector, our preferred explanation is the removal of “job-lock” caused by the state’s provision of housing. A different potential mechanism could be wealth effects which might lead people to invest in new private businesses, and thereby generate greater demand for private-sector labor. This seems unlikely *ex ante* for the reasons that we discussed in Section 2.1. Nevertheless, we will investigate it after we present the main results in Section 5.4.

Our strategy for estimating the effect of the housing reform on wages is similar to the strategy discussed in this section. We will describe it in more detail when it becomes relevant.

4 Data

4.1 Housing Reforms

We construct a new data set of housing reform dates by collecting information on the date when workers were first allowed to buy their houses from their work units. We will refer to this reform

as the “housing reform” hereafter. We also collected the dates of the introduction of subsequent housing reforms that allowed owners of private housing to rent or sell on the market (referred to simply as the “second reform”). Information on both reforms was obtained by a search of province-level newspapers.

4.2 The Urban Household Income and Expenditure Surveys

We obtain household survey data from the *Urban Household Income and Expenditure Surveys* (UHIES) conducted by China’s National Bureau of Statistics. The UHIES is a stratified random sample of the urban population. We have access to the UHIES data for sixteen provinces over the years 1986-2005. The data are a repeated cross-section of households.

Our employment and wage analysis will focus on household heads. The definition of household head according to the UHIES manual is “the person who is regarded by household members as the one who makes the decision in general household affairs”. Since the average household size in our sample is three, the omitted members are typically the spouse and a dependent child.¹⁵

The UHIES categorizes employment into the following categories: i) employed by a state-owned enterprise; ii) employed by a collective-owned enterprise; iii) employed by “other” enterprise; iv) employed by a private enterprise; v) self-employed or small enterprise owner (hereafter referred to as “self-employed”); vi) “retired and re-employed”; and vii) other. We classify individuals in categories (i) and (ii) as state-sector workers. The category “other enterprises” presents a difficulty in classification: this includes partially privatized state-owned firms (in many cases, the state retains a controlling share), as well as joint ventures and foreign-owned firms.¹⁶ Since we are unable to assign this category to either the state or private sector in any meaningful way, we exclude this category from our main regressions for sector of employment and wages.¹⁷

The UHIES reports registered unemployment, which is widely regarded as an undercount of true unemployment (Meng, 2012). Thus, to examine unemployment, we construct a broad measure of

¹⁵We show in the appendix that our estimates are similar for the spouse as well as when we examine all adult household members. For brevity, we will not discuss these results.

¹⁶We examined the composition of this category in some detail using the 2005 China Household Income Project (CHIP) dataset, which provides more disaggregated information (but is only available for a few years). Less than one-fourth of these “other enterprises” were joint ventures or foreign owned in 2005, about half were state-controlled and the rest were of unknown (“other”) ownership.

¹⁷Since such “mixed” enterprises often had housing policies similar to state-owned firms, we also conduct robustness checks where we classify these as part of the state sector. Our results change very little with this addition. The results are available upon request.

unemployment based on the number of workers who report no sector of employment.¹⁸ Since this measure includes those who are still in school, those who are unable to work because of disability, and those who voluntarily choose to exit the labor force, it should be interpreted as an upper bound of unemployment rates.

There are several features of the UHIES data that are important to keep in mind. First, it reports total labor earnings for each individual, which we use as a proxy for wages in the absence of data on the amount of time worked. Therefore, our results should be cautiously interpreted bearing in mind that we assume that the number of hours worked does not change systematically with the introduction of the housing reform.¹⁹ Second, the survey questions regarding household income changed after 1988. For consistency, we do not examine wages prior to 1989, which reduces the sample size available for the wage analysis. Consistent with the strong growth of the Chinese economy in the 1990s, average real wages more than doubled in the post-reform period compared to the pre-reform period.²⁰

Another limitation of the UHIES is that it under-counts migrant laborers. Before 2002, the survey only sampled households with urban household registrations. After 2002, a limited number of households with rural household registration are included in the survey (e.g. only 2% of the sample in 2003 have rural registration).²¹ For consistency, individuals with rural household registration in the 2002-2005 surveys are excluded from the sample for the main analysis. Given that almost none of the migrant workers worked in the state sector or enjoyed any housing benefits (based on government policy), the omission of migrants should not affect our examination of the reduced form effect of the housing reform on the housing ownership, the sector of work or wages.²² However, the undercounting of migrants means that we could mis-measure the increase in private-sector labor

¹⁸We restrict our sample to household heads between 20 and 80 years of age. When defining labor market variables, we consider females aged 22-60 and males aged 22-65 to be in the labor force. The age ranges are chosen with the following logic. Most workers have completed their education by age 22. The retirement age for state employees are 60 for men and 55 for women. Since many are reemployed after the standard retirement age, we examine men up to 65 and women up to 60 years of age. Our results are not sensitive to the choice of age range. Estimates using alternative definitions are available upon request. Unemployed individuals are those who are in the labor force, but report no sector of employment.

¹⁹Note that in 1995, China introduced a law limiting the number of days worked each week to five. In our estimates, this will be absorbed by the calendar year fixed effects.

²⁰All wages are reported as real wages.

²¹This is probably because the majority of rural-urban migrants either live on the periphery of cities where rents are cheaper or in dormitories or workplaces such as construction sites.

²²Several papers have also documented the existence of labor market segregation and that migrant workers often do not have access to the same opportunities as those with urban hukou (Meng, 2012; Meng and Zhang, 2016).

supply due to the housing reform. We will discuss this more in Section 6 when it becomes relevant.

We also collect data on city-level GDP and government revenue, and province-level GDP growth and state-sector employment growth rates. These data were obtained from province and city-level statistical yearbooks, and will be used as robustness controls for our regression analysis.

4.3 Descriptive Statistics

We have both the reform date and household data for 27 prefecture or higher level cities over the entire 1986-2005 period.²³ The cities in our sample began to implement the reform in 1988. By the time that the national law was proclaimed in 1994, 93% of the cities had already introduced the housing reform. The last city to introduce the housing reform did so in 1997 (see Appendix Table A.1). These statistics reveal several important facts. First, they show that there is substantial variation in the timing of the introduction of the housing reform for our empirical strategy to exploit. Second, the fact that the earliest reforms in our sample occurred at the beginning of the period of our study means that when we examine pre-trends for each year before the reform, we lose a substantial number of observations. Third, since 93% of cities had already introduced the housing reform by 1994, it would be inappropriate to use the national law in 1994 as a uniform reform date for all of China. Finally, the fact that the last city to implement the housing reform did so in 1997 means that our analysis, which uses data up to 2005, should be interpreted as the effect of labor supply shifts on wages for an eight-year period (i.e. the average of the short- and medium-run effects).

According to anecdotal evidence, low government revenues were the main determinant of early reform introduction. We check whether this was true on average by examining the correlation between housing reform timing and city government revenues, which are reported by city-level statistical yearbooks. We aggregate our data to the city level and estimate the correlation between the year of introduction and the pre-reform average log government revenue measured in 1986-1988.

²³The household survey covers 16 provinces or municipalities over 1986-2005, comprising 100 cities before 2002 and 238 city codes from 2002. We have housing reform dates for 42 of these cities. However, only 28 of the 42 cities were included in the pre-2002 surveys. Of these, one was a county-level city which was excluded to preserve comparability within the sample. The remaining 27 cities are all prefecture or higher level cities. Our data comprises around 40% of the households for the pre-2002 surveys and one-third of households for the surveys since 2002. The cities excluded from our final data set are mostly smaller or newly established cities. Because of this dominance of large cities, households in our final data set have higher income, more educated household heads, and smaller household sizes relative to the households which are not included.

