Housing Booms and Shirking*

Quanlin Gu†, Jia He‡, Wenlan Qian§

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† Guanghua School of Management, Peking University, Beijing 100871, P.R. China (email:linng@vip.sina.com)

‡ School of Finance, Nankai University, Tongyan Road No.38, Haihe Education Park, 300350 Tianjin, P.R. China (email: hejia@nankai.edu.com).

§ Department of Finance, Business School, National University of Singapore, Mochtar Riady Building, 15 Kent Ridge Drive, BIZ1 07-67, Singapore 119245 (email: wenlan.qian@nus.edu.sg).

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Abstract

Using a unique credit card dataset obtained from a leading Chinese commercial bank with 10% credit card market share, we study the impact of house price increase on individual shirking behavior at work. We use the type and actual *time stamps* of 9.3 million credit card transactions by over 200,000 card holders to detect non-work-related transactions during work hours. After positive shocks to house prices, employees in the "shocked" cities experienced an immediate and permanent increase (by 8% per month) in their propensity to use work hours to attend to personal needs. The treatment group did not increase their overall credit card use in the post-shock period, and we find no effect in the neighboring, unaffected cities or among the non-working population in the "shocked" cities. The post-shock response is driven by homeowners, with an even greater impact among owners with a higher housing wealth (i.e., those with multiple homes). Consistent with increased shirking and lower productivity interpretations, further analyses find no evidence of the treatment group working harder at other hours of workdays. The increase in work-hour non-work activity concentrates in early and near-lunch hours, and on days near the end of the work week. In addition, the response is more pronounced among employees with lower work incentives—older workers in state-owned enterprises. Overall, findings in this paper offer novel insight into the real effect of house price increase through its influence on work effort choices—our estimate implies an elasticity of shirking propensity with respect to house price of 1.6.

JEL Classification: D12, D14, D91, E21, H31, R3

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1. Introduction

Many countries have experienced large and lasting housing booms during the last two decades. There is an active discussion both in the academic literature and within policy circles on the aggregate implications of housing booms, especially after the financial crisis when many housing markets dived into a long, severe bust period. Much of the research focuses on the real consequences of consumption and investment, with a growing line of work that studies the influence of housing booms on labor market dynamics. A thriving housing market likely steers individual's educational and work choice, leading to both labor allocation and productivity implications for the aggregate economy.

In this paper, we study the labor supply response to house price increases. Rather than studying the lumpy labor market participation or occupational decisions, we focus on the more continuous choice of labor supply—in particular work effort decisions. Shirking behavior, when employees exert less effort and spend unproductive time on non-work activities, is prevalent at work place. A survey conducted by salary.com in 2014 finds that 90% of American employees wasted time during work hours and close to 70% spent at least one hour unproductively every day.[1] The same survey estimates the cost to employers in the range of several hundred billion dollars annually. Rising house prices potentially change the tradeoffs of effort choice in several ways. With house price increases, homeowners benefit from a large windfall of (housing) wealth, which increases both the appeal of leisure and the opportunity cost of effort. In addition, a booming housing market tends to increase labor demand. More and potentially better employment opportunities become available, which also encourages shirking due to the reduced cost. A decrease in effort results in lower labor productivity and has direct bearing on the aggregate economic growth.

Despite its importance, this research question has received little academic attention likely due to several empirical challenges. Shirking is hard to detect and measure. Traditional labor supply proxies such as earnings and hours of work are typically observed with noise and at a low frequency, subjecting them to confounding (labor demand) interpretations. More importantly, they do not capture work intensity such as the effort level. Another key challenge lies in the difficulty in isolating exogenous variation in house price movements, which is required for causal inference. This paper combines a novel, administrative dataset, which allows us to detect non-work behavior during work hours, with a unique setting in China's housing market to study the labor supply impact of house price increase.

China's housing market has experienced phenomenal growth since the early 2000s. Compared with the U.S., China's housing boom is of greater magnitude and has lasted longer (Glaeser, et al., 2017). The large housing booms, which are also prevalent across Chinese cities, provide more power and therefore are an ideal setting for researchers to identify the impact of house price increase on labor supply.

We measure time use at work with confirmed credit card transactions, based on a novel dataset obtained from a leading commercial bank in China that covers its entire population of more than 22 million credit card holders in China's 32 provinces and municipalities (as of 2012). The bank has a 10% market share in China's credit card industry, and credit cards have become a primary

[1] http://www.salary.com/2014-wasting-time-at-work/

method of household consumption in China (more than 48% of the country's household consumption, equivalent to 18% of China's GDP, occurred through credit cards in 2012). Thus, our credit card dataset allows us to capture representative household behavior in a large sample of consumers with a high degree of accuracy and granularity.

Credit card transactions and cardholder information are available in a 22-month period between 2008:01 and 2009:10 for a random sample of the bank's credit card customers. Using more than 9.3 million credit card transactions, we observe individual's credit card behavior on the transaction basis, including the amount, type, location, and exact time of each credit card swipe. We propose a novel measure of shirking by making use of the *time stamp* (up to the second) of each credit card transaction in our sample. Using the credit card for non-work-related transactions during work hours is strongly indicative of work-time shirking for an employed individual. We use the credit card transaction types provided by the bank to identify non-work-related transactions and focus on the propensity to carry out such transactions during work hours as our main measure. To further control for unobserved heterogeneity in this measure across individuals, we rely on the within-person change in our empirical analysis to identify changes in shirking behavior.

Moreover, the dataset provides a rich array of information on individual cardholder's demographic and socioeconomic characteristics such as birth date, gender, education and marital status, and credit limit. More importantly, we observe the individual cardholder's homeownership status as well as detailed and verified information on their employment, which includes employment status (employed, unemployed, or retired), industry of employer, employer type (government, SOE, or private sector), occupation type, and position rank. The comprehensive individual-level characteristics help improve our identification and trace out the economic mechanism.

We motivate our analysis by documenting a positive correlation between our main shirking measure and the lagged house price growth rate in the local city at a monthly frequency. While the correlation suggests a plausible positive effect of house prices, a causal interpretation of the finding faces severe challenges due to the non-random nature of house price changes. Unobserved (time-varying) factors such as local demand shocks may drive house price movement and individuals' labor-market decisions at the same time. To address the identification challenge, we exploit the unique institutional setting in China's land auction market and use the announcement of the land auction, which sets a nationwide record for the highest land price per square meter ("Land King"), as a plausibly exogenous shock to the house price of the winning land parcel's city.

In China, land auction prices reflect developers' projection of future house prices. When the land auction hits a national record high price, it is a particularly bullish signal of the local housing market. Having become salient events over the years that attract media coverage and attention, Land King announcements are commonly perceived by the public as positive indicators of local house prices. There are three Land King events that satisfy such criteria during our sample period, and the three winning cities are Shanghai (August 27, 2008), Hangzhou (August 18, 2009), and Xiamen (September 8, 2009). Consistent with this perception, local house prices in these three cities experienced a monthly increase of 5% on average in our sample period after the Land King announcements.

It is important to note that the crucial identifying assumption of our empirical strategy hinges on the imperfect ability to predict the precise city and the precise timing of the national record-setting land auctions. The three "shocked" cities, Shanghai, Hangzhou, and Xiamen, experienced strong house price growth in the past but are not among the highest in 120 Chinese cities during the four-year pre-shock period. More specifically, the timing of the three Land King announcements is unpredictable, since they were not preceded by abnormally high house price growth in the three cities during the pre-shock period.

Using the three Land King events as shocks to house prices, we analyze the within-individual response in their propensity to use credit cards for personal transactions during work hours among the treatment group—employed credit card holders living in Shanghai, Hangzhou, or Xiamen. The employed individuals in the unaffected cities, who are matched based on observable demographics and employment characteristics, serve as the control group to estimate the counterfactuals. We conduct the analysis at the monthly level and control for individual fixed effects and allow industry- and employer-type-specific year-month fixed effects, and cluster the standard errors at the city level.

After the Land King shocks, employed individuals in the three shocked cities became about 1.7% more likely to use work hours to attend to their personal needs. The coefficient estimates are highly statistically significant at the one-percent level. The effect is economically meaningful: compared with the treatment group's pre-shock mean of 21.3%, the estimated average monthly response is equivalent to an eight-percent increase in the propensity. We explicitly test the parallel trend assumption by including in the regression a pre-shock dummy for the pre-shock month among the treatment group. We find statistically and economically insignificant coefficient estimates for the pre-trend dummy. This further supports our identifying assumption, as there is no differential trend in the outcome variable between the treatment group and the control group in the month immediately before the shocks. Moreoever, we study the post-shock response in cities neighboring the shocked cities, based on the idea that cities within close proximity share correlated economic fundamentals and strong economic ties. Therefore, if the estimated response is driven by some unobserved positive economic shocks, then we expect to see a similar response in the cities that are close to the winning cities of Land Kings. We find no change in the propensity of work-hour personal transaction behavior in cities neighboring those that announced Land King.

A plausible interpretation for the rise in the instance of work-hour personal transactions is due to the treatment group's overall increase in their credit card use during the post-shock period. We directly test this hypothesis and find no evidence of post-shock increase in credit card activity among the treatment group. We also find no post-shock change in work-hour personal transaction propensity among those living in the three shocked cities who are not working. This suggests that the effect we observe for the treatment group captures labor supply response, rather than other behavioral changes in the credit card use pattern. We also verify that the effect is not driven by outliers: 60% of the treatment group experienced an increase in their propensity to use credit cards for personal transactions. The prevalence of the effect makes it unlikely to be explained by individuals' decision to quit their jobs after the house price shocks.

We consider two economic mechanisms to explain the response. First, the large wealth windfall after positive house price shocks will influence the labor supply choice of homeowners by raising the opportunity cost of effort. Renters do not benefit from the positive house price shocks and should not increase their shirking propensity. We investigate this economic channel by studying the differences in the post-shock response between homeowners and renters. Consistent with this hypothesis, we find a strong response among homeowners. The effect for renters is indistinguishable from zero both statistically and economically. Furthermore, we find heterogeneous effect among homeowners, with a much stronger response for homeowners with higher housing wealth (e.g., those with multiple homes).

Another possible mechanism is through the labor demand channel. After positive house price shocks, the labor demand curve likely shifts outward due to the development of real estate and other industries (Charles, Hurst, and Notowidigdo, 2017b). More employment opportunities in the market increase an average worker's outside options and thereby reduce the cost of shirking (e.g., Burda, Genadek and Hamermesh, 2016). Local non-real-estate companies may also endogenously respond to the more optimistic housing market by changing their business focus, which in turn affects their employees' work effort (e.g., Deng et al., 2011; Chen et al., 2017). However, this economic mechanism applies to both homeowners and renters, which is inconsistent with our finding of a concentrated response among homeowners. We further investigate this hypothesis by exploiting the high-frequency nature of our data to study the timing of the response. Under the plausible assumption of a slow adjustment in labor demand, we expect to see a delayed and gradual response. Inconsistent with the prediction, we observe a significant response starting from one to two months after the Land King announcements. Moreover, the effect is persistent and (almost) constant throughout the 12-month post-shock period. Overall, these findings show strong support for increased housing wealth as the underlying economic mechanism.

What do our findings imply for labor productivity? Is it possible that the treatment group maintained their productivity by working harder at other hours of the day? We look at personal credit card transaction behavior during different hours. If the treated individuals move their work activity to other hours of the day, then we should observe a lower occurrence of non-work-related credit card transactions during those times. We find no evidence that the treated individuals decreased the probability of using credit cards for personal transactions during lunch hours (12pm–2pm). Moreover, they became even more likely to have non-work-related credit card transactions in the early hours (8am–9am) or late hours (5pm–9pm) of the day. Looking within the work hours, we find a similar pattern. The effect is concentrated in the early morning (9–10am) and right before lunch (11am–12pm). Taken together, the evidence is inconsistent with the hypothesis that the treatment group changed their work hours after the shocks. Instead, the results suggest that they became more likely to show up late for work and take leave earlier at the end of the workday or before lunch.

The treated individuals may also become more efficient after the shocks to maintain their productivity and take time off to attend to personal needs without hurting productivity. This explanation implies a stronger increase in work-hour credit card use among more skilled or motivated workers. However, we find a stronger effect among workers with lower work incentives—older people approaching retirement age, especially those who work in SOEs that have weak pay-performance sensitivity. In sum, the collective evidence provides support for an interpretation of increased shirking after the house price shocks with lower labor productivity implications.

Finally, we conduct a battery of additional analyses. To further test the parallel trend assumption, we use pre-shock dummies with different lengths of the pre-shock window and find qualitatively and quantitatively similar results. Our results are also robust to two alternative control groups to estimate the counterfactuals in our analysis. We vary our measure of shirking by restricting to leisure spending during work hours or by studying the number of non-work-related transactions during work hours. The main results remain to hold.

There is a growing literature on the labor market consequences of housing, especially the recent housing boom, is growing. Mian and Sufi (2014) show that the decline in housing net worth played a key role in explaining the sharp decline in U.S. non-tradable employment between 2007 and 2009. Charles, Hurst, and Notowidigdo (2017a) study how the national boom and bust in the U.S. housing market affect college attendance choices, leading to a potential labor misallocation implication. Sodini, et al. (2017) show that homeownership has a positive but short-lived effect on earnings, consistent with a debt-induced labor supply increase. We directly contribute to the literature by providing the first empirical analysis on the effect of housing booms on worker effort. Our main estimate suggests an 8% monthly increase in shirking propensity in cities that experienced a 5% post-shock monthly increase in house prices. This implies an elasticity of shirking propensity with respect to house price of 1.6. In this aspect, our results also echo existing research that documents the corporate sector's distraction from their normal business activity after significant house price increases in China (Deng et al., 2011; Chen et al., 2017).

