The Effect of Wealth Shocks on Shirking: Evidence from the Housing Market[*]

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Abstract

This paper studies the effect of housing wealth shocks on workplace shirking. We use the type and actual *time stamps* of credit card transactions to detect non-work-related behavior during work hours. After positive shocks to house prices, affected homeowners experienced a fast and persistent increase (by 19% per month) in their propensity to use work hours to attend to personal needs. The post-shock response is more pronounced among homeowners with a greater wealth increase, with poorer career potential, or for occupations with higher monitoring costs. Our estimate implies an elasticity of shirking propensity with respect to house price of 3.8.

JEL Classification: J22, R3, D1, E21, G21

Keywords: Labor supply, shirking, effort, wealth effect, housing wealth, incentive cost, productivity, credit card, household finance, China

1. Introduction

This paper studies the effect of wealth shocks on an important yet less studied labor supply choice—workplace shirking. The contractual work hours are typically fixed in the short run, and labor market frictions prevent households from easily changing the number of work hours (Chetty, Friedman, Olsen, and Pistaferri, 2011), making on-the-job shirking a natural margin of adjustment for labor supply. Indeed, on-the-job shirking is prevalent in the workplace: a survey conducted by salary.com in 2014 shows 90% of American employees waste time during work hours and close to 70% spend 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.

How do wealth shocks influence workers' shirking incentives? Individuals face the key tradeoffs of a higher utility derived from on-the-job leisure activities (i.e., workplace shirking) and the cost of lower labor income if they subsequently are found shirking. A lower labor income from losing the current job incentivizes effort and discourages shirking (Shapiro and Stiglitz, 1984). A positive wealth shock makes the lower labor income consequence less costly, thereby leading to a positive shirking response. We focus on housing wealth in the paper, given the significant role of housing in a typical household's wealth portfolio—house price shocks generate considerable changes in household wealth. Moreover, the large and persistent housing booms recently experienced by many countries greatly influence macroeconomic fluctuations through a wide variety of transmission channels (e.g., Liu, Wang, and Zha, 2013; Deng et al., 2015; Chen, Liu, Xiong, and Zhou, 2017; Gao, Sockin, and Xiong, 2020). Consequently, the negative impact of house price increases on shirking and work incentives likely imposes large motivational costs for firms and has a direct bearing on the aggregate productivity.

Empirically, measuring shirking behavior at the micro-level is hard due to its elusive nature. Traditional labor supply proxies such as earnings and hours of work are typically observed at a low frequency and are subject to confounding (labor demand) interpretations. More importantly, they do not capture work intensity such as the effort level. To overcome this challenge, we exploit a novel, administrative dataset that allows us to detect non-work behavior during work hours.

We measure time use at work with the precise *time stamp* of confirmed credit card transactions, based on a novel dataset obtained from a leading commercial bank in China with a 10% credit card market share. Credit cards have become an important method of household consumption in China (total credit card spending in 2008-2009 was RMB 1.9 trillion, accounting for more than 15% of total household consumption in China). Credit card transactions and cardholder information are available in a 22-month period between January 2008 and October 2009 for a random sample of the bank's credit card customers. We observe individuals' credit card transaction behavior including the amount, type, location, and exact time of each credit card swipe. We propose a novel shirking measure based on the propensity among the employed population to use credit cards for non-work-related transactions at offline stores during work

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

hours. To further control for the unobserved heterogeneity across individuals, we rely on the within-person change in our empirical analysis to identify changes in shirking behavior. The dataset also provides a rich array of information on individual cardholders' demographic and socioeconomic characteristics. Importantly, we observe individual cardholders' homeownership status as well as detailed and verified information on their employment, with which we construct the employed owner sample for the main empirical analysis.

Since the early 2000s, China's housing market has experienced phenomenal and persistent growth that spans many cities (Glaeser et al., 2017). The homeownership rate is over 80% in urban China, and housing equity accounts for two thirds of a typical Chinese household's wealth (Gan et al., 2013). Therefore, China's setting provides a powerful test of the hypothesis on the incentive consequence of house price increases.

We first motivate our analysis by documenting a positive correlation between our measure of shirking and the lagged city-level house price at a monthly frequency. Although the correlation suggests a plausible positive effect, a causal interpretation of the finding faces severe challenges due to the non-random nature of house price movements. Unobserved factors such as local demand shocks may simultaneously drive house price movements and individuals' labor market decisions. To address the challenge, we use multiple identification methodologies. Our main analysis exploits the unique institutional setting in China's land auction market and uses the announcement of land auctions that set nationwide price records for the highest per-unit land price as plausibly exogenous shocks to the house price of the winning land parcel's city.