When we regress the year of the housing reform on the one-year lag of log city government revenues, we find that the R-squared is 0.72, which means that pre-reform city government revenues explains 72% of the variation in the timing of the reforms across cities (results not shown in tables).²⁴ This provides strong support for the belief that the main determinant of timing of the housing reform was pre-reform city government revenues.

Only 11% of households owned their own housing before the introduction of the housing reforms, whereas 73% owned their housing afterward. This increase is paralleled by a decline in the percentage of households that rent from the state from 86% to 23% (Table 1, Panel A).²⁵

In our sample, the average household head is more likely to be male, approximately fifty years of age and has twelve years of education (Table 1, Panel B). Consistent with state-sector restructuring, we see that it grows from seven to seventeen percentage-points from before to after the reform (Table 1, Panel B).

To examine the sector of employment, we restrict the sample to households where the head reports a non-missing value for this variable. The descriptive statistics are very stark. Prior to the reform, there was virtually no one in the private sector. After the reform, 5% of the sample report as being employed by a private enterprise, while 4% report being self-employed or owning a small enterprise. Analogously, we see a decline of state-sector employment from 97% to 80%. Consistent with the aging demographic, we find that the number of “retired and re-employed” workers increases from 1 to 8%.

Before presenting the regression results, we examine the evolution of the outcomes of interest before and after the introduction of the housing reform to see whether there is a pre-trend. This also allows us to investigate whether the outcomes begin to change when the reform is introduced. Otherwise, we would be concerned that our post-reform estimates capture spurious trends. Figure 1 plots private housing ownership against the number of years since the housing reform. Year “0” indicates the first year of the reform. It shows little change over time prior to the housing reform and then a gradual but continuous rise afterwards. This pattern is consistent with the notion that it takes some time for the individual work units to clarify the procedures and pricing for the

²⁴When we regress the year of the housing reform on a contemporaneous measure of log city government revenues, we find that the R-squared is 0.82, which means that current city government revenues explains 82% of the variation in the timing of the reforms across cities (results not shown in tables).

²⁵The share of households owning housing and renting from the state does not add up to 100%; the remainder comprise those who rent from the private sector and those who live in “other” types of housing.

transfer of housing to private ownership, or for households to save up enough money to actually buy out the housing from the state (access to mortgage loans was severely limited in China during this period). Figure 2 plots the analogous figure for the fraction of workers employed by a private enterprise. It shows that there are almost no workers in the private sector prior to the housing reform, but afterwards, there is a gradual and continuous increase. Both figures indicate that there is no pre-trend prior to the introduction of the housing reform and the trend changes gradually after the reform. Figure 3 plots private-sector entrepreneurship against the number of years since the housing reform. Although there is an increase over time, it appears to begin later than for housing ownership and private-sector employment.

5 The Effect of the Housing Reform on Housing Status and Labor Supply

5.1 Housing Status

Using the difference-in-differences specification shown in equation (1), we find that the housing reform increased the probability of a household owning their home by 6.8 percentage-points (Table 2, column 1). The coefficient is statistically significant at the 10% level. The baseline estimates control for the age, sex and years of educational attainment of the household head. In column (2), we additionally control for the quadratic terms of the age and schooling of the household head to allow their influences to be non-linear. The estimates are unchanged. In column (3), we include controls for the interaction of year fixed effects with the household head's age and with her education. This is to address the fact that the returns to age and education may change as the economy liberalizes. Note that wild bootstrapped p-values are presented at the bottom of the table. They are all significant at the 10% level, except for column (2), where the p-value is 0.102. Since wild bootstraps produce very similar estimates across the various specifications, for brevity, we will not discuss them henceforth.

5.2 Private-Sector Employment

We find that individuals are significantly more likely to be involved in private-sector work after cities implement urban housing reforms (Table 3, column 1). The reform increased the probability of a household head working for a private business by two percentage-points, which is statistically significant at the 5% level. The estimates are very robust to different ways of controlling for individual characteristics in columns (2) and (3). In contrast, we find no effect of housing reform on the probability of the household head being self-employed or owning a small enterprise (columns 4-6). The coefficient is small in magnitude and statistically insignificant.

Given that the overall probability of employment in the private sector increased from zero to 6.7 percent over the period 1986-2005, our estimates imply that housing reforms account for 30 percent of the overall increase in private-sector employment over this period.

The probability of renting housing from the state or state-sector employment are both decreasing almost one-for-one with the increase in private housing and private-sector employment respectively, though the estimated coefficients are statistically insignificant (results available upon request).²⁶

5.3 Private-Sector Wages

Table 4 presents estimates for the effect of the housing reform on wages. Since the reform shifted labor from the state sector to the private sector, the effect on wages will differ across sectors. Therefore, we estimate the following equation, which captures this differential effect.

$$\ln w_{ijt}^k = \lambda_1 \ln H_{jt} + \lambda_2 [\ln H_{jt} \times P_{ijt}] + X_{jt}' \gamma_k + Z_{ijt}' \zeta_l + \gamma_j + \delta_t + \varepsilon_{ijt}, \quad (2)$$

where $\ln w_{ijt}^k$, the log wage of individual i living in city j during year t and who works in sector k (private, state) is a function of a dummy variable that equals one if the urban housing reform had taken place in that city, H_{jt} ; its interaction with a dummy variable for whether individual i works in the private sector, P_{ijt} ; a vector of individual controls such as the sex, age and years of education and the uninteracted dummy for whether an individual works in the private sector, Z_{ijt} ; a vector of time-varying city characteristics that we introduce later in the robustness checks, X_{jt} ; city and year

²⁶That these estimates are imprecise is probably due to the fact that the changes caused by the housing reform are small compared to the total number of state-sector workers and the total number of those that rent from the state.

fixed effects. As before, our standard errors are clustered at the city level.

λ_1 is the effect of the housing reform on the wage of an individual working in the state sector in year t . λ_2 is the differential effect of the housing reform on the state versus private sector. $\lambda_1 + \lambda_2$ is the effect of the housing reform on the wages of an individual working in the private sector in year t .

Figure 4 illustrates the variation behind equation (2). It plots the wage residuals (demeaned by age, age², gender, years of educational attainment, years of education² and city fixed effects) in the private and state sectors over time. It shows that prior to the housing reform, private-sector workers earned higher wages than public sector workers. Private-sector wages declined to the same level as the state sector almost immediately following the reform and stayed at the same level afterwards.²⁷ λ_2 from equation (2) will capture the average difference (vertical gap) between the two lines before and after the housing reform.

Column (1) shows the result with the baseline controls. The number of observations is slightly smaller than for the private-sector employment estimate because of missing values in the wage variable. The coefficient for the post-reform dummy variable is close to zero, -0.031, and statistically insignificant, which implies that the reform had no effect on state-sector wages. This is consistent with the belief that state-sector wages in our context may not be set to maximize profits, and also with the fact that the state sector is very large such that the housing reform does not dramatically reduce the labor supply for the state sector. In contrast, the interaction of the post-reform dummy variable and the dummy variable of whether a person works in the private sector is -0.56 and statistically significant at the 1% level. This means the housing reform significantly reduced private-sector wages. If we sum the coefficients, $-0.56 + (-0.031) = -0.591$, the estimates imply that the housing reform reduced private-sector wages by approximately 59%.