We also add to the broad literature on the impact of housing wealth. Prior studies find significant consumption response to housing wealth (Campbell and Cocco, 2007; Browning, Gortz, and Leth-Petersen, 2013; Mian, Rao, Sufi, 2013; Agarwal and Qian, 2017; Sodini et al., 2017). Our findings suggest that an increase in housing wealth has a negative impact on the labor supply by making effort costlier (for homeowners). Lastly, we broadly contribute to the literature on housing as a transmission channel to the aggregate economy (Lustig and Van Nieuwerburgh, 2005; Mian and Sufi, 2009, 2011; Bhutta and Keys, 2016; Chen et al., 2017; Di Maggio et al., 2017). The results in this paper point out the need to consider the negative labor productivity implications associated with house price increases.

The remainder of the paper is organized as follows. Section 2 describes background information about China's housing market. Section 3 introduces the data and empirical strategy. Section 4 presents the empirical results on the average post-shock response and several falsification tests. Section 5 discusses the economic mechanism. Section 6 presents evidence of the productivity implications, and Section 7 shows additional robustness results. Section 8 concludes.

2. China's Housing Market

2.1 Background information

China is the largest developing economy with a rapidly growing housing market. Since the founding of the People's Republic of China (PRC) in 1949, the housing market in China has experienced several waves of reforms. A milestone reform event happened in 1998 with the issue of the 23rd Decree[2]: housing was no longer welfare oriented, and the objective was to build a private housing market. From then on, the government would no longer distribute housing to the public and all households were required to buy or rent a house from the private housing market.

[2] The full name of the State Council Document is 'Notice of the State Council on Further Deepening Urban Housing System Reform and Speeding Up Housing Construction'.

This change brought about a new stage of development in the Chinese housing market. The number of privately built houses and house prices began to grow dramatically. According to the National Bureau of Statistics of China, investment in China's real-estate sector was 30 trillion Chinese Yuan (4.5 trillion US Dollar) in 2008, having increased by 20.9% compared to the previous year.

China's housing market has since then experienced phenomenal growth. According to statistics from the National Bureau of Statistics of China, the average transaction price in the country increased by more than 200% from 2000 to 2015 (see Figure 1, Panel A). Even in real terms, China's house prices rose by more than 10% on an annual basis (Glaeser et al., 2017). In comparison, the U.S. market witnessed a housing boom with close to 60% price increase between 2000 and 2007, followed by a bust during the financial crisis, before house prices slowly recovered close to their pre-crisis level by the end of 2015 (Figure 1, Panel B). Therefore, China's housing market appears to grow at a faster rate with a persistent trajectory over the last 20 years.

[Insert Figure 1. About Here]

There is also great heterogeneity in the development of the housing market across regions. A common classification identifies four tiers of Chinese cities based on past house price growth. The first-tier cities include the top four cities (Beijing Shanghai, Shenzhen, and Guangzhou), and the second-tier cities include most provincial capitals and the more developed prefecture cities. Third- and fourth-tier cities are generally much smaller cities. To illustrate the cross-sectional heterogeneity, we plot the house price growth between 2003 and 2007 of 120 Chinese cities in Figure 2, based on the house price indices estimated by Fang et al. (2016). The geographical distribution of the house price growth across cities is consistent with the corresponding economic development; economically more developed cities (regions) are also associated with stronger house price growth rates during the period.

[Insert Figure 2 About Here]

2.2. Land Auctions in China

One important characteristic of China's recent housing market growth is the emergence of public land listing and auction system to determine land prices. The first land auction in China was held in Shenzhen in 1987. However, from 1987 to 2004, there were no public auctions of land parcels. Developers were required to contact local governments about land parcels they were interested in, and they would then negotiate a price without an auction. In 2004, a new policy was implemented that all residential and commercial urban land had to be listed and auctioned publicly (Wu, Gyourko, and Deng 2012). All developers were required to bid at land auctions based on their assessment of the local housing demand and projection of future house prices.

Since China liberalized its real estate market in the 1990s, strong housing demand as well as rising competition among developers accelerated the pace of property development and residential land values have also skyrocketed in recent years (Deng, Gyourko and Wu, 2012, 2015). Rising house prices boosted developers' confidence in making land-purchase decisions. The fierce competition for land in the more developed cities pushed up land prices to record highs (either in terms of total price or unit price). Such record-setting land auctions have become salient events that draw media attention and discussion, and the winning land parcel is commonly known as "Land Kings." By taking into account the land costs in their profit-maximization problem, real estate developers will not participate in the land auctions unless the (expected) future house price in the local market exceeds the bidding price for the underlying land. Put differently, the land transaction price aggregates developers' expectation of future house prices. Therefore, Land King events, or record-setting land auctions, are perceived as bullish signals about the future price trend in the local housing market.

3. Data and Empirical Strategy

3.1. Data

We use a unique credit card dataset obtained from a leading Chinese commercial bank, which enjoys 10% of the country's credit card market covering all 32 provinces and municipalities in China. The dataset obtained from the bank contains individuals' monthly credit card statement information from 2004 to 2012 of the entire population of over 22 million credit card accounts (as of 2012).

The dataset also contains the transaction information of each credit card account in a 22-month period from 2008:01 to 2009:10, including transaction amount, merchant category code, location of the transaction, transaction date, and the precise time stamp (up to the exact minute of the day) of each credit card transaction. In addition, we obtain a rich set of demographic and socioeconomic characteristics of a random sample of the population of credit-card holders. In addition to information on common demographics such as birth date, gender, ownership status, educational level, marital status, income, and approved credit limit, we also observe detailed employment information, including employment status, industry of employer, employer type (government, SOE, or private sector), occupation, and position rank.

This dataset offers several advantages. First, our sample covers a large panel of consumers in China and captures representative household behavior. Credit cards have become a primary method of household consumption in China. According to the "Blue Book on the Development of China's Credit Card Industry," released by the China Banking Association, the total credit card transaction volume amounts to RMB 10 trillion by the end of 2012, equivalent to 18% of China's GDP in 2012. Credit card spending accounts for over 27% and 48% of China's entire household consumption in 2009 and 2012 respectively.[3] A major online media outlet, NetEase Financial, conducted a survey on credit card use among 16,000 users. Most credit card holders surveyed (70%) indicated their preference to use credit cards as a payment method whenever and wherever possible. The large, representative coverage of our bank's credit card holders facilitates our study of household behavior in China.

Second, our dataset contains rich information about individual behavior. We can track individuals' credit card behavior at the transaction level, including the amount, type, location, and the exact time of each credit card swipe. This allows us to observe individuals' behavior, including the time of their credit card transactions, with a high degree of granularity. Such rich and high frequency data empower our identification of the effect of house prices on work-time shirking behavior. Moreover, detailed information about individual cardholders' demographic and socioeconomic

[3] In mid-size and large cities, which are over-represented in our dataset, credit card spending is likely to represent a greater fraction of household consumption.

characteristics, especially wealth indicators and employment-related information, helps improve identification and allows us to trace out the underlying economic mechanisms.

Third, our administrative dataset provides high-quality observations with low measurement errors. We can track exact individual behavior through recorded credit card transactions, offering more precision compared to traditional survey-based data sources to understand individual or household decision making. In addition, we observe individual credit card holders' demographic and socioeconomic characteristics with greater accuracy. The bank collects and verifies personal information whenever it starts a new banking relationship with an individual. For example, at the time of credit card application, consumers in China are *required* to submit proof of their ID and employment information. In our sample, close to 92 percent of the credit card holders opened their account with the bank within two years before our test period. As a result, we can observe the account holder's demographics including their employment status and employer type with precision.[4]

3.2. Measuring Shirking

We use our representative sample of credit card transactions and make use of the exact *time stamp* of each credit card transaction in our sample. Since we can identify credit card holders' employment status, observing a personal transaction charged on credit cards during work hours is strongly indicative of work-time shirking for an employed individual. To capture the propensity of such behavior, we define our main shirking measure, *Work-hour personal transaction dummy*, as a dummy variable equal to 1 if the credit card holder ever has a non-work-related credit card transaction during work hours in a month, and zero otherwise.[5]

Work-hours are defined as 9am – 12pm and 2pm – 5pm on workdays. We note the presence of variation across employers or across regions on the actual work hours—some may start at 8am while others end at 6pm (or even later). Moreover, lunch hours likely exhibit cross-sectional heterogeneity as well. Our chosen work-hours are motivated to avoid ambiguity and measurement errors, since 9am – 12 pm and 2pm – 5pm describe work time with greater certainty (we also explicitly study the credit card transaction behavior during other hours of workdays in the later analysis). Workdays include Mondays to Fridays that do not fall on public holidays according to the official holiday calendar in 2008 and 2009. When credit card transactions occur out of town, the cardholder could be on vacation or travelling for work purposes. Therefore, we do not classify these out-of-town transactions as work-hour transactions.

[4]Official reported income in China is well known to understate its true value (Deng, Wei, and Wu, 2017). To minimize the measurement error in the income variable, we follow the literature and focus on the approved credit limit as the proxy of individual wealth (e.g., Gross and Souleles, 2002; Agarwal and Qian, 2014). Credit limit, as granted by the bank, incorporates the applicant's income and other wealth indicators (such as home ownership, education, employer, occupation and position rank) and offers a more informative indicator of the card holder's wealth.

[5] We use the dummy variable as the main measure for the following reasons. First, the number of credit card transactions in a month is around four or five in the overall sample and less than one transaction on average occurs during work hours and for non-work-related reasons (see Table 2). This suggests that studying the extensive margin (with the dummy variables) captures the first order effect. Second, the number of non-work-related credit card transactions is weakly correlated with the intensity of shirking behavior as we do not observe the length of the transaction (one credit card transaction could take more time than two other credit card transactions). In Section 7, we also study the robustness of our results with respect to our shirking measure with several alternative definitions.

We classify credit card transactions based on the merchant categories provided by the bank. To illustrate, Table 1 provides a breakdown of more than 9.3 million credit card transactions in our credit card transaction sample. 65.38% transactions are spent on goods and services, and the remaining 34.62% transactions are related to payment of credit card bills, utility bills, fees associated with government services, and financial services such as insurance or investment products. Panel B of Table 1 presents a frequency breakdown of the top five credit card transaction types according to the internal bank classifications, including (onsite) payment of financial services, warehouse retailer, department store, fee payment and restaurant.

[Insert Table 1 About Here]

To account for the possibility that some credit card transactions may be related to work, we focus on transactions of personal spending on goods and services as well as payment and purchase of financial services. Specifically, we exclude spending items on hotels, transportation, and training expenses.[6]

Our transaction-based measure, based on actual time stamps of personal transactions charged on credit cards, provides a strong signal of work-time shirking behavior at a high frequency.[7] On the other hand, we cannot detect the exhaustive list of shirking behavior, as our credit card data do not capture other shirking methods such as spending time on personal phone calls or social media. In addition, differences in this measure across individuals may also reflect differences in work hours as well as other unobserved heterogeneity in the cross section. For example, some occupations have more flexible work time (e.g., professors), while others work at odd hours (e.g., doctors and nurses). As a result, comparing the measure across individuals may confound interpretation. To alleviate the influence of these measurement errors on the interpretation, we will rely on exogenous variation in house prices and study the within-individual change to difference out the cross-sectional unobserved heterogeneity.

To construct the final analysis sample, we apply several filtering criteria. We exclude dormant/closed accounts and accounts that remained inactive (i.e., with no transactions) for at least half of the sample period between 2008:01 and 2009:10. We restrict our focus to the top Chinese 300 cities (by population) since the remaining cities are small and non-representative with few credit card accounts. To study the labor market effect of housing booms, we further restrict the sample to individuals older than 22. We also exclude the supplementary credit card holders from the sample to cleanly identify the effect of house prices on the working population (we do not observe demographics and employment information for supplementary card holders). Thus, the final sample comprises a monthly panel between 2008:01 and 2009:01 for 209,148 credit card holders. Among these card holders, the bulk of the analysis focuses on 202,778 employed individuals. We use the card holders who are not working—retired or unemployed—in our falsification analysis.

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[6] In the robustness check, we also use a stricter definition of non-work-related transaction by further excluding spending on dining, bars and clubs, gyms, golf, medical services and other service categories, which are ambiguous in nature.

[7] To measure shirking, traditional labor supply measures such as earnings or hours worked are inapplicable. Some use indirect and noisier proxies: Ichino and Maggi (2000) measure shirking with the number of absence episodes in a year for one Italian bank.

3.3 Identification Strategy

Before we describe our identification strategy, we first provide some motivating evidence of the correlation between our shirking measure and the past house price growth. To do so, we use the house price index of 120 Chinese cities estimated by Fang et al. (2016) and calculate the monthly house price growth. In our dataset, we can identify 110 of the 120 cities in Fang et al. (2016), after which we examine whether an (employed) credit card holder's propensity to conduct a personal transaction during work hours is associated with the previous month's house price growth in the city they reside in. The preliminary results indeed suggest a positive relationship: a 10% increase in the past month's local house price growth is associated with a 0.4% increase in the employed cardholder's likelihood to use credit cards for personal purposes during work hours (see Table IA.1 in the Internet Appendix).

While the correlation provides suggestive evidence of a plausible positive effect of house prices on shirking behavior, a causal interpretation of the finding faces severe challenges due to the non-random nature of house price changes. Unobserved (time-varying) factors such as local demand shocks may drive house price movement and individuals' labor market decisions at the same time. As an example, more skilled workers, who may have a taste for work, likely self-select to high house growth areas that tend to have better amenities, leading to a downward bias of the effect of house prices on shirking. The measurement error of our main shirking measure can also contaminate the interpretation, as discussed previously (Section 3.2).