In China, land supply is determined by local governments via auctions in which developers bid for the land parcels based on their projection of future house prices. Since the 2000s, strong housing demand as well as rising competition among developers has pushed up land prices, sometimes setting record-high prices in the local market. Occasionally, a local historic-high land price also breaks the nationwide record, which is commonly known as the (national) "Land King." Compared with other high-price land auctions, Land Kings are salient events that draw wide media coverage, which in turn triggers an immediate upward adjustment in expectation that quickly gets capitalized in local house prices.

Three of the 2,291 land auctions held in 35 major Chinese cities during our sample coverage period are Land Kings: Shanghai (August 27, 2008), Hangzhou (August 18, 2009), and Xiamen (September 8, 2009). After the Land King announcements, local house prices in these three cities experienced a rapid increase of 5% per month, on average, in our sample period (difference-in-differences regression analysis also confirms a strong post-event house price increase). One crucial identifying assumption of our empirical strategy hinges on the unpredictability of the precise timing and location *pair* of the national record-setting land auctions. Indeed, we show the timing of the three Land King announcements was unanticipated because they were not preceded by abnormally high local house price growth, and the price trajectory exhibits no pre-shock difference between the three Land King cities and other major cities in China under a difference-in-differences framework.

Using the three Land King events as shocks to house prices, our empirical strategy reduces to comparing a tight window around three distinct dates of Land King announcements for each winning city. The staggered treatment dates also help circumvent the confounding influence of common macro trends. At the monthly frequency, we analyze the within-individual response of the propensity among employed homeowners in Shanghai, Hangzhou, or Xiamen to use credit cards for personal transactions during work hours. Matched employed owners in the unaffected cities serve as the control group to estimate the counterfactual trends in macroeconomic conditions.

After the Land King shocks, employed owners in the three shocked cities became 2.5% more likely to use work hours to attend to their personal needs. The coefficient estimates are highly statistically significant at the 1% level. The effect is also economically meaningful: compared with the treatment group's pre-shock mean of 13%, the estimated average monthly response is equivalent to a 19% increase in the propensity. To further sharpen our identification, we explore the within-city variation based on geographical proximity to the land parcels winning the Land King titles. Individuals close to the Land King districts experienced a stronger response than those in the Land King cities but far from the winning districts.

Looking into the dynamics of the response, we observe a fast response, which starts to manifest itself in the first month after the Land King announcements and becomes highly significant statistically in the second month. On the other hand, we find no significant response in the month before the event announcement, consistent with the parallel-trends assumption. The fast and significant response among the treated individuals after the Land King shocks provides additional support for our identification strategy that relies on the exogenous timing of the Land King events.

One concern is that winning the Land King title is correlated with positive local economic shocks, which may increase workers' outside options and thus encourage shirking. The fast response, as discussed above, helps mitigate this concern since wages (and labor demand in general) are unlikely to adjust in such a short window due to their stickiness. We further perform three placebo tests by studying the post-shock response in cities neighboring the Land King cities, the response to other land auctions in Land King cities that feature high transaction prices yet do not win the Land King titles, and the response to the Land King shocks among employed renters. We find no change in the propensity of work-hour personal transaction behavior in these three placebo tests. The main finding is also unlikely to be driven by an outward shift in labor demand as a result of Land King events, since employees in real estate–related industries did not exhibit a stronger response, even though their labor market benefits more from a booming housing market.

In addition, our main finding captures work-behavior response rather than changes in credit-card-use habits since we observe no post-shock change in work-hour personal-transaction propensity among the non-working residents in the three Land King cities. Furthermore, the effect is unlikely to be explained by individuals' decision to quit their jobs after the house price increase given the prevalence of the effect: 68% of the treatment group experienced an increase in their propensity to use credit cards for personal transactions.

We perform a series of heterogeneity analyses to investigate the underlying economic channel. Consistent with the wealth channel interpretation, the increase in work-hour personal transactions is significantly stronger among owners with a larger amount of housing. We also find a significantly stronger response when a reduction in work effort is less costly, for example, among workers facing poorer career potential (e.g., employees reaching retirement age), or when the employer's monitoring cost is higher (e.g., more flexible work hours). The variation in the effect by occupation characteristics suggests our main findings indeed capture the effect on effort incentives. Finally, we discuss the role of asset illiquidity, given the difficulty in accessing housing wealth gains, especially in China's context in which cash-out refinancing is infeasible. Consistent with the idea that an increase in shirking is not constrained by one's current liquidity on hand, the treatment group overall exhibited a significant shirking response regardless of their ability to access housing wealth. On the other hand, only multiple-home owners, who could sell and realize housing wealth gains, increased their spending, consistent with the fact that an increase in spending requires access to the wealth gains.