Another way of assessing the magnitude of the effect is to estimate the standardized interaction coefficient (not in the Tables). We find that a one-standard deviation increase in the interaction of the post-housing-reform dummy variable and the dummy variable for whether an individual works in the private sector causes a 0.21 standard-deviation reduction in private-sector wages.

There could be several forces which cause wages to decline in the private sector. First, there

²⁷Recall that we can examine fewer pre-reform years for wages than for housing and employment because wage data begin to be reported later than the other variables.

is the obvious effect of the increase in private-sector labor supply. Second, the housing reform effectively reduced non-wage benefits from the state sector. If pre-reform private-sector wages paid a premium to compensate for housing, then reduction of housing benefits from state employment could reduce private-sector wages. Finally, negative selection out of the state sector could also play a role. Given that there was much uncertainty about economic reform and the infant private sector, and that many other benefits (e.g., health care) were still tied to state employment, it may be that the housing reform caused relatively unproductive workers to shift into the state sector.

It is beyond the scope of our paper to be conclusive about this point. But we can attempt to shed some light onto this question by investigating the importance of the reduction in state-provided housing benefits in driving the wage results. To do this, we impute the amount of housing subsidies for state-sector workers and add it to the wages of state-sector workers for the pre-reform period.²⁸ The results from using this alternative wage measure are shown in column (11). They are very similar to the baseline. Thus, to the extent that our imputation does not systematically understate housing benefits, the reduction in housing benefits after the housing reform seem to play a limited role in the wage analysis. Nevertheless, investigating this further and also better understanding the other possible channels through which the housing reform affects wages is an important avenue for future research.

We will discuss the additional results in Table 4 later in the section on Robustness.

5.4 Mechanisms: Untying Access or Income Effect?

Before delving into the robustness exercises, we consider the mechanisms underlying our main results. As we discussed earlier, the initial housing reform that we examine untied access to housing from state-sector employment. A second round of reforms legalized the sale of private housing and allowed households to sell or rent (i.e., monetize) their privately owned housing. To investigate whether our results reflect the untying from the first reform or potential income/wealth effects due to the second reform, we control for the introduction of the second reform, which also varied by city

²⁸To impute the housing subsidies, we use data on rent and the quality of housing (e.g., total area, total living area, amenities, city and year fixed effects) for private-sector workers that rent housing from private owners and data on the quality of housing from pre-reform state-sector workers to predict the market value of the rent for these workers. Then, we subtract the actual value from the market value to estimate the state subsidy, and add the subsidy to the earnings of pre-reform state-sector workers. These variables are reported by the UHIES and the estimates are available upon request.

and year, in our baseline. The results are shown in column (2) of Tables 4 and 6. Our main estimates on housing ownership, the sector of employment and wages are very similar to the baseline, which is re-stated in column (1) of each table. This supports our interpretation that the estimated effects of the housing reform reflect the reduction in mobility cost caused by untying housing access from state-sector employment. The lack of a significant effect of housing reforms on self employment or entrepreneurship is also consistent with the hypothesis that these reforms did little to increase household wealth and thereby relax credit constraints to engaging in self employment.

5.5 Robustness

In this section, we show that our results are not driven by potential confounding variables.

5.5.1 Placebo outcomes: Re-employment and Unemployment

The main concern for interpreting the effects of the housing reform as causal arises from the possibility that the timing of the housing reform and the outcomes of interest are outcomes of omitted factors such as poor performance in the city economy, or declining performance of the state-sector in the city (since the urban economy is mostly driven by the state-sector in this context). In that case, the estimates will confound the impact of the housing reform with the influences of these other factors. We investigate this possibility with two placebo outcomes.

The first outcome we examine is the category of “retired and re-employed workers” discussed earlier. In the UHIES, these are workers that retired because of reaching the state-employment mandatory retirement age and then become re-employed afterwards with the same or a different enterprise (which can be state- or privately owned).²⁹ While state-owned units were under political pressure to keep up employment rates for workers below the mandatory retirement age, re-employment for those who have passed the retirement age was left to the discretion of each work unit. As such, it is a reasonable indicator of the economic health of the local economy. Under our preferred interpretation, being retired and re-employed is a placebo outcome. We find that this is indeed the case: housing reforms have no effect on the probability of being retired and re-employed (Table 5, columns 1-3).

²⁹They are distinct from laid off workers, often referred to as *xia gang* workers, which we capture with the unemployment variable that is discussed next.

The second placebo outcome is the upper-bound of total unemployment (see Section 4). The sample used for this exercise is larger than the main sample used earlier because the latter includes only the working-age individuals that report some employment. Table 5, columns (4)-(6) show that the reform had no effect on this broad measure of unemployment. This is consistent with our preferred interpretation that the main effect of housing reforms was to induce workers to enter the private sector.

We also computed unemployment rates after excluding those who are no longer in the labor force, along the same lines as Feng et al. (2015). Housing reforms do not have an impact on this more traditional measure of unemployment (results available upon request). We prefer our broader measure since it accounts for any potential misclassification of whether someone is in the labor force, and because exiting the labor force might also be a response to other policy changes such as SOE restructuring.

5.5.2 Other Household Members

The results presented thus far reflect the employment status of the household head. We find similar effects for spouses and for all adult members of the household. For the latter, we use the number of household members working in a given sector as our dependent variable (see Appendix Table A.2).

5.5.3 Additional Controls

The remaining robustness exercises test the sensitivity of our results to additional controls. We focus on the statistically significant results: private ownership of housing, employment by a private enterprise and wages.

State-Sector Restructuring

As we described earlier in Section 2.2, the main alternative reform that could confound our results is SOE restructuring, which caused mass privatization and closing of state-sector firms. One may be concerned that this reform pushed workers into the private sector, which led to a decline in private-sector wages and an increase in private housing ownership. To address this issue, we use data on the number of state-owned firms in the manufacturing sector from *China's City Statistical Year-*

books for 1988-1998, and from the *Annual Survey of Above-scale Industrial Firms Panel* (ASIFP) (1998-2005).³⁰ First, to examine whether the housing reform coincided with SOE restructuring, we regress the city-year-level share of state-owned manufacturing firms on the housing reform dummy, controlling for city and year fixed effects. We define the share as the number of state-owned manufacturing firms divided by the total number of manufacturing firms, which is a reasonable proxy for enterprise restructuring since the latter reform triggered the closure of numerous state-owned firms and the privatization of many others.³¹ The coefficient is 0.0083 and statistically insignificant (not shown in tables). This goes against the concern that the two reforms are confounded.

Another way to address this concern is to control for the fraction of SOEs in our baseline specification. Tables 4 and 6 column (3) show that our results are robust to the inclusion of this control. These results go against the concern that the main findings are confounded with SOE restructuring.³²

Macroeconomic Trends

Next, we individually control for variables that are correlated with regional economic performance, and in particular, the performance of the state and private sectors in each region, by including province-level GDP growth rates and province-level state-sector employment growth rates as additional regressors. Tables 4 and 6 columns (3)-(4) show that neither of these additional controls changes the main coefficient on the impact of urban housing reform.

City-level Trends

³⁰These two datasets are very comparable. The latter dataset is often referred to as “the Census of Manufacturing Firms”. It contains all state-owned manufacturing firms and privately owned manufacturing firms with revenues that exceed five million RMB. The correlation between the two sources for the only common year, 1998, is 0.997. Year effects control for the fact that the sampling method for the data may change year to year because of the change in datasets.