To address the identification challenge, we exploit the unique institutional setting of China's land auction market and use the announcement of the land auction, which sets the nation-wide record of the highest land price per square meter (i.e., "Land King"), as a plausibly exogenous shock to the house price of the winning land parcel's city. There are three Land King events that satisfy such criteria during our sample period, and the three winning cities are Shanghai (August 27, 2008), Hangzhou (August 18, 2009), and Xiamen (September 8, 2009). More details of these land auctions are described in Panel A of Table IA.2 in the Internet Appendix.

As mentioned in Section 2, land auction prices reflect developer's projection of future house prices. When the land auction hits the national record high price, it is a particularly bullish signal of the local housing market. Having become salient events over the years that attract media coverage and attention, Land King announcements are commonly perceived by the public as positive indicators of local house prices.[8] Based on the house price index estimated by Fang et al. (2016), we find the three shocked cities experienced a significant increase in house price during the same post-shock period (as our main analysis window), with an average monthly appreciation rate of 5%.[9]

A crucial identifying assumption lies in the exogenous nature of these events. Admittedly, the cities of winning land parcels typically are more economically developed with a higher house price level on average. However, the exogenous variation arises from the imperfect ability to predict the precise city and the precise *timing* of the record-setting land auctions. Figure 2 shows the

[8] For example, the announcements of these three Land Kings are widely covered by online media such as Sina.com, Sohu.com, and Tencent.com.

[9] We also conduct a diff-in-diff analysis on the house price growth rate and find the same result—the three Land King winning cities experienced a large and statistically significant house price growth in the post-shock period, relative to the price change in unaffected cities.

distribution of house price growth from 2003 to 2007 among 120 major Chinese cities. Shanghai, Hangzhou, and Fujian are not among the highest house price growth cities in the four-year period before the Land King events. Furthermore, it is arguably difficult to forecast the exact month of these Land King announcements. Our analysis in Table IA.2, Panel B provides further evidence: past house price levels or growth rates (up to three months of lag) cannot predict the occurrence of the Land King shocks in the three cities used in our analysis. We will further test the exogeneity assumption by 1) studying the parallel trends assumption in the work-hour personal transaction behavior among the treatment group; and 2) exploit the high frequency nature of our data to study the response in a short window after the shocks.

3.4. Empirical Specification

Using the three Land King events as shocks to house prices, we analyze the within-individual response in their propensity to use credit cards for personal transactions during work hours among the treatment group—employed credit card holders living in Shanghai, Hangzhou, or Xiamen. We use the employed individuals in the unaffected cities as the control group to estimate the counterfactuals.

We use the following regression model to estimate the average spending response:

$$Y_{i,t} = \delta_t + \alpha_i + \beta_{post} D_{i,post} \ + \ \epsilon_{i,t} \tag{1}$$

The dependent variable, $\mathrm{y_i}$, refers to our main measure *Work-hour personal transaction dummy*, which is a dummy variable equal to 1 if an (employed) individual *i* ever uses their credit cards for non-work-related transactions during work hours in month *t*; and 0 otherwise. α_i represents individual fixed effects to absorb time-invariant factors at the individual level. $D_{i,post}$ is a dummy variable equal to one in the post-shock months for treated individual *i*, and zero otherwise.[10] δ_t represents a vector of year-month fixed effects to control for common trends that affect individuals' likelihood of conducting non-work-related credit card transactions during work hours. To better control for time varying trend in the labor market conditions for each industry or for each employer type (government, SOE, or private sector), we also allow for industry-specific and employer-type-specific time trends in the empirical specifications. β_{post} in Equation (1) captures the treatment group's average post-shock change in the propensity to use credit cards for non-work-related transactions during work hours.

To explicitly test the parallel trends, we also estimate the following specification:

$$Y_{i,t} = \delta_t + \alpha_i + \beta_{pre} D_{i,(-1m,-1m)} + \beta_{evt} D_{i,0m} + \beta D_{i,post} \ + \ \epsilon_{i,t} \tag{2}$$

$D_{i,(-1m,-1m)}$ is a dummy variable equal to one for the pre-shock month if individual *i* is in the treatment group, and zero otherwise. Specifically, it will take a value of one for the month 2008:07 if the employed individual *i* lives in Shanghai, or for the month 2009:07 if the employed individual *i* lives in Hangzhou, or for the month 2009:08 if the employed individual *i* lives in Xiamen. For

[10] Since the Land Kings were announced in the middle of the month, we are unable to assign the event month as either pre or post-shock month. Therefore, we exclude the months when Land Kings were announced from the sample in estimating Equation (1).

the treatment group, the absorbed period is from the beginning of the sample period (2008:01) to two months before the shocks and is the benchmark period against which our estimated response is measured. Therefore, β_{pre} estimates the change in the propensity of non-work-related credit card transactions during work hours in the one-month pre-shock period relative to the benchmark period. Validity of our identification strategy requires parallel trends, i.e., β_{pre} is statistically and economically indistinguishable from zero. In Equation (2), we also include the event months and use a separate parameter (β_{evt}) to estimate the treatment group's response in the event month.

In addition, we estimate the dynamics of the average post-shock response. Specifically, for each event month *s* (*s*=-1, -1, …, 12), and estimate the following specifications (in each regression, we exclude the treatment group's observations after the event month *s*).

$$Y_{i,t} = \delta_t + \alpha_i + \beta_{pre} D_{\mathrm{i,(-1m,-1m)}} + \epsilon_{i,t}, \qquad s < 0 \tag{3a}$$

$$Y_{i,t} = \delta_t + \alpha_i + \beta_{pre} D_{\mathrm{i,(-1m,-1m)}} + \beta_{evt} D_{i,0m} + \epsilon_{i,t}, \qquad s = 0 \tag{3b}$$

$$Y_{i,t} = \delta_t + \alpha_i + \beta_{pre} D_{\mathrm{i,(-1m,-1m)}} + \beta_{evt} D_{i,0m} + \beta_{post,s} D_{i,s} + \epsilon_{i,t}, \quad s > 0 \tag{3c}$$

where $D_{i,s}$ is a dummy variable equal to one for the event months between *1* and *s* for a treated individual *i*. $\beta_{post,s}$ in Equation (3c) thus captures the average post-shock response to the Land King events between event months *1* and *s*, relative the benchmark period (i.e., 2008:01 – one month before the shock). β_{pre} in Equations 3 (a, b, c) estimates the average change for the event month *-1*, relative to the benchmark period. β_{evt} has the same interpretation as before, as it measures the treatment group's response during the month of Land King announcement relative to the benchmark period.

By running a series of regressions that gradually extend *s*, we can trace the dynamics of the response. For example, β_{-1} estimates the average change in the treatment group's propensity in event month *-1*, relative to the benchmark period, and β_{12} captures the average change in the treatment group's propensity from event month *1* to *12* in the post-shock period, relative to the benchmark period.

Equations (1) - (3) are estimated using ordinary least squares (OLS), and the standard error are clustered at the city level.

3.5. Summary Statistics

Table 2, Panel A provides summary statistics of demographics and credit card activities for the treatment and control groups in our sample. The treatment group (individuals living in Shanghai, Hangzhou, or Xiamen) are noticeably different from the control group. On average, the treatment group is 33.5 years old and 0.5 years younger than the average control group's age. Both groups have a similar fraction of female credit card holders, but the treatment group is much less likely to be married (58% vs. 71%). Credit card holders in the treatment group have an average credit limit that is close to RMB 11,000 higher than the control group (in relative terms the difference is 111% of the control group's average credit limit). Seventy-six percent of the treatment group own a home, compared with the fraction of 80% for the control group. The treatment group is also more likely to hold a college degree or above than the control group (47% vs. 40%), has a greater fraction of individuals working in the private sector (80% vs. 62%) or holding senior ranks (44.1% vs. 34%). The differences are economically meaningful and statistically significant. To the extent that labor market choices (such as shirking) plausibly differ by wealth and employment characteristics, one legitimate concern arises whether the control group captures a valid counterfactual in the estimation.

[Insert Table 2 About Here]

To this end, we construct a matched sample of individuals in Shanghai, Hangzhou, and Xiamen (treatment) and individuals in control cities (control) that are observationally similar. Specifically, we compute propensity scores based on a logistic regression using a rich set of account information, as well as demographics information including (natural logarithm of) age, a quadratic polynomial of credit limit, ownership status dummies, female dummy, marital status dummy, college dummy, and a dummy for the employer type (government, SOE, or private sector). We use the nearest neighbor matching without replacement to identify a matched observation for each treated individual. The summary statistics of the treatment and the matched control group are reported in Panel B of Table 2.

After matching, the difference between the treatment and control groups in age, homeownership, marital status, education, type of employer, position rank, and credit limit become statistically insignificant. The magnitude of the differences is also economically small. The fraction of female cardholders in the matched control group is slightly smaller than that for the treatment group (significant at the 10% level), but the economic magnitude of the difference is negligible (1.3%). In addition to the mean statistics, we also compare the distributions of the two continuous variables between the treatment and the matched control groups. Figure 3 shows that both age and the credit limit (at account opening) have a similar and comparable distribution between the treatment and the matched control group. In sum, we have a panel of observationally similar treatment and control group, which facilitates a more precise estimate of the counterfactuals and identification of the treatment effect in our analysis. We will use the treatment group and the matched control group as our sample in the main analysis. Admittedly, the matched sample approach may not eliminate the unobservable differences between the treatment group and control groups. In our analysis, we will explicitly test for the parallel trend assumption in the pre-shock period. In Section 7, we also verify the robustness of our results with alternative counterfactual groups.

[Insert Figure 3 About Here]

Finally, we provide a comparison of the credit card activities between treatment and control. Panel C of Table 2. During our sample period, card holders in the treatment group charge an average of 4.7 transactions per month on their credit cards. In comparison, the control group on average has a monthly credit card transaction count of 4.1 (in the full sample) and 4.8 (in the matched sample). Twenty-three percent of the treatment group has (at least) one non-work-related credit card transaction during work hours in a given month, compared with the control group’s fraction 31% in both the full sample and the matched sample.

4. Main Results

4.1. The Average Post-Shock Response

We begin by estimating the average response after the Land King shocks among the treatment group. Specifically, we study the change in the treated individual's propensity to use credit cards for non-work-related transactions during work hours in the post-shock months relative to the pre-event months. We estimate Equation (1) and report the results in Panel A of Table 3.

Column 1 shows the regression results by including individual and year-month fixed effects. After the Land King shocks, employed individuals in the three shocked cities became 1.7% more likely to use their credit cards for personal transactions during work hours. The coefficient estimate is statistically significant at the one-percent level. The effect is economically meaningful: compared with the treatment group's pre-shock mean of 21.3%, the estimated average response is equivalent to an eight-percent increase in the propensity.

[Insert Table 3 About Here]

The specification in column 1 controls for the overall time trend in the likelihood of the employed population to have non-work-related credit card transactions during work hours during our sample period. To allow time trends to vary by industry or by employer type, we include industry-specific time fixed effects, or employer-type-specific time fixed effects, or both in columns 2 to 4. The bank's data provide 15-industry classification of the individual cardholder's employer, and the employer type has three categories: government, SOE, or private sector. We continue to find a significant response after the Land King shocks among the treatment group. In column 4, we control for both industry-specific and employer-type-specific year-month fixed effects, and the estimated coefficient is 0.0175, which is significant at the one-percent level.

Next, we estimate Equation (2) by explicitly testing the parallel trend assumption. We report the results in Panel B of Table 3. Under the hypothesis that Land Kings are house price shocks exogenous to the employed individuals in our sample, and that the treatment group and the control group are comparable, we expect no differential trend between the treatment group and the control group in the short period immediately before the Land King announcements. To test this, we include a pre-shock dummy, $D_{i,(-1m,-1m)}$, equal to 1 for the pre-shock month for the treatment group, and the coefficient estimate β_{pre} should be zero under the parallel trend assumption. We also include an event-month dummy for the treatment group ($D_{i,0m}$) to study the immediate response after the announcements. For the treatment group, the absorbed period is from the beginning of the sample period (2008:01) to two months before the shock events and is the benchmark period against which our estimated response is measured.

In all four specifications (with different time fixed effects), we consistently find a statistically insignificant estimate of β_{pre}. In addition, the magnitude of β_{pre} estimates are economically small. To interpret, we do not find a differential trend between the treatment and control groups, in the work-hour personal transaction propensity during the one-month pre-shock period relative to the benchmark period. Similarly, we find an insignificant response during the announcement month among the treatment group. Moreover, the estimates for the post-shock dummies remain significant both statistically and economically. We conduct a formal F-test of the difference between β_{post} and β_{pre}, and we can reject the hypothesis that the two coefficients are equal (e.g. *pvalue* < 0.001 in column 4 specification). Taken together, the results provide strong support for our identifying assumption: the treatment group exhibited no difference in their pre-event behavior and only increased their propensity to have non-work-related credit card transactions during work hours in months *after* the Land Kings were announced.

4.2 Post-shock Response in the Neighboring Cities

While the absence of pre-trend suggests the Land King announcements were unanticipated by the past local economic conditions, we conduct further falsification analysis to mitigate concerns about confounding factors.

Specifically, we focus on neighboring cities of the shocked cities, based on the idea that cities sharing geographic proximity have similar economic exposure. For example, Jiangsu and Zhejiang are two provinces next to Shanghai. Shanghai and its close neighbors in Jiangsu and Zhejiang form the well-known economic region ("Yangtse River Delta Zone"). Economic development in Shanghai and cities in the two neighboring provinces is highly correlated due to similar economic fundamentals and strong economic ties within the region. Therefore, if the estimated response is driven by some unobserved positive economic shocks, then we are likely to see a similar response in the cities that are close to the winning cities of Land Kings.