Finally, we conduct a battery of additional analyses to investigate the robustness of our results. To corroborate the causal interpretation of our empirical design, we follow Ferreira and Gyourko (2011) and Charles, Hurst, and Notowidigdo (2018) and use city-specific structural breaks in house prices as an alternative identification for positive house-price shocks. We again document a positive effect of housing-wealth shocks on employee shirking, which enhances the generalizability of our findings. In addition, we find no evidence that treated individuals switch their timing of shirking by reducing the instance of credit card use during lunch hours or during early/late hours of workdays. Finally, our results are also robust to alternative control groups, alternative measures of shirking, and different model specifications.

A growing literature examines the labor consequences of housing market conditions. Mian and Sufi (2014) find 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 (2018) study how the national boom and bust in the U.S. housing market affect college attendance choices, leading to potential labor-misallocation implications. Sodini et al. (2017) show homeownership has a positive but short-lived effect on earnings, consistent with a debt-induced labor supply increase. We provide the first empirical analysis on the effect of house price increases on work effort incentives. More broadly speaking, our results contribute to the literature on the wealth effect on labor supply by offering novel evidence on the labor supply response through on-the-job shirking (e.g., Imbens, Rubin, and Sacerdote, 2001; Cesarini et al., 2017). Our main estimate suggests a 19% monthly increase in shirking propensity in cities that experienced a 5% post-shock monthly increase in house prices. This finding implies an elasticity of shirking propensity with respect to house price of 3.8.

We also add to the broad literature on the real impact of the housing market dynamics. Prior studies find a significant effect of housing wealth on household consumption and borrowing (Campbell and Cocco, 2007; Gan, 2010; Browning, Gørtz, and Leth-Petersen, 2013; Mian, Rao, Sufi, 2013; Agarwal and Qian, 2017; Sodini et al., 2017). Others focus on housing as a transmission channel to other real economic outcomes (Lustig and Van Nieuwerburgh, 2005, 2010; Mian and Sufi, 2009, 2011; Chaney et al., 2012; Liu et al., 2013; Deng et al., 2015; Bhutta and Keys, 2016; Chen et al., 2017; Di Maggio et al., 2017; Stroebel and Vavra, 2019; Huang et al., 2019). The results in this paper point out the need to consider the distortionary effect of house price increases on effort incentives.

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 main empirical results on the post-shock response. Section 5 discusses the economic mechanism. Section 6 shows additional robustness results. Section 7 concludes.

2. China's Housing Market

2.1 Background information

China is the largest developing economy with a rapidly growing housing market. A milestone reform event happened in 1998 with the issue of the 23rd Decree: housing was no longer welfare oriented, and the objective was to build a private housing market. Thereafter, 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. This change introduced 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 (NBS), investment in China's real estate sector was 30 trillion Chinese yuan (4.5 trillion USD) in 2008, equivalent to a 21% increase over the previous year's investment.

China's housing market has since experienced phenomenal growth (Liu and Xiong, 2020). According to statistics from the NBS, the average transaction price in the country increased by more than 200% from 2000 to 2015 (see Figure IA.1, Panel A, of the Internet Appendix). 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 a 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 IA.1, Panel B, of the Internet Appendix). Therefore, China's housing market appears to grow at a faster rate, with a persistent trajectory over the last 20 years.

Great heterogeneity also exists 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 of 120 Chinese cities between 2003 and 2007 in Figure IA.2 (in the Internet Appendix), based on the house price indices estimated by Fang et al. (2015). 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.

Housing plays a significant role in a typical household's wealth portfolio in China. The homeownership rate is over 80% in urban China, and almost 20% of urban households own two or three houses (Gan et al., 2013). In addition, housing equity accounts for two thirds of a typical Chinese household's wealth (China Household Finance Survey, 2018). Importantly, Chinese households do not have easy access to their housing collateral; cash-out refinancing is not allowed in China, and selling is the predominant method to access housing wealth.