³¹The total number of manufacturing firms in each city is reported in the City Statistical Yearbooks (1986-2005). This measure will not capture the reform to the extent that it shifted employment or revenues. We can alternatively define the share of state-owned firms using a revenue-based definition. The results are very similar and available upon request.

³²Our results are similarly robust if we control for the share of manufacturing output from state-owned firms in a given city and year. The results are available upon request.

Since the timing of housing reform was partly determined by the city’s fiscal needs, we would like to control for city government revenues. However, the implementation of housing reforms could, in turn, have a direct effect on city government income (e.g. city governments are responsible for funding housing subsidies and are the recipients of the money paid for housing purchases). Thus, we instead control for pre-reform city government revenues (averaged over 1986-88), interacted with the full set of year dummy variables. Similarly, coastal cities grew significantly faster than interior regions during the period of our study. We address the possibility of this differential growth pattern by including controls for a dummy variable for if the city is in a coastal province interacted with a full set of year dummies (note that the time-invariant effects of being coastal are already captured by the city fixed effects). Tables 4 and 6 columns (6)-(7) show that the effects of the housing reform are not confounded by any of these other characteristics.

Sector-specific Employment Pre-trends

Another way to address the concern that the housing reform was introduced earlier in cities that were experiencing faster private-sector growth or more state-sector slowdown is to directly control for pre-reform private- and state-sector employment growth rates, each interacted with a full set of year fixed effects. Tables 4 and 6 columns (8)-(9) show that our results are very robust to the inclusion of these rigorous controls.

Finally, in column (10) of Tables 4 and 6, we control for all of these variables simultaneously. The estimates are very similar to the baseline in magnitude and precision. The results in this section suggest that our main results are unlikely to be driven by omitted variables.

6 Scaling the Reduced Form Effect with 2SLS

The main results show that the housing reform had economically important effects on private housing ownership, private-sector labor supply and private-sector wages. Another way to assess the economic significance of the housing reform is to use the results on private-sector labor supply and wages to conduct a scaling exercise. We can ask: what is the effect of the change in private-sector labor supply that is caused by the housing reform on private-sector wages? To examine this, we

estimate the following instrumented second-stage equation:

$$\ln w_{ijt}^k = \pi_1 \ln L^p_{jt} + \pi_2 [\ln L^p_{jt} \times P_{ijt}] + X'_{jt} \gamma_k + Z'_{ijt} \zeta_l + \gamma_j + \delta_t + \varepsilon_{ijt}, \quad (3)$$

where $\ln w_{ijt}^k$, the log wage of individual i living in city j during year t and who works in sector k (private, state) is a function of the log of the total number of workers in the private sector, $\ln L^p_{jt}$; the interaction between the log of the number workers in the private sector and a dummy variable for whether individual i works in the private sector, P_{ijt} . We control for a vector of individual controls such as the sex, age and years of education, Z_{ijt} ; a vector of time-varying city characteristics that we introduce later in the robustness checks, X_{jt} ; city and year fixed effects. As before, our standard errors are clustered at the city level.³³ The main effect of whether an individual works in the private sector is also included in the vector of individual controls.

The private-sector labor supply (measured using our UHIES sample), $\ln L^p_{jt}$, is instrumented by the introduction of the housing reform; and the interaction variable, $\ln L^p_{jt} \times P_{ijt}$, is instrumented by the interaction of the introduction of the housing reform and a dummy for whether an individual works in the private sector. $\hat{\pi}_1$ is the estimated average effect of a change of private-sector labor supply on the wages of non-private-sector workers. $\hat{\pi}_1 + \hat{\pi}_2$ is the estimated effect of a change in private-sector wages due to a change in private-sector labor supply (the inverse of the labor-demand elasticity). As such, we expect $\hat{\pi}_1 + \hat{\pi}_2$ to be negative. Since state-sector wages may not equal the marginal product of labor, we do not have any *a priori* expectation on the sign of the coefficient π_1 , which represents the impact of private-sector labor supply on state-sector wages.

This scaling exercise requires several assumptions. First, since we are using cross-city variation in labor supply, the implicit assumption is that these cities are independent labor markets. For the context of our study, where the reforms are mostly being introduced in the late 1980s and early 1990s, this assumption is plausible since migration between cities was tightly restricted by policy. Based on statistics from the census, cross-city migrants were only 1.37 percent of the population in 1990 and 6.3 percent in 2000. Second, inter-city trade can be a source of factor price equalization across cities even in the absence of worker migration. However, cross-province trade in China

³³Note that we are unable to implement wild bootstraps for the 2SLS standard errors. However, most of the results on housing and employment status show that p-values are similar between the bootstrapped and non-bootstrapped methods. Thus, we believe that the standard errors are unlikely to be greatly attenuated by the small number of clusters.

was inhibited by the presence of inter-province tariff and non-tariff barriers throughout the 1990s (Poncet, 2005). Third, long-run effects can be very different as capital or technology adjusts to these changes in labor supply. Our estimates should be interpreted as short-to-medium-run effects.

Note that the goal of this 2SLS estimate is to scale the effect of the housing reform – it asks: how much does the reform-induced variation in private-sector labor supply affect wages? It cannot identify the causal effect of a change in private-sector labor supply on wages. This is important to keep in mind because the latter exercise requires that the instruments not only be correlated with the endogenous second stage variables, but also fulfill the exclusion restriction that they only affect wages through the instrumented endogenous variables.³⁴

The 2SLS estimates are presented in Table 7.³⁵ The coefficient for the uninteracted labor supply term shows that increasing the private-sector labor force has a small and statistically insignificant impact on wages in the state sector (estimates of $\hat{\pi}_1$ in column 1). The interaction term (estimate of $\hat{\pi}_2$ in column 1) shows that there is a much larger, negative and statistically significant effect on wages in the private sector. It is -0.204 and statistically significant at the 5% level. The sum of the uninteracted and interacted coefficients, $\hat{\pi}_1 + \hat{\pi}_2$, is -0.296 (see the bottom of the Table). This implies that if the housing reform doubles the private-sector labor supply, private-sector wages will decline by approximately 29.6%. Thus, viewed through the channel of labor supply, the housing reform was likely to have had an economically important effect on private-sector wages.

To compare our estimates to those from other contexts, we calculate the implied labor demand elasticity, $\frac{1}{\hat{\pi}_1 + \hat{\pi}_2}$, which is shown at the bottom of the table. It shows that increasing private-sector labor supply by 1% reduces private-sector wages by 0.30%, implying a private-sector labor demand elasticity of -3.38. This is larger than several other well-known estimates from the literature. For example, Acemoglu et al. (2004) estimate the female labor demand elasticity in the United States to be -1.2 to -1.5, while Addison et al. (2008) estimate labor demand elasticity for unskilled and

³⁴For example, the decision to work in the private sector is a choice variable, which means that one of the instruments, the interaction of the housing reform and whether an individual works in the private sector, is partly based on an endogenous variable. As we discussed earlier in Section 5.3, negative selection into the private sector could be one of the forces behind the reduced from effect of the housing reform on private-sector wages. A related concern is that the housing reform may affect private-sector wages through channels other than labor supply. If this were the case, then our 2SLS coefficient could overstate the negative effect (if wages would decline less when there are no omitted variables) or understate the negative effect (if wages would decline more when there are no omitted variables). Since the elasticity is the inverse of the coefficient, the former means that the implied elasticity for labor demand is a lower bound, and the latter means that it would be an upper bound.