To test this idea, we focus on the Land King announced in August 2008 in Shanghai and study the response in cities of Jiangsu and Zhejiang. We choose not to study the neighboring cities of Hangzhou and Xiamen mainly because of the short post-event sample associated with those two Land King announcements. We use the other unaffected cities—excluding Shanghai, Hangzhou, and Xiamen—as the control group. We conduct the analysis in the sample period from 2008:01 to 2009:06 (with 10 post-shock months) to avoid confounding effects around the second Land King announcement in August 2009 (in Hangzhou). We use the same specifications in Equation (1) and (2) and report the results in Table 4.

Column 1 shows the post-shock response among employed individuals in cities of Jiangsu and Zhejiang with individual and year-month fixed effects. The estimated coefficient β_{post} (-0.0009) is negative. Moreover, it is both statistically insignificant and small in economic magnitude. The same result holds when we allow industry- and employer-type-specific time trends (column 2). We also include the pre-shock dummy and the event-month dummy in columns 3 and 4, and again we find no change in the propensity to use credit cards for non-work-related transactions during work hours for individuals in Jiangsu and Zhejiang.

[Insert Table 4 About Here]

4.3. Does the Overall Credit Card Activity Increase?

A plausible interpretation of the result in Table 3 is that the rise in the instance of credit card transactions made during work hours reflects an overall increase in the credit card use among the treated individuals during the post-shock period. We investigate this hypothesis by directly testing the credit card activity after the Land King shocks. First, we study whether the treated individuals become more likely to use their credit cards in general during the post-shock period. Panel A of Table 5 summarizes the results. We find no evidence of an increased propensity of credit card use for the treatment group after the Land King shocks. The estimated coefficient (-0.0022 in column

1) is both statistically insignificant and economically small. In addition, the pre-shock dummy and the event month dummy estimates are both indistinguishable from zero (see columns 3 and 4), which further corroborates the finding of no visible credit card use pattern before and after the shocks.

[Insert Table 5 About Here]

As an additional test, we study the credit card use outside work hours. If the documented effect in Table 3 is due to an overall increase in spending as well as other credit card transactions, we expect to see a similar increase in the propensity to use credit cards in other non-work hours for the treatment group. Contrary to this prediction, we find an opposite effect. Conditional on using the credit cards, the treatment group became less likely to use their credit cards in the non-work hours (e.g., weekends and holidays) after the shocks (Panel B of Table 5). The coefficient estimates are statistically significant, but the economic magnitude is modest (equivalent to 0.8% of the pre-shock average). Collectively, these results suggest no increase in the overall credit card activity among the treatment group after the shocks. Treated individuals somewhat shift away from using the credit cards during the non-work hours to the work hours.

4.4. Post-shock Response by Retirees and the Unemployed

We interpret the increasing propensity of non-work-related credit card transactions during work hours as the treatment group's labor supply response to house price shocks. Given this interpretation, we should only observe a response among the employed individuals in the three shocked cities. On the other hand, the Land King shocks could have triggered other behavioral changes in credit card use among the treated individuals, for example with their (non-working) spouses starting to use their credit cards during the post-shock period. Under this hypothesis, we expect to observe a similar response even among those who are not working. In this regard, we study the post-shock response for credit card holders in Shanghai, Hangzhou, and Xiamen who are retired or unemployed.[11] The control group comprises retirees and the unemployed in the full sample of the unaffected cities. Then we repeat the analysis as in Table 3 and report the results in Table 6.

The non-working population in the shocked cities experienced no change in their work-hour personal transaction propensity after the Land King events (and the estimated response coefficients are negative). The estimated coefficients for β_{post} is indistinguishable from zero and statistically insignificant. The same pattern holds regardless of the choice of time fixed effects and inclusion of pre-event and event-month dummies.

[Insert Table 6 About Here]

[11] One concern is that the unemployment status at the time of account opening may reflect stale information. In addition, unemployment status may be correlated with wealth conditions that lead to a downward-biased estimate. This is less likely in our setting since more than 90% card holders opened their accounts less than two years before our analysis period. Furthermore, we conduct one more analysis by excluding the unemployed and focusing on retirees. Retirement is an absorbing state and therefore the analysis is less subject to the measurement error. We find consistent results both qualitatively and quantitatively.

4.5. Distribution of the Treatment Group's Response

Is the documented response driven by outlier observations? How prevalent is the response by the treatment group? To better understand the scope and nature of the effect, we investigate these questions by studying the distribution of the post-shock response within the treatment group. To do so, we need to have an estimate of the change for each treated individual and thus cannot rely on the regression framework. Instead, we compute the propensity change for each treated individual by properly controlling for time trends. Please refer to Appendix B for detailed description of the computation.

We plot the distribution of the post-shock response in the propensity to use credit cards for personal transactions during work hours for each treated individual in Figure 4. The evidence suggests that our results are not driven by outliers. First, the mean (median) post-shock change is a 1.6% (2.7%), corresponding to a 7.4% (12.5%) increase in the propensity to have non-work-related credit card transactions during work hours. This is largely consistent with the regression result reported in Table 3. Moreover, more than 60% of the treatment group experienced a propensity increase after the shocks. The mode of the distribution (> 16%) sits in the range of [0, 5%].

These patterns help sharpen interpretation of the documented effect. An alternative labor supply response arises from the decision for the treated individuals to quit their job, for example to enjoy life given the housing wealth windfall. Then the observed effect could be due to their off-work leisure consumption rather than distraction on the job. However, this explanation seems implausible to reconcile with the prevalence of the post-shock response—more than 60% of the treated individuals experienced an increase in the propensity of personal use of credit cards during work hours. The broad scope of the positive response also further mitigates the concern that non-working family members of the treated individuals started "borrowing" their credit cards after the shocks.

[Insert Figure 4 About Here]

5. The Economic Mechanism

Next, we explore the economic mechanism underlying the significant post-shock response.

5.1. The Role of Housing Wealth

We show in Section 3 that the Land King announcements predict a strong subsequent price increase in the local housing market (Table IA. 3). This implies a significant increase in housing wealth for existing homeowners. The large wealth windfall, in turn, will influence the labor supply choice by changing the tradeoff between effort and leisure. More specifically, the opportunity cost of leisure rises after the positive house price shocks for homeowners. As a result, they will find it less rewarding to exert effort. We investigate this economic channel by studying the differences in the post-shock response between homeowners and renters. Renters do not benefit from the positive house price shocks. In fact, one may argue that a higher house price translates into a higher cost of living for this group of people, as they face a higher rental cost or a greater down payment requirement for future home purchase. Therefore, they have no incentive to increase shirking after the Land King announcements.

Our bank's data provide homeownership status for credit card holders in the sample. In our matched treatment and control sample, we identify 8,528 homeowners and 2,643 renters.[12] We augment Equation (1) by interacting the post-shock dummy with ownership status and report the results in Table 7.

Columns 1 and 2 report the heterogeneity in the average post-shock response between homeowners and renters (with year-month fixed effects, and industry- and employer-type-specific year-month fixed effects respectively). In both columns, we find a strong post-shock response among homeowners in the treatment group. They became 1.8% more likely to have non-work-related credit card transactions during work hours after the Land King announcements, and the effect is statistically significant at the one-percent level. Renters, on the other hand, did not change their work-time behavior after the shocks, as shown by the statistically insignificant and economically small coefficients (0.0035 for column 1 and 0.0045 for column 2).

[Insert Table 7 About Here]

Among homeowners, we can differentiate owners with mortgages and owners who have no mortgages or who have paid off their mortgages. Owners with no outstanding mortgage in the treatment group may enjoy a greater wealth increase than the owners who have not paid off their mortgages. In columns 3 and 4, we decompose homeowners into these two categories and study the differential response within the owner group. However, we do not find a stronger response among owners without mortgages. One potential reason is that the mortgage status alone is insufficiently informative concerning housing wealth, which is also determined by the value of the house or the number of owned homes. We further test the hypothesis of a stronger effect among wealthier owners by exploiting several (better) proxies of housing wealth based on the credit card holders' demographic and socioeconomic indicators. In this analysis, we focus on the owner subpopulation of the matched sample and report the results in Table 8.

In the credit card approval process, the bank considers multiple demographic and socioeconomic characteristics, including income, education level, marital status, employer type, name, industry, and position rank, to determine the applicant's spending capacity and default probability. Therefore, the granted credit limit is a composite measure of credit card applicants' creditworthiness based on the bank's proven credit scoring algorithm. Owners with a high credit limit are likely wealthier and have greater housing wealth than owners with a low credit limit (Gross and Souleles, 2002; Agarwal and Qian, 2014). Owners with a lower credit limit experienced a significant increase in their propensity to have non-work-related credit card transactions during work hours (Table 8, columns 1 and 2). More importantly, consistent with the hypothesis, owners with a high credit limit are even more likely to use credit cards for non-work-related transactions during work hours in the post-shock period. For example, in column 2 where we control for industry- and employer-type-specific year-month fixed effects, the estimated coefficient (0.0114, statistically significant at the one-percent level) captures the incremental increase in the propensity for owners with high credit limit. This is equivalent to an 85% larger effect than the response by owners with a lower credit limit.

[12] We do not have enough information to assign owner status for the rest of 5,638 individuals in the matched sample, for example when their dwelling status indicates living with family members. We exclude these individuals in the analysis on homeowners in Section 5.

[Insert Table 8 About Here]

We also exploit the transaction feature of our dataset and use the credit card transaction codes to identify owners who have purchased a(nother) home after account opening (but before the Land King shocks). This is a small subset of the owner population (N=27 in the treatment group); however, they are more informative about the shirking incentive. This is because owners with multiple homes can capitalize on the house price increase and access their increased housing wealth. It is possible that other homeowners in our bank's data, with or without mortgages, may own more than one home as well, but transaction-based measures provide a cleaner identification of such owners. We interact the post-shock dummy with a *Multiple homes* indicator and report the results in columns 3 and 4 of Table 8. Despite the small sample of owners with multiple homes in our treatment sample, we find a very significant incremental effect. Compared with other homeowners in our sample, owners with multiple homes became 3.8-3.9% more likely to use credit cards for non-work-related transactions during work hours in the post-shock months. The effect is statistically significant at the one-percent level. Other homeowners experienced a 2-2.2% increase in their propensity in the post-shock period, suggesting that the effect magnitude for owners with multiple homes is close to three times as large as the rest of the owner population.

5.2 Dynamics of the Post-Shock Response

Another plausible channel is through the implications of house price shocks on local labor demand. For example, after positive house price hoscks, the labor demand curve likely shifts outward due to of the development of real estate and related industries (e.g., Charles, Hurst, and Notowidigdo, 2017b). More employment opportunities reduce shirking cost, which could explain the findings documented in Table 3 (Burda, Genadek and Hamermesh, 2016). Alternatively, local non-real-estate companies may endogenously respond to the more optimistic housing market by changing their business focus, which in turn affects work effort of their employees (e.g., Deng et al., 2011; Chen et al., 2017). However, results in section 5.2 find a strong response only among homeowners and there is no effect among treated renters. This means the labor demand channel is unlikely to explain our result, since the mechanism should apply to both groups of treated individuals.

Nevertheless, we perform one more test of the labor demand channel by making use of the sticky nature of labor market adjustment. We will exploit the high-frequency nature of our data and study the response timing. Specifically, we estimate Equations (3a), (3b), and (3c) to analyze the treatment group's average response for each event month s, with $s = -1, 0, \ldots, 12$.

We plot the estimated coefficients of the average response along with the 95-percent confidence intervals in Figure 5. In this analysis, we control for individual fixed effects and allow industry- and employer-type-specific year-month fixed effects. Consistent with the static regression results, the treatment group exhibited no different behavior in the month before the shock (s=-1) or when the Land Kings were announced (s=0). The estimated coefficients are small with wide confidence intervals that cross zero. However, the estimated coefficient starts increasing in the month immediately after the Land King announcement (s=1): the treatment group becomes 0.8% more likely to use credit cards for personal transactions during work hours. In the next month (s=2), the coefficient estimate further increases to 0.018 and becomes statistically significant at the 5% level. To interpret, in the first two post-shock months, the treatment group on average becomes 1.8% more likely to use credit cards for non-work-related transactions during work hours. This is a significant increase by 8.5% relative to the treatment group's pre-shock average. The effect remains persistently high in the remaining 10 post-shock months with a similar magnitude (ranging from 0.019 to 0.021) and strong statistical significance (at the 1% percent level).

[Insert Figure 5 About Here]

In sum, we observe an immediate and significant response among the treated individuals after the Land King shocks. Moreover, the coefficient stays persistent and stable throughout the 12-month post-shock period with no reversal, suggesting a permanent effect. An immediate and permanent response is consistent with prior evidence of the labor supply response to unearned income (wealth shock) (Cesarini, et al., 2017). These findings are inconsistent with the labor demand response interpretation. They also further alleviate the alternative labor supply interpretation; it is unlikely for a significant number of people to quit or switch their jobs immediately after the Land King events.

Taken together, results in this section show strong support for the role of housing wealth as the underlying economic mechanism. Homeowners are positively shocked with an increase in their wealth after the Land King announcements, with a significantly increased likelihood of carrying out non-work-related credit card transactions during work hours. The effect is particularly pronounced among owners with higher housing wealth. Renters, on the other hand, did not benefit from the positive price shocks and did not change their work-time behavior after the Land King announcements.