³⁵The first stage estimates are shown in Appendix Table A.3.

skilled workers in Germany to be -0.65 and -0.69 respectively.³⁶

Interestingly, if we assume a Cobb-Douglas production function, our estimates indicate that the implied labor share in the private sector is approximately 0.76 to 0.82. The fact that this is higher than the standard assumption of 0.66 (i.e., two-thirds) is consistent with the belief that private-sector firms in China were under-capitalized during the early and mid-reform era.

In columns (2)-(10), we gradually add controls for macroeconomic conditions that were used earlier in Tables 4 and 6. The estimates are very robust.

Column (11) shows the 2SLS estimates with the state-sector wage correction discussed earlier in Section 5.3.

Column (12) conducts a second adjustment. We recalculate the private-sector labor supply to include migrant workers, with the assumption that all of these migrant workers are employed by the private sector. We obtained independent city-level estimates of the fraction of migrants in the labor force from the 1% sample of the 1990 and 2000 censuses and the 20% sample of the 1% population survey of 2005, interpolated these for the intervening years, and used these to construct alternative estimates of the private-sector labor supply in each city.³⁷ The best approximation of migration during this period suggests that rural migrants increased from approximately 30 million in 1989 to 113 million in 2005 (Zhan, 2005). In our data, the share of migrants in the total urban workforce increased from about 9% in 1990 to about 23% in 2005. Column (12) shows that the 2SLS estimates using this adjusted private-sector labor supply variable are very close to the baseline in column (1). Thus, we believe that our estimates are unlikely to be biased by the exclusion of illegal migrants. Note that the reduced form estimates for this specification are the same as those reported in Table 4 column (1).

7 Conclusion

The goal of this study is to make progress on two questions. What are effective policies for gradually transitioning labor into the private sector? How did untying access to housing from state-sector employment affect the privatization of housing, labor and wages in the infant private sector? Using

³⁶Our 2SLS estimates is similar in spirit to Acemoglu et al. (2004), which uses variations in female labor supply across U.S. states to estimate the elasticity of demand for female workers. Since their sample is restricted to the employed population and being in the labor force can be a choice, we face similar selection issues as their study.

³⁷See the Data Appendix for a detailed description.

the staggered timing of the Chinese urban housing reform, we show that untying housing benefits from state-sector employment accounts for 30% of the increase in private-sector employment in Chinese cities during 1986-2005, which in turn, reduced wages in the private sector by over 50%.

The estimates should be interpreted cautiously as specific to the context of our study. Relative to other transitioning economies, China experienced little political and social upheaval and steady economic growth. Nevertheless, the insights that untying important benefits, such as housing, from state-sector employment is an effective policy for gradually releasing workers from the state sector and this could lower wages in the infant private-sector are likely to be generalizable.

Most past empirical studies of economic transition have been descriptive or focused on cross-country comparisons. Our study provides an example of how policy experiments and newly available micro-data from transitioning economies can be used to provide better identified estimates and enhance our understanding of the transition process.

A worthwhile endeavor for future research is to better understand the mechanisms. Our results are able to shed some light by showing that the results we estimate are most likely to be driven by the untying of access from state-sector employment rather than income or wealth effects from the privatization of housing (which is likely to have played a more important role in later years when housing was more easily sold, rented or collateralized). However, there are many other interesting questions that still need to be addressed. For example, as we discussed in the paper, it is important to understand the extent to which the large reduction in private-sector wages was driven by the influx of labor to the infant private sector, which is widely believed to be under capitalized; versus the extent to which this initial reduction in wages was caused by other forces, such as negative selection out of the public sector.

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Table 1: Descriptive Statistics

	Full Sample			Pre-Reform			Post-Reform		
	Obs. (1)	Mean (2)	Std. Dev. (3)	Obs. (4)	Mean (5)	Std. Dev. (6)	Obs. (7)	Mean (8)	Std. Dev. (9)
	A. Characteristics of the Household								
Own Housing	107,480	0.70	0.46	21,796	0.11	0.31	85,684	0.73	0.44
Rent from State	107,480	0.26	0.44	21,796	0.86	0.34	85,684	0.23	0.42
	B. Characteristics of the Household Head								
Male	107,480	0.60	0.49	21,796	0.62	0.49	85,684	0.60	0.49
Age	107,480	49.53	11.14	21,796	44.61	11.44	85,684	49.76	11.07
Years of Education	107,480	11.65	2.83	21,796	10.71	3.02	85,684	11.69	2.81
Unemployed	107,480	0.17	0.37	21,796	0.07	0.26	85,684	0.17	0.38
<i>All households where the head reports a non-missing value for sector of employment</i>									
Employed by Private Enterprise	77,781	0.05	0.21	18,813	0.00	0.05	58,968	0.05	0.22
Self-Employed or Small Enterprise Owner	77,781	0.04	0.20	18,813	0.00	0.05	58,968	0.04	0.20
State Employee	77,781	0.81	0.39	18,813	0.97	0.18	58,968	0.80	0.40
Retired and Re-employed	77,781	0.07	0.26	18,813	0.01	0.11	58,968	0.08	0.26
Log Wages (Labor Earnings)	69,414	9.30	0.77	10,446	8.47	0.46	58,968	9.33	0.77

Notes: Data are from the Urban Household Income and Expenditure Surveys. Variables for housing and employment status are available for 1986-2005. Wage variables are available for 1989-2005.

Table 2: The Effect of the Housing Reform on Private Housing Ownership

Dependent Variable: Dummy Variable for Own Housing			
	(1)	(2)	(3)
Post Reform	0.068 (0.0783)	0.067 (0.0824)	0.075 (0.0600)
Controls			
City FE, Year FE, Household Head's Age, Sex, and Years of Education	Y	Y	Y
Household Head's Age ² , Years of Education ²	N	Y	N
Household Heads Age * Year FE, Years of Education * Year FE	N	N	Y
Observations	107,480	107,480	107,480
R-squared	0.220	0.220	0.223
Wild bootstrap	(0.094)	(0.102)	(0.070)

Notes: The standard errors are clustered at the city level. P-values are presented in parentheses. Wild bootstrapped p-values are presented at the bottom of the table. The sample is at the household level (1986-2005) .

Table 3: The Effect of the Housing Reform on Private-Sector Employment

	Dependent Variables: Employment Status Dummy Variables					
	Employed by Private			Self-Employed		
	(1)	(2)	(3)	(4)	(5)	(6)
Post Reform	0.020 (0.0259)	0.021 (0.0246)	0.018 (0.0364)	0.009 (0.191)	0.009 (0.187)	0.005 (0.485)
Controls						
City FE, Year FE, Age, Sex, and Years of Education	Y	Y	Y	Y	Y	Y
Age ² , Years of Education ²	N	Y	N	N	Y	N
Age * Year FE, Years of Education * Year FE	N	N	Y	N	N	Y
Observations	77,781	77,781	77,781	77,781	77,781	77,781
R-squared	0.041	0.041	0.045	0.047	0.047	0.054
Wild bootstrap	(0.040)	(0.059)	(0.059)	(0.178)	(0.238)	(0.495)

Notes: The standard errors are clustered at the city level. P-values are presented in parentheses. Wild bootstrapped p-values are presented at the bottom of the table. The sample is restricted to household heads who report a non-missing value for employment status (1986-2005).

Table 4: The Effect of the Housing Reform on Wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Dependent Variable: Log(real wage)										
	Baseline										
Post Reform * Private Sector	-0.560 (0.000)	-0.574 (0.000)	-0.557 (0.000)	-0.558 (0.000)	-0.570 (0.000)	-0.605 (0.000)	-0.591 (0.000)	-0.555 (0.000)	-0.564 (0.000)	-0.590 (0.000)	-0.555 (0.000)
<i>Wild bootstrap</i>	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.004)
Post Reform	-0.031 (0.508)	-0.054 (0.242)	-0.033 (0.488)	-0.030 (0.520)	-0.033 (0.487)	-0.015 (0.703)	-0.021 (0.619)	-0.035 (0.473)	-0.034 (0.473)	-0.048 (0.277)	-0.035 (0.462)
<i>Wild bootstrap</i>	(0.588)	(0.330)	(0.564)	(0.602)	(0.574)	(0.740)	(0.678)	(0.552)	(0.548)	(0.408)	(0.524)
Observations	69,414	69,414	69,119	69,414	68,538	69,414	69,414	69,414	69,414	68,334	69,414
R-squared	0.371	0.372	0.372	0.371	0.375	0.372	0.372	0.372	0.372	0.378	0.397
Controls											
Post Second Housing Reform	N	Y	N	N	N	N	N	N	N	Y	N
Fraction of state-owned firms in city	N	N	Y	N	N	N	N	N	N	Y	N
Province GDP growth	N	N	N	Y	N	N	N	N	N	Y	N
Province state-sector employment growth	N	N	N	N	Y	N	N	N	N	Y	N
Pre-reform city income * year FE	N	N	N	N	N	Y	N	N	N	Y	N
Coastal province * year FE	N	N	N	N	N	N	Y	N	N	Y	N
Pre-reform private-sector employment growth * Year FI	N	N	N	N	N	N	N	Y	N	Y	N
Pre-reform state-sector employment growth * Year FE	N	N	N	N	N	N	N	N	Y	Y	N

Notes: All regressions control for age, sex and years of education, the uninteracted dummy for whether an individual works in the private sector, city and year fixed effects. The standard errors are clustered at the city-level. P-values are reported in parentheses. Wild bootstrapped p-values are presented in italics in parentheses. The sample is restricted to household heads who report a non-missing value for employment status and for whom labor income data is available (1989-2005). The number of observations varies across specifications due to missing variables in the additional controls.

Table 5: The Effect of the Housing Reform on Re-employment and Unemployment

	Dependent Variables: Employment Status Dummy Variables					
	Retired and Re-Employed			Unemployed		
	(1)	(2)	(3)	(4)	(5)	(6)
Post Reform	0.010 (0.379)	0.013 (0.204)	0.006 (0.559)	0.012 (0.455)	0.004 (0.758)	0.008 (0.590)
Controls						
City FE, Year FE, Age, Sex, and Years of Education	Y	Y	Y	Y	Y	Y
Age ² , Years of Education ²	N	Y	N	N	Y	N
Age * Year FE, Years of Education * Year FE	N	N	Y	N	N	Y
Observations	77,781	77,781	77,781	107,480	107,480	107,480
R-squared	0.209	0.275	0.223	0.082	0.121	0.085
Wild bootstrap	(0.455)	(0.297)	(0.594)	(0.337)	(0.832)	(0.535)

Notes: The standard errors are clustered at the city level. P-values are presented in parentheses. Wild bootstrapped p-values are presented at the bottom of the table. The samples in columns (1)-(3) are restricted to household heads who report a non-missing value for employment status (1986-2005). The samples in columns (4)-(6) include all household heads.

Table 6: The Effect of the Housing Reform on Private Housing Ownership and Private-Sector Employment – Robustness to Additional Controls

	Dependent Variable									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Baseline										
A. Dummy Variable for Own Housing										
Post Reform	0.068 (0.0783)	0.070 (0.0804)	0.064 (0.0957)	0.069 (0.0753)	0.066 (0.0869)	0.070 (0.0748)	0.070 (0.0803)	0.064 (0.1216)	0.070 (0.0595)	0.068 (0.1105)
Observations	107,480	107,480	107,071	107,480	106,122	107,480	107,480	107,480	107,480	105,813
R-squared	0.2197	0.2197	0.2198	0.2197	0.2207	0.2208	0.2200	0.2204	0.2200	0.2232
Wild bootstrap	(0.094)	(0.096)	(0.116)	(0.094)	(0.104)	(0.112)	(0.092)	(0.178)	(0.078)	(0.184)
B. Employed by a Private Enterprise Dummy Variable										
Post Reform	0.020 (0.0259)	0.020 (0.0310)	0.017 (0.0457)	0.020 (0.0323)	0.022 (0.0204)	0.018 (0.0830)	0.023 (0.0154)	0.019 (0.0378)	0.020 (0.0399)	0.018 (0.0416)
Observations	77,781	77,781	77,486	77,781	76,905	77,781	77,781	77,781	77,781	76,701
R-squared	0.0408	0.0408	0.0414	0.0409	0.0417	0.0418	0.0415	0.0412	0.0410	0.0453
Wild bootstrap	(0.040)	(0.040)	(0.058)	(0.028)	(0.020)	(0.096)	(0.026)	(0.040)	(0.020)	(0.068)
Controls										
Fraction of state-owned firms in city	N	N	Y	N	N	N	N	N	N	Y
Post Second Housing Reform	N	Y	N	N	N	N	N	N	N	Y
Provincial GDP growth	N	N	N	Y	N	N	N	N	N	Y
Province state-sector employment growth	N	N	N	N	Y	N	N	N	N	Y
Pre-reform city income * Year FE	N	N	N	N	N	Y	N	N	N	Y
Coastal province * Year FE	N	N	N	N	N	N	Y	N	N	Y
Pre-reform private-sector employment growth * Year FE	N	N	N	N	N	N	N	Y	N	Y
Pre-reform state-sector employment growth * Year FE	N	N	N	N	N	N	N	N	Y	Y

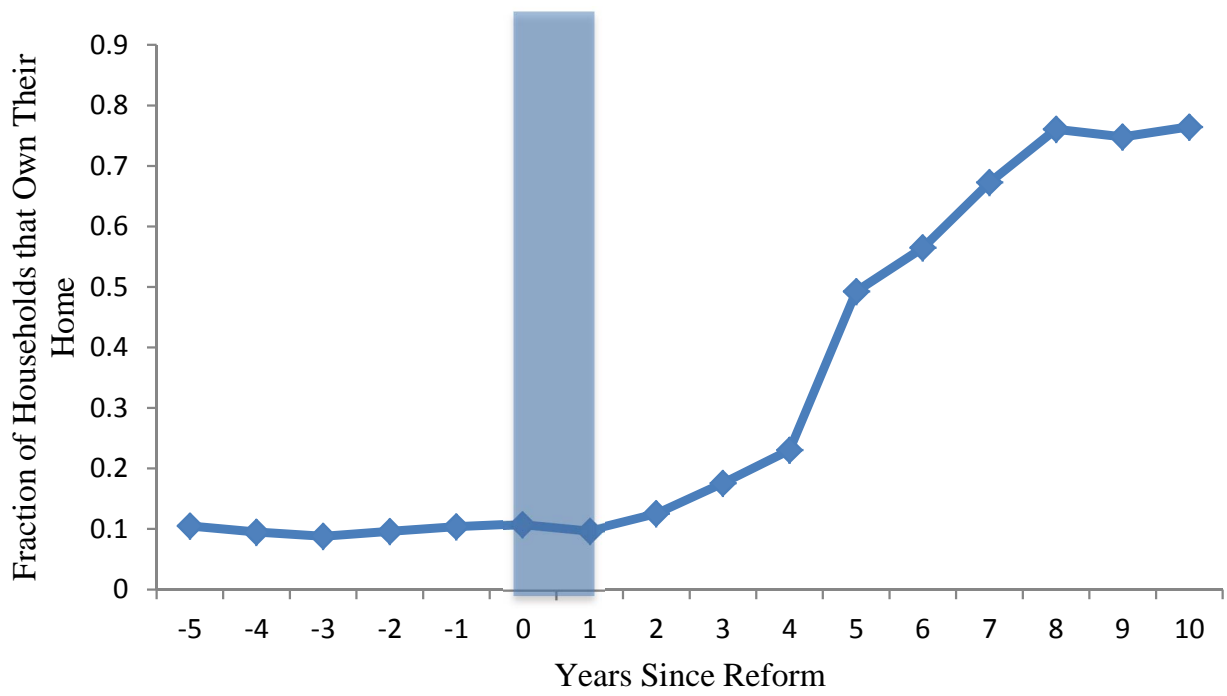
Notes: All regressions control for age, sex and years of education, city and year fixed effects. The standard errors are clustered at the city-level. P-values are reported in parentheses. Wild bootstrapped p-values are presented at the bottom of each table. The sample is restricted to household heads who report a non-missing value for employment status (1986-2005). The number of observations varies across specifications due to missing variables in the additional controls.