6. Productivity Implications

Our results by far suggest that after positive house price shocks, workers become distracted and more frequently engage in non-work-related activities during business hours. To identify the implications for labor supply as well as labor productivity, we conduct several analyses to study other behavioral responses by the treatment group.

6.1. Switching Work-hours in the Post-Shock Period?

One conjecture lies in the possibility that the treated workers shift their working hours during the day. During the post-shock period, they might have started their work earlier, shortened the lunch break, or extended their work hours into later hours of the day. On the other hand, a shirking interpretation suggests lower work incentive in other (extended work) hours.We directly test the hour-switching hypothesis using our transaction data. If the treated individuals move their work activity in other hours of the day, then we should observe a lower instance of non-work-related credit card transactions during those times. We create dummy variables to indicate the presence of non-work-related credit card transactions for lunch hours (12–2pm), early and late hours (8–9am, and 5–6pm), and overtime hours (6–9pm). In the same way as we apply our main measure, we restrict our analysis to no-travel work days in defining these dummy variables. We use these as dependent variables and estimate the specification in Equation (1). Results are reported in Table 9.

Columns 1 and 2 show that, relative to the same hours in the pre-shock period, the treatment group displayed no change in the likelihood of using their credit cards for personal transactions between 12pm and 2pm of work days. Contrary to the shifting hypothesis, the coefficient estimates are positive. Moreover, the effect is small (0.0062 for column 1 and 0.0070 for column 2) and statistically insignificant.

[Insert Table 9 About Here]

Next, we look at the treatment group's credit card use between 8am and 9am and between 5pm and 6pm. These may be the official business hours for some employers or occupations. A likely alternative interpretation is that the treated individuals increased their labor supply by being more punctual both starting and ending a day's work. If so, we would again expect a reduction in the probability in observing non-work-related credit card transaction during these hours. Surprisingly, we find a significant increase, rather than decrease, in the treatment group's propensity to use their credit card for personal transactions between 8–9am and 5–6pm during the post-shock period (columns 3 and 4). The effect is statistically significant at the five-percent level. It appears that the treatment group became more distracted from work in these early or late (business) hours.

Lastly, we examine the extent to which the treatment group increased their labor supply in the extended business hours (6pm–9pm) of work days during the post-shock period. Columns 5 and 6 of Table 9 reveal, again, an increased propensity by the treatment group to use their credit cards for personal transactions between 6pm and 9pm of work days after the Land King announcements. To interpret, they appeared to become less likely to work overtime.

The collective evidence is inconsistent with the hour-switching hypothesis. Instead, the findings suggest that the treated individuals became less motivated to work in other, extended (work) hours in the post-shock period as well.

6.1. Decomposition of the Response by Work Hour and by Day of Week

We conduct one more analysis by decomposing our main measure into six dummies to indicate the presence of non-work-related credit card transactions for 9–10am, 10–11am, 11am–12pm, 2–3pm, 3–4pm, and 4–5pm. We estimate Equation (1) separately using these six hourly dummies as dependent variables. We plot the estimated coefficients along with the 95-percent confidence intervals in Panel A of Figure 6.

It becomes evident that much of the post-shock increase in the instance of credit card use for personal transaction occurs during the first hour (i.e., 9–10am). Relative to the same hour in the pre-shock period, the treatment group becomes 0.8% more likely to use their credit cards for personal transactions between 9am and 10am of work days after the Land King announcements (statistically significant at the one-percent level). Another hour interval that experienced a significant increase in the instance of credit card use is 11am to 12pm (coefficient estimate = 0.0082, statistically significant at the one-percent level). We conclude that these results, combined with the results in Table 8, reveal that the treatment group did not increase their labor supply in other hours of work day after the Land King announcements. The hourly pattern of personal use of credit card suggests that, after positive house price shocks, the treatment group became more likely to show up late for work, enjoy a longer lunch break, and leave earlier at the end of the workday.

[Insert Figure 6 About Here]

Are there particular days of the week that we observe a more prominent response? One might expect a reduced work morale near the end of the week. In Panel B of Figure 6, we plot the estimated coefficients, along with 95-percent confidence intervals, of five regressions separately analyzing responses from Monday to Friday. We see no increased instance of personal use of credit cards on working Mondays. However, we see a small increase in such instances on Tuesdays, followed by no-action Wednesdays. Finally, the largest spikes occur for the last two days of the week—Thursdays and Fridays. These patterns are consistent with a shirking interpretation: workers start the week fresh from two days of leisure consumption during the weekend, but their effort level deteriorates as the week progresses, with the shirking incentives peaking at the end of the work week.

6.3. Becoming More Efficient in the Post-Shock Period?

If the treatment group becomes more efficient after the shocks, that fact that they can finish their work tasks in a shorter period and attend to personal needs in work hours does not necessarily hurt productivity. We study this hypothesis by exploiting the cross-sectional heterogeneity by work incentives. The above explanation implies a stronger increase in work-hour credit card use among more skilled or motivated workers. On the other hand, if high house prices disincentive work effort and productivity through a wealth effect, we should observe a stronger effect among less motivated workers with poorer career potential. We use the proximity to retirement age as a proxy for individuals' work incentive. As one approaches retirement age, the upside potential for income increase and promotion diminishes quickly, and thus we hypothesize that the close-to-retirement card holders in the treatment group are less likely to become more efficient at work.

We first estimate Equation (1) by interacting the post-shock dummy with an indicator of age >= 50. Results are reported in columns 1 and 2 of Table 10. We find a stronger increase in the propensity to use credit cards for personal transactions by the older population in the treatment group. The magnitude of their response is more than twice that among the younger (<50) population in the treatment group. We further look at the older employees who work in state-owned enterprises (SOE), who arguably have even less work incentive due to SOEs' weak pay-performance sensitivity. Results in columns 3 and 4 show that older SOE employees are much more likely to increase the propensity to use credit cards for personal transactions during work hours, compared to the other older employees. Therefore, our finding of a stronger effect among workers with lower work incentives provide support for an increased shirking and a lower labor productivity interpretation.

[Insert Table 10 About Here]

7. Additional Analysis

In this section, we conduct a battery of tests to verify the robustness of our main results.

7.1. Parallel Trends: Alternative Pre-shock Windows

To further validate the parallel trends assumption, we allow various lengths of the pre-event window. Our results on the average post-shock response are robust to the different pre-shock event window choices. We re-estimate Equation (1) by replacing the one-month pre-shock window dummy with a two-month pre-event window dummy, a three-month pre-event window dummy, and a four-month pre-event window dummy respectively. The results remain qualitatively and quantitatively similar. For brevity, we report the results in the Internet Appendix (Table IA.3 of the Internet Appendix) for this and the rest of the analysis in this section.

7.2. Counterfactuals: Alternative Control Groups

We consider two alternative control groups to estimate the counterfactuals in our analysis. First, we use the employed individuals in cities that are geographically close to the three shocked cities as the control group, including cities in Zhejiang, Jiangsu, Fujian, and Guangdong provinces. Individuals living within geographic proximity are likely to share similar preferences regarding credit card use. Nearby cities also likely share the same work-hour norms and other employer preferences. Results remain very robust: the effect magnitude is comparable with the matched sample analysis in Table 3 and the estimates for the pre-shock dummies are indistinguishable from zero both statistically and economically (Table IA.4, Panel A). Second, we use all individuals in the unaffected cities as the control group. The analyses using the full, unmatched sample produce very similar estimates and inference, which provide validation of our matched sample approach (Table IA.4, Panel B of the Internet Appendix).

7.3. Alternative Measures of Work-hour Personal Transactions

Finally, we replace our main measure of shirking with several alternative proxies (see Table IA.5 in the Internet Appendix). First, we use stricter filtering criteria for credit card transactions that are not related to work. In addition to hotel, transportation, and training expenses, we further exclude the following items from the personal transaction identification: spending on dining, bars and clubs, gyms, golf, medical services, and other service categories, which are ambiguous in nature. We also restrict our focus to work-hour spending on retailers, department stores, theatres and spas to study the response for the treatment group's leisure activities during work hours. The dependent variable in column 3 is the (natural logarithm of) the number of work-hour personal transactions, and the dependent variable in column 4 is the number of work-hour personal transactions, divided by the total number of credit card transactions in the same month. Given these different measures of personal transactions, the main results remain to hold.

In addition to examining the propensity to use credit cards for personal transactions during work hours, we check the robustness of our results by using the number of work-hour personal transactions as our measure of shirking. To control for differences in the level of total credit card use across individuals, we also scale the number of work-hour personal transactions by the total number of credit card transactions in the same month. We continue to find a significant increase at the intensive margin using both measures.

8. Concluding Remarks

In this paper, we study the impact of house price increase on individuals' shirking behavior at work, by exploiting a unique credit card dataset obtained from a leading Chinese commercial bank with 10% market share of the credit card industry. We use the type and actual *time stamps* of more than 9.3 million credit card transactions of over 200,000 card holders to identify non-work-related transactions during work hours. This transaction-based measure provides a strong signal of shirking at the individual level and at a high frequency.

After positive shocks to house prices, treated individuals experienced an immediate and permanent increase in the propensity to use credit cards for non-work-related transactions during work hours. The overall credit card use, on the other hand, did not increase for the treatment group after the shocks. In addition, we find no effect for workers in the neighboring, unaffected cities or among retirees and the unemployed in the "shocked" cities. The large windfall of (housing) wealth makes leisure more appealing and effort costlier. Consistent with this channel, the post-shock response is driven by homeowners in the treatment group, with an even greater impact among owners with a higher housing wealth (i.e., those with multiple homes). Renters, on the other hand, experienced no change in the post-shock period. Consistent with increased shirking and lower productivity interpretation, further analyses find no evidence of the treatment group working harder at other hours of the day. The increase in work-hour non-work activity concentrates in morning and near-lunch hours and is stronger near the end of the work week. In addition, the response is stronger among workers with low work incentives.

The documented increase in shirking is economically significant. Our main estimate suggests an 8% monthly increase in shirking propensity in cities that experienced a 5% post-shock monthly increase in house prices. This implies an elasticity of shirking propensity with respect to house price of 1.6. This is likely an underestimate due to the fact that we only capture one form of shirking, i.e., through credit card use during work hours.

Overall, our paper points to an understudied yet important real consequence of house price increase, with direct implications for labor productivity and economic growth. This is particularly pertinent for China, which has experienced an unrivalled growth trajectory in the housing market since the early 2000s.

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APPENDIX A VARIABLE DEFINITIONS

<u>Work-time Transaction Variables (Derived from Credit Card Data)</u>

Total # CC transactions refers to the total number of credit card transactions an individual makes in a month.

Work-hour personal transaction dummy is a dummy variable equal to 1 if the credit card holder ever uses credit cards for non-work-related credit card transactions during work hours in a month, and zero otherwise. We classify credit card transactions based on the merchant categories provided by the bank. Non-work-related transactions include personal spending on goods and services as well as payment and purchase of financial services that are charged on credit cards. We exclude spending items that are potentially related to work such as hotel, transportation, and training expenses. In the robustness check, we also use a stricter definition of non-work-related transaction by further excluding spending on dining, bars and clubs, gyms, golf, medical services and other service categories that are ambiguous in nature. To define work hours, we focus on 9am –12pm and 2pm – 5pm of weekdays (i.e., Mondays to Fridays) that do not fall on public holidays and do not have out-of-town credit card transactions (i.e., days of travel).

Credit card transactions dummy is a dummy variable equal to 1 if the credit card holder uses the credit card in a month; and 0 otherwise.

Credit card transactions in non-work hours dummy is a dummy variable equal to 1 if the credit card holder uses the credit card during non-work hours in a month; and 0 otherwise.

Work-hour leisure spending dummy is a dummy variable equal to 1 if the credit card holder ever has a credit card transaction on retailers, department stores, theatres and spas during work hours in a month, and zero otherwise.

 # work-hour personal transactions provide the count of non-work-related credit card transactions that occur during work hours in a month for each credit card holder.

<u>Demographic Variables</u>

Age is the individual cardholder's age at the transaction year. ***Older*** is a dummy equal to 1 if the credit card holder is older than 50 years old, and 0 otherwise.

Female is a dummy variable that equals one if the credit card holder is female; zero otherwise.

Married is a dummy variable that equals one if the credit card holder is married; zero otherwise.

College is a dummy variable that equals one if the credit card holder obtains a college degree or above; zero if below college.

Own is a dummy variable equal to 1 for homeowners, and zero otherwise. ***Own with mortgage*** is a dummy variable equal to 1 for homeowners who have outstanding mortgage payments, and zero otherwise. ***Own without mortgage*** is a dummy variable equal to 1 for homeowners who have paid off mortgage payments, and zero otherwise. ***Multiple homes*** is a dummy variable equal to 1 if the individual owns multiple houses in our sample; and 0 otherwise. Specifically, an individual is considered to own more than one home if they were an owner at the time of account opening *and* had a property purchase transaction (on credit card) after account opening (but before the Land King shocks).

Rent is a dummy variable equal to 1 for renters in the sample, and zero otherwise.

Credit limit is the total credit line (in RMB) of all the credit cards within this bank as of the card origination year. ***High Credit Limit*** is a dummy variable equal to 1 if an individual's credit limit (at account opening) is in the top tercile of the distribution among all card holders in our sample.

Employment-related Variables

SOE is a dummy variable equal to 1 if the credit card holder works in the State-owned Enterprises, and zero otherwise. ***Government*** is a dummy variable equal to 1 if the credit card holder works in the government agencies, and zero otherwise. ***Private*** is a dummy variable equal to 1 if the credit card holder works in private enterprises, joint ventures, or as self-employed, and zero otherwise.

High-rank is a dummy variable equal to 1 if the credit card holder holds a senior-rank position at work—including CEO, director, department manager, chief physician, and full professor, and zero otherwise. The information is obtained from the occupation reported at account opening.

Retire is a dummy variable equal to 1 for retired individuals. An individual is retired when the cardholder is older than 60 (for male) or older than 55 (for female) or enters "retired" as the employment status at account opening, and zero otherwise.

Unemployed is a dummy variable equal to 1 when the credit card holder enters "unemployed" as the employment status at account opening, and zero otherwise.

Appendix B. Definition of Change in Propensity of Work-hour Personal Transactions (In Figure 4)

We compute the post-shock change in the propensity to have work-hour personal transactions for everyone in the treatment group in the following steps (by properly controlling for time trend).

A. For each month in our sample period, we compute the average of the work-hour personal transaction dummy across (employed) individuals in the matched control group. The purpose of this step is to create a counterfactual for the propensity to have work-hour personal transactions using the average statistics for the matched, unaffected cities. This is similar to the fixed effects in the regression framework, with the objective to control for common trends in work-hour personal transaction (in a period when China's housing market experienced strong growth in general).

B. Then we adjust the monthly work-hour personal transaction propensity for individuals living in the shocked cities (Shanghai, Hangzhou, and Xiamen) by subtracting the same-month average computed in the previous step.

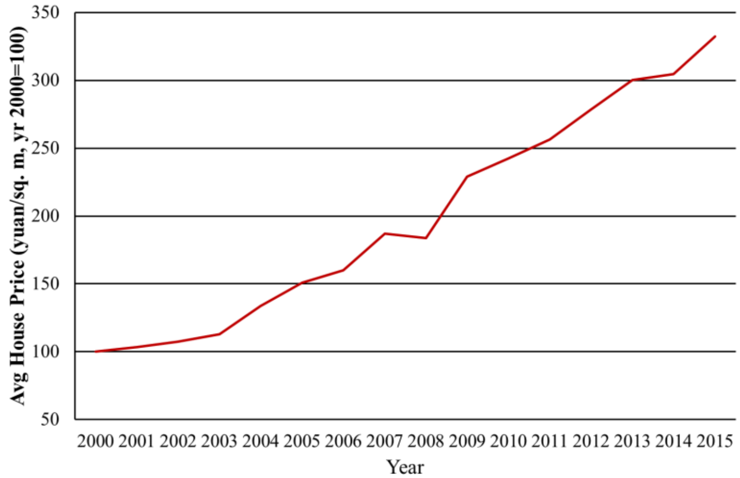
C. For each treated individual, we calculate the average of the adjusted monthly work-hour personal transaction propensity during the pre-shock period and the post-shock period respectively.

D. Finally, for each treated individual, we subtract the pre-shock period average of adjusted work-hour personal transaction propensity from the post-shock period equivalence.

FIGURE 1. COMPARISON OF HOUSE PRICE GROWTH BETWEEN CHINA AND U.S.

This figure plots the annual house price growth in China and in the U.S. between 2000 and 2015. Panel A shows the trend in the average transaction price in China (source: National Bureau of Statistics in China). It is calculated as 'Total Residential House sale'/'Total Floor Area of Sale'. The floor area of completed residential houses is the total floor area that has been completely built. Panel B shows the trend in the house price index in the U.S. (source: FHFA).

Panel A.



Panel B.

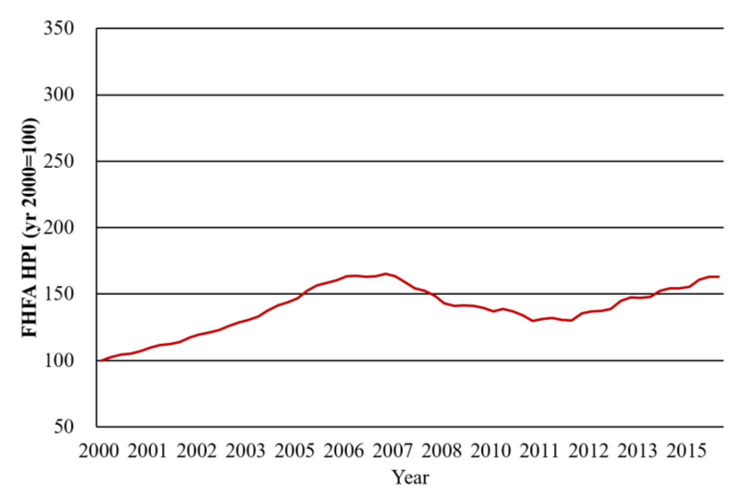


FIGURE 2. HOUSE PRICE GROWTH IN MAJOR CHINESE CITIES

This figure plots the heatmap characterizing the house price growth from across 120 major Chinese cities. We use the house price index at the end of 2007 estimated by Fang et al. (2016), which represents the level of house price in each city relative to its level at the beginning of 2003 (the house price index level at the beginning of 2003 is equal to one). Based on the coefficient estimates, 120 cities are grouped into four categories, with the darkest color corresponding to cities with the largest house price growth during the 2003-2007 period. The three shocked cities in our sample—Shanghai, Hangzhou, and Xiamen—are also highlighted in the figure. Note that grey is used to indicate states for which we do not have (enough) data for estimation.

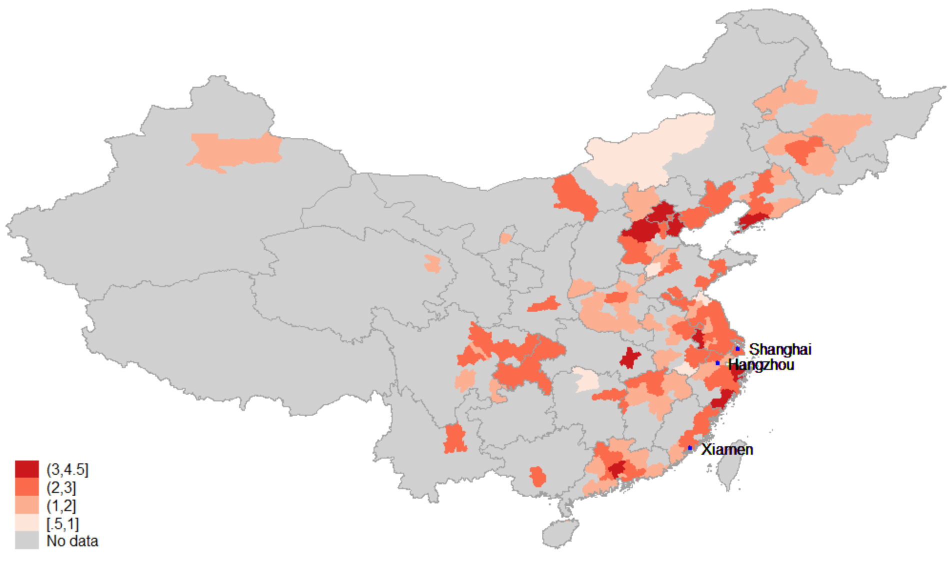
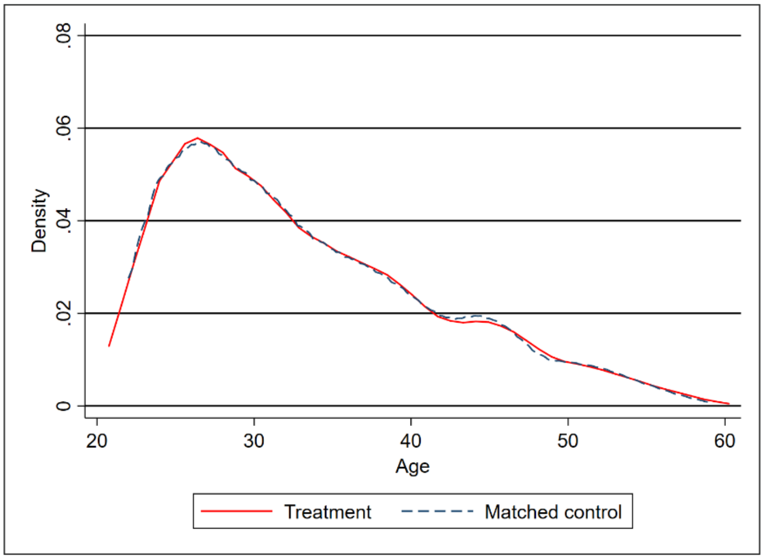


FIGURE 3. KERNEL DENSITY PLOTS

This figure shows the distribution comparison between the treatment group and the matched control group. Panel A shows the kernel density plots of age, and Panel B shows the kernel density plots of (log) credit limit at the time of account opening.

Panel A



Panel B

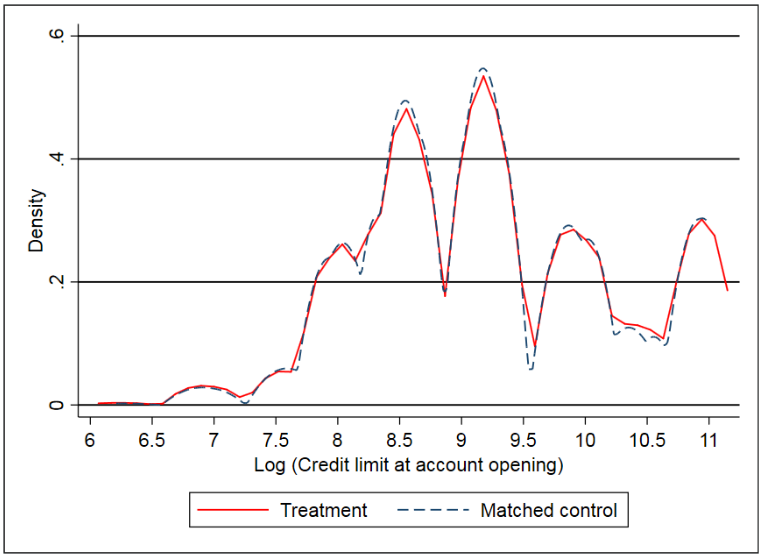


FIGURE 4. DISTRIBUTION OF CHANGE IN WORK-HOUR PERSONAL TRANSACTIONS

This figure plots the distribution of the post-shock change in the propensity of work-hour credit card transactions. X-axis shows the change in the propensity of work-hour personal transactions (after adjusting for time trends). Please refer to the Appendix B for detailed description on the construction of the variables variable definitions.

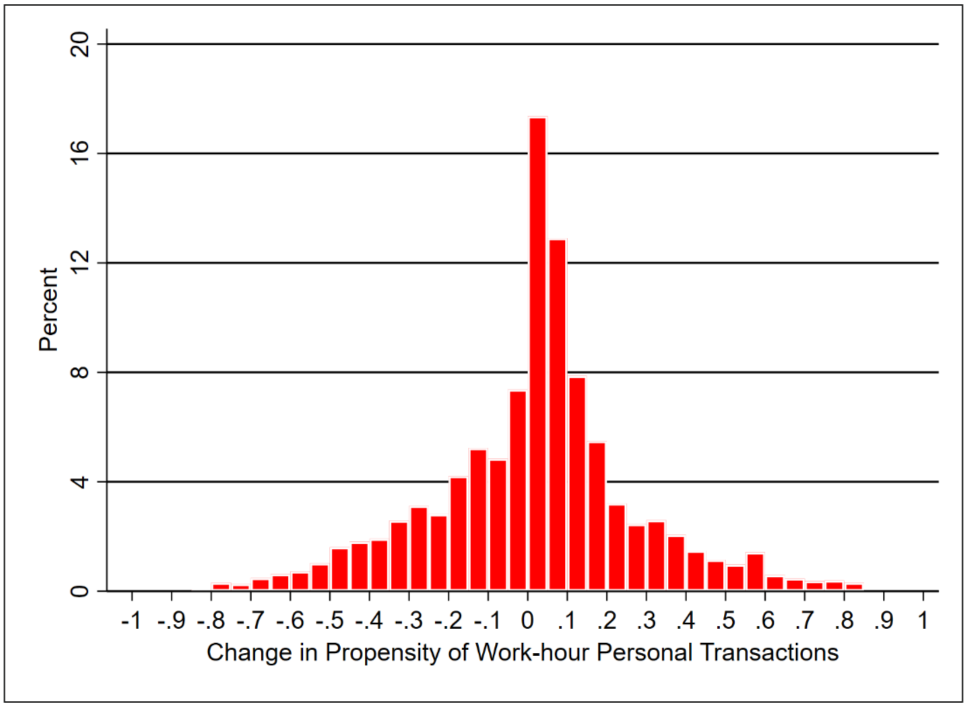


FIGURE 5. ESTIMATED RESPONSE DYNAMICS

This figure plots the entire paths of estimated coefficients b_s, s= -1, 0, ..., 10, 11, 12, from estimating Equation (3a) (3b), and (3c), along with their corresponding 95 percent confidence intervals. The x-axis denotes the *s*th month after the Land King auction, and the y-axis shows the estimated response.