Table 7: The 2SLS Effect of Labor Supply on Wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Dependent Variable: Log(real wage)											
	Baseline											
Log (private sector labor supply) *	-0.204	-0.236	-0.201	-0.216	-0.182	-0.189	-0.207	-0.206	-0.205	-0.206	-0.207	-0.209
Private sector (π_2)	(0.011)	(0.018)	(0.009)	(0.019)	(0.000)	(0.002)	(0.017)	(0.012)	(0.011)	(0.002)	(0.012)	(0.025)
Log (private sector labor supply)	-0.092	-0.139	-0.089	-0.108	-0.047	-0.065	-0.096	-0.097	-0.096	-0.107	-0.100	-0.113
(π_1)	(0.320)	(0.203)	(0.330)	(0.330)	(0.523)	(0.399)	(0.301)	(0.322)	(0.316)	(0.212)	(0.297)	(0.346)
Private sector dummy	1.259	1.486	1.242	1.334	1.110	1.161	1.283	1.278	1.269	1.269	1.288	1.330
	(0.025)	(0.034)	(0.022)	(0.039)	(0.002)	(0.006)	(0.034)	(0.025)	(0.025)	(0.004)	(0.025)	(0.047)
Observations	69,414	69,414	69,414	68,538	69,414	69,414	69,414	69,414	69,119	68,334	69,414	69,414
R-squared	0.363	0.358	0.364	0.365	0.367	0.366	0.363	0.364	0.364	0.370	0.388	0.362
Controls												
Post Second Housing Reform	N	Y	N	N	N	N	N	N	N	Y	N	N
Province GDP growth	N	N	Y	N	N	N	N	N	N	Y	N	N
Province state-sector employment growth	N	N	N	Y	N	N	N	N	N	Y	N	N
Pre-reform city income * year FE	N	N	N	N	Y	N	N	N	N	Y	N	N
Coastal province * year FE	N	N	N	N	N	Y	N	N	N	Y	N	N
Pre-reform private-sector employment growth * Year FE	N	N	N	N	N	N	Y	N	N	Y	N	N
Pre-reform state-sector employment growth * Year FE	N	N	N	N	N	N	N	Y	N	Y	N	N
Fraction of state-owned firms in city	N	N	N	N	N	N	N	N	Y	Y	N	N
$\pi_1 + \pi_2$ (estimate)	-0.296	-0.375	-0.290	-0.324	-0.229	-0.255	-0.303	-0.303	-0.301	-0.314	-0.307	-0.322
$\pi_1 + \pi_2 = 0$ (p-value)	(0.072)	(0.062)	(0.069)	(0.097)	(0.048)	(0.048)	(0.079)	(0.077)	(0.073)	(0.027)	(0.070)	(0.117)
Labor demand elasticity $1/(\pi_1 + \pi_2)$	-3.38	-2.67	-3.45	-3.09	-4.37	-3.92	-3.30	-3.30	-3.32	-3.18	-3.26	-3.11

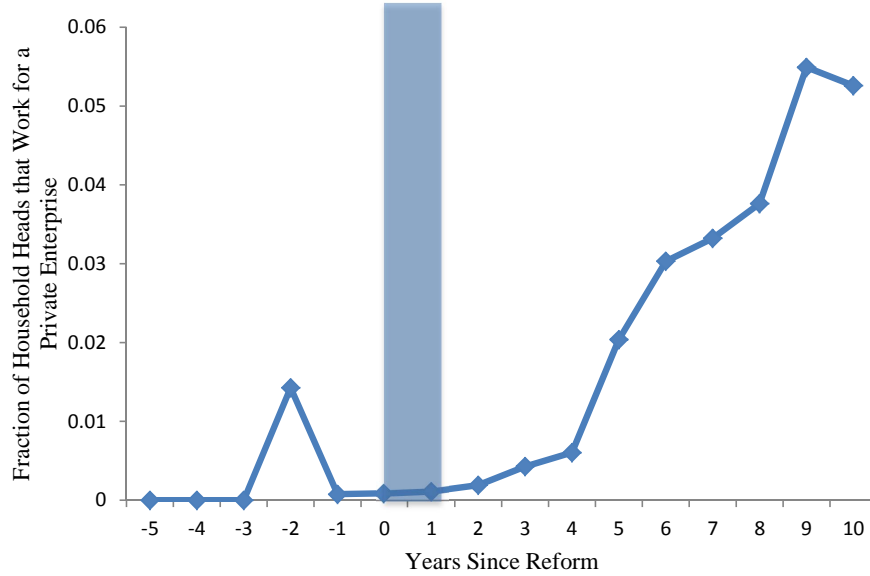
Notes: All regressions control for age, sex, years of education, the uninteracted dummy for whether an individual works in the private sector, and city and year fixed effects. The standard errors are clustered at the city-level. P-values are reported in parentheses. The sample is restricted to household heads who report a non-missing value for employment status (1989-2005). The number of observations varies across specifications due to missing variables in the additional controls.

Figure 1: Private Housing Ownership



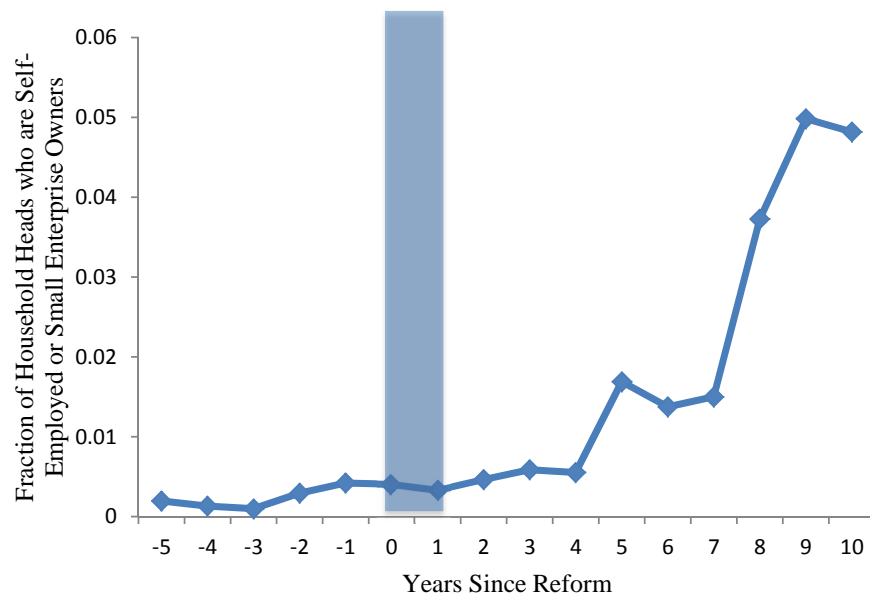
Notes: The data are reported by the UHIES, 1986-2005. The sample comprises of households from 27 cities.