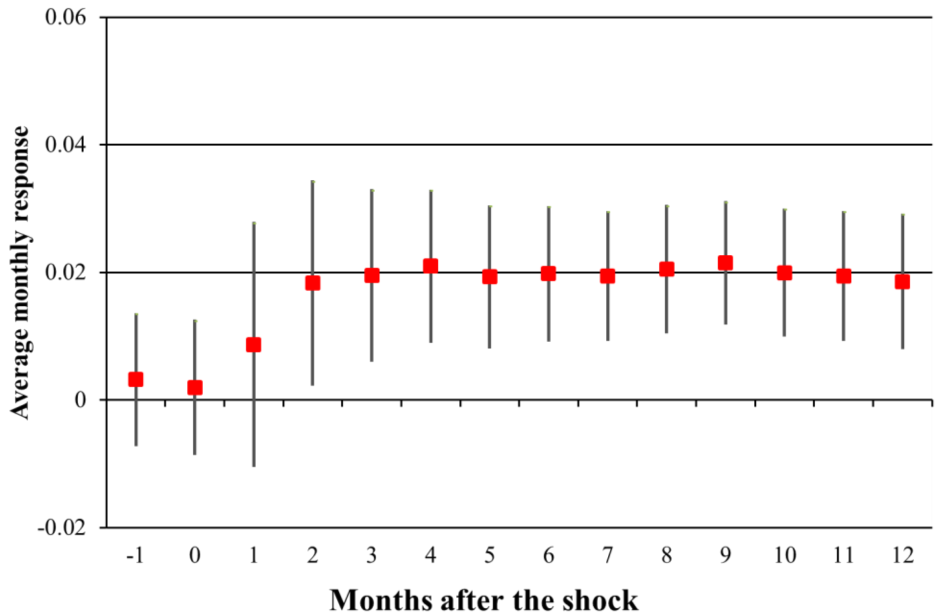
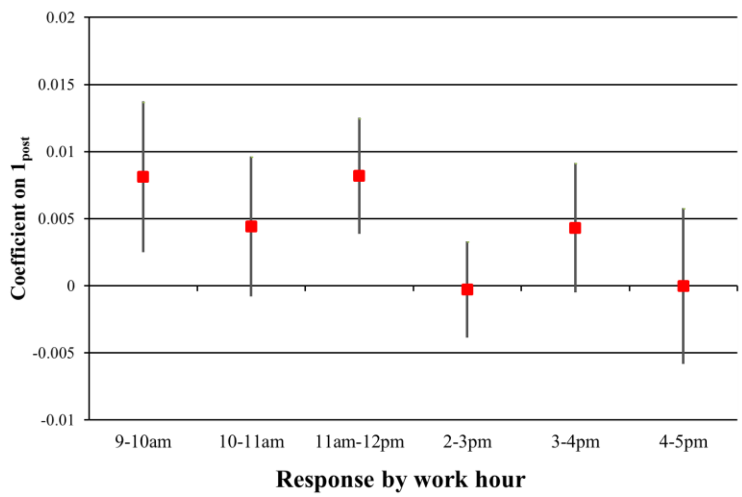


FIGURE 6. RESPONSE BY WORK-HOUR AND DAY OF WEEK

This figure plots the distribution of the propensity of having non-work-related credit card transaction on workdays 1) for each work hour (between 9am and 5pm) (Panel A) and 2) by day of the week (Panel B). Specifically, we decompose the dependent variable used in Table 3 into each work-hour interval or into Monday – Friday and repeat the same analysis as in Table 3. We plot the estimated coefficients along with their corresponding 95 percent confidence intervals.

Panel A



Panel B

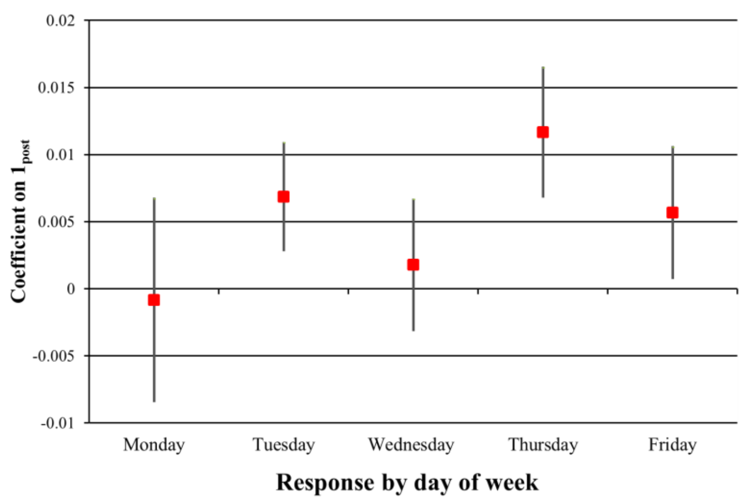


TABLE 1. TYPE OF CREDIT CARD TRANSACTIONS

This table provides a breakdown of more than 9.3 million credit card transactions in our full test sample during the 2008:01-2009:10 period. Panel A presents a frequency breakdown of the types—whether the cardholder uses the credit card to spend on goods and services or to pay for their credit card bills, utility bills, fees associated with government services, and financial services such as insurance or investment products. Panel B presents a frequency breakdown of the top five credit card transaction types according to the internal bank classifications.

	Fraction (%) (N=9,329,296)
Panel A: Types of credit card transactions	
Spending on goods and services	65.38
Payment of financial services, government fees and utility bills	34.62
Panel B: Top 5 transaction types	
(Onsite) payment of financial services	26.72
Warehouse retailer	23.79
Department store	11.95
Fee payment	4.72
Restaurant	3.95

TABLE 2. SUMMARY STATISTICS

This table reports the summary statistics of our treatment and control sample, both before and after propensity score matching (based on the nearest neighbor). The treatment sample consists of employed individuals residing in any of the three "shocked" cities—Shanghai, Hangzhou, and Xiamen—and the control group comprises employed individuals living in the other 297 unaffected Chinese cities. We require individuals/accounts to have at least one transaction in half of the 22-month sample period between 2008:01 and 2009:10 (or half of the months since card opening). We also restrict our analysis to individuals between the age of 22 and 80. Panel A and B show the comparison of demographics between the treatment and control groups before and after propensity score matching. Panel C shows the comparison of credit card transaction frequency and the fraction of personal credit card transactions that occur during work hours (based on the monthly average during the six-month period before the shocks). Please refer to Appendix A for detailed variable definitions.

Panel A: Before matching comparison

	Treatment group		Control group		Diff.
	Mean	SD	Mean	SD	(Control-Treatment)
	(1)	(2)	(3)	(4)	(5)
Age	33.3	8.4	33.8	8.2	0.5***
Female (%)	42.6	49.5	42.9	49.5	0.3
Married (%)	58.0	49.4	71.2	45.3	13.2***
Own	76.4	42.5	80.1	40.0	3.7***
College (%)	47.0	49.9	40.0	49.0	-7.0***
Private (%)	79.5	40.3	61.5	48.7	-18.1***
High-rank (%)	44.1	49.7	34.2	47.4	-9.9***
Credit limit (RMB)	20,275	26,566	9,593	12,229	-10,682***
N	8,422		194,356		

Panel B: After matching comparison

	Matched treatment group		Matched control group		Diff.
	Mean	SD	Mean	SD	(Control-Treatment)
	(1)	(2)	(3)	(4)	(5)
Age	33.3	8.4	33.3	8.1	-0.0
Female (%)	42.6	49.5	41.3	49.3	-1.3*
Married (%)	58.0	49.4	57.4	49.4	-0.6
Own	76.5	42.4	76.2	42.6	-0.4
College (%)	47.2	49.9	47.8	50.0	0.6
Private (%)	79.75	40.3	79.7	40.2	0.1
High-rank (%)	44.1	49.7	43.7	49.6	-0.3
Credit limit (RMB)	20,256	26,541	19,863	26,025	-394
N	8,420		8,420		

Panel C. Pre-shock monthly credit card transactions

	Treatment group		Control group		Matched treatment group		Matched control group	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total # CC transactions	4.65	3.95	4.09	5.17	4.65	3.95	4.45	4.05
Work-hour personal trans dummy	0.23	0.30	0.31	0.31	0.23	0.30	0.31	0.30
# work-hour personal trans	0.37	0.70	0.54	1.01	0.37	0.70	0.53	0.83
N	8,422		194,356		8,420		8,420	

TABLE 3. THE AVERAGE POST-SHOCK RESPONSE

This table shows the average response to the house price shock by the treatment group—employed consumers living in Shanghai, Hangzhou, or Xiamen, based on the matched sample during our sample period from 2008:01 to 2009:10. The dependent variable *Work-hour personal transactions dummy* is a dummy variable equal to 1, if an individual ever uses credit cards for non-work related transactions during work hours in a month; and 0 otherwise. $1_{-1m,-1m}$ is a dummy that equals 1 in the month before the shocks among the treatment group, and to zero otherwise. 1_{0m} is a dummy that equals 1 for the shock month among the treatment group, and zero otherwise. 1_{post} is a dummy that equals 1 for the post-shock months among the treatment group, and zero otherwise. In Panel A, we exclude the event months for the treatment group from our analysis. In Panel B, we include the event months and directly test the response during the event month. Please refer to Appendix A for detailed variable definitions. Standard errors are clustered at the city level. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level respectively.

	(1)	(2)	(3)	(4)
	Work-hour personal transactions dummy			
Panel A				
1_{post}	0.0170***	0.0174***	0.0169***	0.0175***
	(3.35)	(3.46)	(3.39)	(3.53)
Individual FE	Y	Y	Y	Y
Year-month FE	Y	N	N	N
Industry year-month FE	N	Y	N	Y
Employer type year-month FE	N	N	Y	Y
Observations	194,036	194,036	194,036	194,036
R-squared	0.319	0.320	0.319	0.320
Panel B				
$1_{-1m,-1m}$	0.0047	0.0048	0.0045	0.0045
	(0.68)	(0.67)	(0.66)	(0.65)
1_{0m}	0.0049	0.0038	0.0049	0.0040
	(0.88)	(0.67)	(0.90)	(0.73)
1_{post}	0.0181***	0.0186***	0.0180***	0.0186***
	(3.11)	(3.21)	(3.14)	(3.28)
Individual FE	Y	Y	Y	Y
Year-month FE	Y	N	N	N
Industry year-month FE	N	Y	N	Y
Employer type year-month FE	N	N	Y	Y
Observations	200,452	200,452	200,452	200,452
R-squared	0.318	0.319	0.318	0.319

TABLE 4. RESPONSE IN (SHANGHAI'S) NEIGHBORING CITIES

This table shows results of the response, after the Land King event in Shanghai (i.e., 2008:08), by (employed) individuals living in the neighboring cities of Shanghai—cities in the provinces of Jiangsu and Zhejiang. *Neighboring cities*$_{-1m,-1m}$ is a dummy that equals 1 during the pre-shock month (i.e., 2008:07) for residents in Jiangsu and Zhejiang, and zero otherwise. *Neighboring cities*$_{0m}$ is a dummy that equals 1 for residents in Jiangsu and Zhejiang in the event month (2008:08), and zero otherwise. *Neighboring cities*$_{post}$ is a dummy that equals 1 for the post-shock months among residents in Jiangsu and Zhejiang, and zero otherwise. In this analysis, we exclude the treated cities—Shanghai, Hangzhou, and Xiamen—from the sample and focus on the period from 2008:01 to 2009:06 (before the second Land King event in sample). We exclude the event month observations for the treatment group from our analysis in columns 1 and 2. Please refer to Appendix A for detailed variable definitions. Standard errors are clustered at the city level. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level respectively.

	(1)	(2)	(3)	(4)
	Work-hour personal transactions dummy			
Neighboring cities$_{1m,-1m}$			0.0012	-0.0001
			(0.24)	(-0.02)
Neighboring cities$_{0m}$			-0.0070	-0.0059
			(-1.23)	(-1.01)
Neighboring cities$_{post}$	-0.0009	-0.0002	0.0003	0.0005
	(-0.12)	(-0.03)	(0.04)	(0.07)
Individual FE	Y	Y	Y	Y
Year-month FE	Y	N	Y	N
Industry year-month FE	N	Y	N	Y
Employer type year-month FE	N	Y	N	Y
Observations	1,210,990	1,210,990	1,278,862	1,278,862
R-squared	0.338	0.338	0.334	0.334

TABLE 5. THE POST-SHOCK CHANGE IN OVERALL CREDIT CARD ACTIVITY

This table shows the result of the overall credit card use in the post-shock period. The dependent variable in Panel A, *Credit card transactions dummy*, is a dummy variable equal to 1, if an individual uses credit cards in a month; and 0 otherwise. The dependent variable in Panel B, *Credit card transactions in non-work hours dummy*, is a dummy variable equal to 1, if an individual uses credit cards during non-work hours in a month; and 0 otherwise. Please refer to Table 3 and Appendix A for detailed variable definitions. Standard errors are clustered at the city level. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level respectively.

	(1)	(2)	(3)	(4)
Panel A				
	Credit card transactions dummy			
$1_{-1m,-1m}$			0.0056	0.0062
			(0.80)	(0.81)
1_{0m}			0.0002	0.0010
			(0.02)	(0.11)
1_{post}	-0.0022	-0.0023	-0.0025	-0.0025
	(-0.91)	(-0.97)	(-0.80)	(-0.77)
Individual FE	Y	Y	Y	Y
Year-month FE	Y	N	Y	N
Industry year-month FE	N	Y	N	Y
Employer type year-month FE	N	Y	N	Y
Observations	226,240	226,240	233,622	233,622
R-squared	0.190	0.191	0.188	0.189
Panel B				
	Credit card transactions in non-work hours dummy			
$1_{-1m,-1m}$			-0.0034	-0.0030
			(-1.45)	(-1.23)
1_{0m}			-0.0034	-0.0027
			(-1.43)	(-1.08)
1_{post}	-0.0066***	-0.0064***	-0.0077***	-0.0076***
	(-3.01)	(-3.01)	(-3.33)	(-3.30)
Individual FE	Y	Y	Y	Y
Year-month FE	Y	N	Y	N
Industry year-month FE	N	Y	N	Y
Employer type year-month FE	N	Y	N	Y
Observations	194,036	194,036	200,452	200,452
R-squared	0.186	0.188	0.183	0.185

TABLE 6. RESPONSE AMONG RETIREES AND THE UNEMPLOYED

This table repeats the same analysis as in Table 3 on the non-working population, which includes retirees and the unemployed. We exclude the event month observations for the treatment group from our analysis in columns 1 and 2. Please refer to Table 3 and Appendix A for detailed variable definitions. Standard errors are clustered at the city level. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level respectively.

	(1)	(2)	(3)	(4)
	Work-hour personal transactions dummy			
$1_{-1m,-1m}$			0.0359	0.0408
			(0.87)	(0.90)
1_{0m}			0.0066	0.0053
			(0.26)	(0.20)
1_{post}	-0.0141	-0.0129	-0.0106	-0.0091
	(-1.33)	(-1.18)	(-0.61)	(-0.49)
Individual FE	Y	Y	Y	Y
Year-month FE	Y	N	Y	N
Industry year-month FE	N	Y	N	Y
Employer type year-month FE	N	Y	N	Y
Observations	43,757	43,757	43,953	43,953
R-squared	0.411	0.418	0.410	0.417

TABLE 7. DIFFERENCE IN RESPONSE BY HOME OWNERSHIP

This table shows the response heterogeneity by home ownership in our sample of employed individuals. We exclude the event month observations for the treatment group from our analysis. Please refer to Table 3 and Appendix A for detailed variable definitions. Standard errors are clustered at the city level. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level respectively.

	(1)	(2)	(3)	(4)
	Work-hour personal transaction dummy			
1_{post}	0.0035	0.0045	0.0066	0.0073
	(0.75)	(0.93)	(1.41)	(1.45)
$1_{post} \times$ Own	0.0177***	0.0178***		
	(24.53)	(11.88)		
$1_{post} \times$ Own with mortgage			0.0187***	0.0191***
			(7.27)	(7.97)
$1_{post} \times$ Own without mortgage			0.0154***	0.0154***
			(27.97)	(7.57)
Individual FE	Y	Y	Y	Y
Year-month FE	Y	N	Y	N
Industry year-month FE	N	Y	N	Y
Employer type year-month FE	N	Y	N	Y
Observations	130,700	130,700	120,356	120,356
R-squared	0.318	0.320	0.316	0.318

TABLE 8. MORE ON THE HOUSING WEALTH EFFECT

This table shows response heterogeneity—using various housing wealth proxies—in the sample of employed homeowners. We exclude the event month observations for the treatment group from our analysis. Please refer to Table 3 and Appendix A for detailed variable definitions. Standard errors are clustered at the city level. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level respectively.

	(2)	(3)	(4)	(5)
	Work-hour personal transaction dummy			
1_{post}	0.0114**	0.0134**	0.0199***	0.0215***
	(2.17)	(2.34)	(3.53)	(3.66)
1_{post} × High credit limit	0.0120***	0.0114***		
	(5.72)	(5.29)		
1_{post} × Multiple homes			0.0381***	0.0388***
			(5.15)	(4.66)
Individual FE	Y	Y	Y	Y
Year-month FE	N	N	N	N
Industry year-month FE	Y	Y	Y	Y
Employer type year-month FE	Y	Y	Y	Y
Observations	102,595	102,595	102,595	102,595
R-squared	0.319	0.322	0.319	0.322

TABLE 9. CHANGE OF CREDIT CARD TRANSACTION BEHAVIOR IN OTHER HOURS

This table shows the response in non-work-related transactions during other hours of work days for the employed individuals in our sample. The dependent variable in columns 1 and 2 is a dummy variable equal to 1 if an individual ever had a non-work-related transaction between 12pm and 2pm of work days in a given month, and 0 otherwise. The dependent variable in columns 3 and 4 is a dummy variable equal to 1 if an individual ever had a non-work-related transaction between 8am and 9am or between 5pm and 6pm of work days in a given month, and 0 otherwise. The dependent variable in columns 5 and 6 is a dummy variable equal to 1 if an individual ever had a non-work-related transaction between 6pm and 9pm of work day in a given month, and 0 otherwise. We exclude the event month observations for the treatment group from our analysis. Please refer to Table 3 and Appendix A for detailed variable definitions. Standard errors are clustered at the city level. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Personal transactions dummy					
	Lunch hours (12-2pm)		Early and late hours (8-9am, 5-6pm)		Overtime hours (6-9pm)	
1_{post}	0.0047	0.0063	0.0106**	0.0114**	0.0215***	0.0222***
	(0.97)	(1.26)	(2.19)	(2.39)	(5.66)	(5.91)
Individual FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	N	Y	N	Y	N
Industry year-month FE	N	Y	N	Y	N	Y
Employer type year-month FE	N	Y	N	Y	N	Y
Observations	194,036	194,036	194,036	194,036	194,036	194,036
R-squared	0.240	0.242	0.199	0.201	0.281	0.283

TABLE 10. DIFFERENCES IN RESPONSE BY AGE

This table shows the response heterogeneity by age in the sample of employed individuals. We exclude the event month observations for the treatment group from our analysis. Please refer to Appendix A for detailed variable definitions. Standard errors are clustered at the city level. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level respectively.

	(1)	(2)	(3)	(4)
	Work-hour personal transaction dummy			
1_{post}	0.0155***	0.0161***	0.0155***	0.0161***
	(3.01)	(3.18)	(3.01)	(3.18)
$1_{post} \times$ Older	0.0219***	0.0215***	0.0200***	0.0188***
	(6.39)	(6.65)	(5.62)	(6.20)
$1_{post} \times$ Older SOE employee			0.0269***	0.0395***
			(4.97)	(3.91)
Individual FE	Y	Y	Y	Y
Year-month FE	Y	N	Y	N
Industry year-month FE	N	Y	N	Y
Employer type year-month FE	N	Y	N	Y
Observations	194,036	194,036	194,036	194,036
R-squared	0.319	0.320	0.319	0.320

INTERNET APPENDIX
(NOT INTENDED FOR PUBLICATION)

TABLE IA.1 CORRELATION BETWEEN HOUSE PRICE GROWTH AND WORK-TIME CREDIT CARD TRANSACTION BEHAVIOR

This table shows the results on the correlation between an (employed) individual's propensity to have work-hour personal transactions (using credit cards) in a given month and the past month's local house price growth between 2008:01 and 2009:10. We use the change in the monthly house price index, developed by Fang, et al. (2015), to measure the house price growth at the city level. The analysis sample covers credit card holders in 110 Chinese cities where house price index data can be merged with our data. Please refer to the Appendix A for variable definitions. Standard errors are clustered at the city level. T-statistics are reported in parentheses under the coefficient estimates. Significant at *** 1%, **5%, and *10%.

	(1)	(2)	(3)	(4)
	Work-hour personal transactions dummy			
Lagged change in house price index	0.0401	0.0413*	0.0405*	0.0420*
	(1.66)	(1.72)	(1.69)	(1.75)
Individual FE	Y	Y	Y	Y
Year-month FE	Y	N	N	N
Industry year-month FE	N	Y	N	Y
Employer type year-month FE	N	N	Y	Y
Observations	1,223,708	1,223,708	1,223,708	1,223,708
R-squared	0.336	0.336	0.336	0.336

TABLE IA.2 "LAND KING": DETAILS OF THE WINNING LAND PARCELS

The following table shows the details of the winning land parcels used in our analysis. Panel A describes the three residential land parcels that broke the nation-wide record of unit price in land auctions between 2008:01 and 2009:10 in China. Panel B shows results of the regression on the predictability of the Land King events based on past house prices. We use the monthly house price indices for 120 Chinese cities, developed by Fang, et al. (2016) using a proprietary dataset on mortgage loans, between 2008 and 2009. *Land king shock* is a dummy equal to 1 for the announcement months of the three treated cities (Shanghai, Hangzhou, and Xiamen). *Price index*$_{-1m}$ is the house price index in the last month, *Price index*$_{-2m}$ is the house price index with a two month lag, and *Price index*$_{-3m}$ is the house price index with a three month lag. *Price change*$_{-1m}$ is the change in house price index for the last month, and we define the two other price change variables in a similar way. Robust standard errors are included. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level respectively.

Panel A

City	District	Transaction Date	Total Price(RMB, mils)	Unit Price (RMB/m^{2})
Shanghai	Changning	August 27, 2008	328	24,118
Hangzhou	Shangcheng	August 18, 2009	778	24,295
Xiamen	Simei	September 8, 2009	1,047	30,940

Panel B

	Land king shock (= 1)		
	(1)	(2)	(3)
Price index$_{-1m}$	-0.0065		
	(-0.77)		
Price change$_{-1m}$	0.0136		
	(1.30)		
Price index$_{-2m}$		-0.0080	
		(-0.91)	
Price change$_{-2m}$		0.0104	
		(1.38)	
Price index$_{-3m}$			-0.0080
			(-0.91)
Price change$_{-3m}$			0.0024
			(0.52)
City FE	Y	Y	Y
Year-month FE	Y	Y	Y
Observations	2,855	2,854	2,854
R-squared	0.082	0.082	0.082

TABLE IA.3 ALTERNATIVE PRE-SHOCK WINDOWS

This table repeats the same analysis as in Table 3 using alternative pre-shock windows. Column 1 uses a two-month pre-shock window, column 2 uses a three-month pre-shock window, and column 3 uses a four-month pre-shock window. Please refer to Appendix A and Table 3 for detailed variable definitions. Standard errors are clustered at the city level. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level respectively.

	(1)	(2)	(3)
	Work-hour personal transactions dummy		
$1_{-2m,-1m}$	0.0094		
	(1.43)		
$1_{-3m,-1m}$		0.0064	
		(0.76)	
$1_{-4m,-1m}$			0.0092
			(1.14)
1_{0m}	0.0062	0.0060	0.0082
	(1.09)	(0.98)	(1.21)
1_{post}	0.0213***	0.0211**	0.0239**
	(3.26)	(2.55)	(2.56)
Individual FE	Y	Y	Y
Year-month FE	N	N	N
Industry year-month FE	Y	Y	Y
Employer type year-month FE	Y	Y	Y
Observations	200,452	200,452	200,452
R-squared	0.319	0.319	0.319

TABLE IA.4 ALTERNATIVE CONTROL GROUPS

This table shows results of performing the same analysis as in Table 3 using alternative control groups. In Panel A, we use residents in the geographically proximate cities among all unaffected cities as the control group. They include cities in Zhejiang, Jiangsu, Fujian and Guangdong. In Panel B, we use the residents in all unaffected cities in our sample as the control group. Please refer to Appendix A and Table 3 for detailed variable definitions. Standard errors are clustered at the city level. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level respectively.

Panel A: Close-by unaffected cities as the control group

	(1)	(2)	(3)	(4)
	Work-hour personal transactions dummy			
$1_{-1m,-1m}$			-0.0064	-0.0055
			(-1.08)	(-0.87)
1_{0m}			0.0042	0.0038
			(0.98)	(0.96)
1_{post}	0.0179***	0.0178***	0.0165**	0.0168**
	(2.78)	(2.75)	(2.35)	(2.33)
Individual FE	Y	Y	Y	Y
Year-month FE	Y	N	Y	N
Industry year-month FE	N	Y	N	Y
Employer type year-month FE	N	Y	N	Y
Observations	718,551	718,551	724,968	724,968
R-squared	0.336	0.337	0.336	0.337

Panel B: All unaffected cities as the control group (full sample)

	(1)	(2)	(3)	(4)
	Work-hour personal transactions dummy			
$1_{-1m,-1m}$			-0.0050	-0.0046
			(-0.66)	(-0.56)
1_{0m}			-0.0023	0.0002
			(-0.62)	(0.05)
1_{post}	0.0164***	0.0181***	0.0159**	0.0177**
	(2.84)	(2.87)	(2.29)	(2.32)
Individual FE	Y	Y	Y	Y
Year-month FE	Y	N	Y	N
Industry year-month FE	N	Y	N	Y
Employer type year-month FE	N	Y	N	Y
Observations	1,983,855	1,983,855	1,990,272	1,990,272
R-squared	0.323	0.324	0.323	0.324

TABLE IA.5 ALTERNATIVE MEASURES OF WORK-HOUR PERSONAL TRANSACTIONS

This table shows results of performing the same analysis as in Table 3 by changing the measurement of work-hour personal transactions. The dependent variable in column 1 is defined based on a stricter definition of non-work-related transactions, by further excluding spending on dining, bars and clubs, gyms, golf, medical services and other service categories, which are ambiguous in nature. The dependent variable in column 2 is defined based on work-hour spending on retailers, department stores, theatres and spas. The dependent variable in column 3 is the (natural logarithm of) the number of work-hour personal transactions, and the dependent variable in column 4 is the number of work-hour personal transactions, divided by the total number of credit card transactions in the same month. Please refer to Appendix A and Table 3 for detailed variable definitions. Standard errors are clustered at the city level. T-statistics are reported in parentheses under the coefficient estimates, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level respectively.

	(1) Work-hour personal transaction dummy (alternative definition)	(2) Work-hour leisure spending dummy	(3) Log(# work-hour personal transactions)	(4) # work-hour personal transactions/ total # CC transactions
1_{post}	0.0186^{***}	0.0278^{***}	0.0173^{***}	0.0102^{***}
	(3.99)	(6.55)	(3.24)	(3.53)
Individual FE	Y	Y	Y	Y
Year-month FE	N	N	N	N
Industry year-month FE	Y	Y	Y	Y
Employer type year-month FE	Y	Y	Y	Y
Observations	194,036	194,036	194,036	194,036
R-squared	0.321	0.289	0.377	0.298