Figure 2: Employed by a Private Enterprise



Notes: The data are reported by the UHIES, 1986-2005. The sample comprises of households from 27 cities.

Figure 3: Self-Employed or Small Enterprise Owners



Notes: The data are reported by the UHIES, 1986-2005. The sample comprises of households from 27 cities.

Figure 4: Private and State Sector Wage Residuals



Notes: The data are reported by the UHIES, 1989-2005. The sample comprises of households from 27 cities. Wage residuals are demeaned by age, age², gender, years of educational attainment, years of education² and city fixed effects.

Data Appendix

Migration Correction We identify “migrant” as those with rural hukou, who live in the city and are in the labor force. There are slight differences across the three years in the way this question was asked. The 1990 census provides information on whether individuals are living in their original hukou registration place or not and if not, for how long they have been living in the current location. The choices for the latter question are: (i) permanently living in the hukou location; (ii) living in the current county/city for more than one year but hukou is in other county/city; (iii) living in the current county/city for less than one year but have left the hukou location for more than one year; (iv) living in the current county/city but hukou location is uncertain; and (v) living abroad. Based on this question we define “rural migrants” in the 1990 census data as those who are in the labor force and hold an agricultural hukou but have lived for over one year in an urban area (city) which differs from their hukou location. The 2000 census added another choice, namely (vi) live in current location for more than 6 months but hukou is in other county/city, so we included this group as well. The 2005 population survey asked two questions: (i) Is your hukou in the current community, other community within the city, or other county/city? (ii) How long have you been away from your hukou registration place? The choice for the second question ranges from less than half year to over six years. We choose those who reside in cities and left hukou place for more than six months to be consistent with our definition for the 2000 census.

Table A.1: The Timing of the Reform

Year of reform	# cities
1988	3
1989	1
1990	2
1991	2
1992	10
1993	6
1994	1
1995	1
1997	1
Total	27

Notes: The data are collected by the authors from province-level newspapers.

Table A.2: The Effect of the Housing Reform on Private-Sector Employment and Unemployment – All Household Members

	Dependent Variable: Employment Status			
	Employed by Private Enterprise (1)	Self-Employed (2)	Retired and re- employed (3)	Unemployed (4)
A. Dummy Variable for the Employment Status of the Spouse				
Post Reform	0.027 (0.00623)	0.009 (0.117)	0.004 (0.767)	-0.015 (0.423)
Observations	67,029	67,029	67,029	98,169
R-squared	0.059	0.040	0.186	0.090
Wild bootstrap	(0.000)	(0.158)	(0.614)	(0.277)
B. The Number of Adult Household Members Working				
Post Reform	0.058 (0.003)	0.014 (0.172)	0.024 (0.299)	0.035 (0.379)
Observations	91,991	91,991	91,991	108,383
R-squared	0.073	0.046	0.102	0.053
Wild bootstrap	(0.000)	(0.099)	(0.357)	(0.436)

Notes: The standard errors are clustered at the city level. P-values are presented in parentheses. Wild bootstrapped p-values are presented at the bottom of each panel. The sample in panel A, columns (1)-(3) are restricted to spouses who report a non-missing value for employment status (1986-2005); column (4) includes all spouses. The sample in panel B, columns (1)-(3) includes all households where at least one member reports a non-missing sector of employment; column (4) includes all households.

Table A.3: First Stage Regressions of the Effect of the Housing Reform on Private-Sector Labor Supply

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Dependent Variable											
	A. Log (private sector labor supply) * work in private sector											
Baseline	Add Imputed Housing Value to State-sector Wages											
	Adjust Private-Sector Labor Supply for Migrants											
Post Reform * Private Sector	3.882 (0.000)	3.880 (0.000)	3.883 (0.000)	3.867 (0.000)	3.865 (0.000)	3.890 (0.000)	3.880 (0.000)	3.884 (0.000)	3.896 (0.000)	3.864 (0.000)	3.882 (0.000)	4.016 (0.000)
Post Reform	-0.057 (0.101)	-0.060 (0.090)	-0.056 (0.102)	-0.055 (0.109)	-0.032 (0.308)	-0.058 (0.112)	-0.057 (0.113)	-0.055 (0.057)	-0.061 (0.075)	-0.034 (0.165)	-0.057 (0.101)	-0.067 (0.064)
Observations	69,414	69,414	69,414	68,538	69,414	69,414	69,414	69,414	69,119	68,334	69,414	69,414
R-squared	0.979	0.979	0.979	0.978	0.979	0.979	0.979	0.979	0.979	0.979	0.979	0.979
F-statistic	28.09	27.81	28.00	28.22	26.11	27.88	29.56	31.14	28.55	27.00	28.09	30.09
	B. Log (private sector labor supply)											
Post Reform * Private Sector	-2.486 (0.000)	-2.470 (0.000)	-2.485 (0.000)	-2.441 (0.000)	-2.053 (0.000)	-2.225 (0.000)	-2.573 (0.000)	-2.447 (0.000)	-2.503 (0.000)	-1.914 (0.000)	-2.486 (0.000)	-2.465 (0.000)
Post Reform	0.464 (0.200)	0.489 (0.192)	0.465 (0.198)	0.418 (0.247)	0.436 (0.221)	0.493 (0.151)	0.483 (0.163)	0.472 (0.145)	0.475 (0.182)	0.512 (0.083)	0.464 (0.200)	0.403 (0.237)
Observations	69,414	69,414	69,414	68,538	69,414	69,414	69,414	69,414	69,119	68,334	69,414	69,414
R-squared	0.951	0.951	0.951	0.953	0.953	0.953	0.954	0.953	0.951	0.963	0.951	0.952
F-statistic	11.08	11.13	10.99	10.82	9.21	13.68	12.06	10.77	10.75	11.26	11.08	10.94
Controls												
Post Second Housing Reform	N	Y	N	N	N	N	N	N	N	Y	N	N
Province GDP growth	N	N	Y	N	N	N	N	N	N	Y	N	N
Province state-sector employment growth	N	N	N	Y	N	N	N	N	N	Y	N	N
Pre-reform city income * year FE	N	N	N	N	Y	N	N	N	N	Y	N	N
Coastal province * year FE	N	N	N	N	N	Y	N	N	N	Y	N	N
Pre-reform private-sector employment growth * Year FE	N	N	N	N	N	N	Y	N	N	Y	N	N
Pre-reform state-sector employment growth * Year FE	N	N	N	N	N	N	N	Y	N	Y	N	N
Fraction of state-owned firms in city	N	N	N	N	N	N	N	N	Y	Y	N	N

Notes: All regressions control for age, sex and years of education, the uninteracted dummy for whether an individual works in the private sector, city and year fixed effects. The standard errors are clustered at the city-level. P-values are reported in parentheses. The sample is restricted to household heads who report a non-missing value for employment status and for whom labor income data is available (1989-2005). The number of observations varies across specifications due to missing variables in the additional controls.