2.2. Land auctions in China

Land supply is determined by the local governments, and land sales constitute a major source of their revenues (Huang, Pagano, and Panizza, 2020). One important characteristic of China's recent housing market growth is the emergence of public land listing and an auction system to determine land prices. The first land auction in China was held in Shenzhen in 1987. However, from 1987 to 2004, no public auctions of land parcels occurred. 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, 2015). All developers were required to bid at land auctions based on their assessment of the local housing demand and projection of future house prices. By taking into account the land costs in their profit-maximization problem, real estate developers do 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.

Since China liberalized its real estate market in the 1990s, strong housing demand as well as rising competition among developers has pushed up land prices, sometimes setting record-high prices in the local market (either in terms of total price or unit price). Occasionally, the local historic-high land price also breaks the nationwide record, and the winning land parcel is commonly known as the (national) "Land King". Land Kings are salient events that draw wide media coverage and trigger an immediate upward adjustment in the expectation of local house prices. Local news reports of Land Kings prompt households to raise their expectation of future house prices, which in turn is quickly capitalized into local house prices. For example, in 2007, transaction prices in the local housing market experienced a 17% increase within two months of a Land King announcement in Chongqing, and another Land King in Nanjing was followed by a 7% price increase in the nearby neighborhood within three days of the announcement.

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 China's credit card market. Our dataset contains the monthly credit card statement information of the entire population of over 22 million credit card accounts, from 2004 to 2012, and covers information on monthly credit card spending, payments, and balances.

The dataset also contains the transaction information of each credit card account in a 22-month period from January 2008 to October 2009, 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. Our analyses focus on this 22-month period, which is the same coverage as the transaction-level dataset, because we rely on transaction-level information to identify shirking activities (which we discuss in detail in section 3.2). In addition, we obtain a rich set of demographic and socioeconomic characteristics of a random sample of the population of credit card holders. Besides information on user demographics such as age, gender, ownership status, educational level, marital status, income, and approved credit limit, we also observe detailed employment information, including employment status, employer industry (15-industry classification), employer type (government, SOE, or private sector), occupation, and position rank. We use the bank's homeownership-status information to construct the owner sample to study the housing wealth effect.

Our data offer several advantages. First, our sample covers a large panel of consumers from all 31 provinces and municipalities in mainland China. This coverage allows us to capture the representative behavior of credit card users in China, who are more likely to live in urban areas and have a higher level of wealth—the subpopulation particularly relevant for studying the effect of housing wealth on workplace shirking behavior. In addition, credit cards had become an important payment instrument in China during our sample period: total credit card spending in 2008-2009 was RMB 1.9 trillion, accounting for 15.4% of total household consumption 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. We can thus observe individuals' behavior, including the time of their credit card transactions, with a high degree of granularity.[2] Such rich and high-frequency data empower our identification of the effect of house prices on work-time shirking behavior.

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 than traditional survey-based data sources. 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

[2] To validate the accuracy of the timing of credit card transactions as recorded by the bank, we take a snapshot of the bank-sent confirmation text message after a randomly chosen credit card transaction. We compare the time of the transaction as stated in the text message with information on the same transaction recorded in the end-of-cycle credit card statement (i.e., the information that we observe in the data). The two records contain the same information about the transaction, including the precise time of the transaction.

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% of the credit card holders in our analysis sample opened their accounts with the bank less than two years before our test period. As a result, we observe the account holder's demographics including their employment status and employer type with precision.[3]

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. Because 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 0 otherwise.[4]

Work hours are defined as 9 am – 12 pm and 2 pm – 5 pm on workdays. We note the presence of variation across employers or across regions on the actual work hours—some may start at 8 am, whereas others end at 6 pm (or even later). Moreover, lunch hours likely exhibit cross-sectional heterogeneity as well. We choose this definition of work hours to avoid ambiguity and measurement errors, because 9 am – 12 pm and 2 pm – 5 pm describe work time with greater certainty (we also explicitly study the credit-card-transaction behavior during other hours of workdays in 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 traveling for work purposes. Therefore, we do not classify these out-of-town transactions as work-hour transactions.

We classify credit card transactions based on the merchant categories provided by the bank. To illustrate, Table IA.1 in the Internet Appendix provides a breakdown of all the card transactions in our credit-card-transaction dataset. We find 68.32% of transactions are spent on goods and services, and the remaining 31.68% of 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. Moreover, warehouse retailer, (onsite) payment of financial services, department store, fee payment, and restaurant are the top five credit-card-transaction types

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[3] 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 for 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 (e.g., homeownership, education, employer, occupation, and position rank) and offers a more informative indicator of the cardholder's wealth.

[4] We use the dummy variable as the main measure because the number of credit card transactions in a month is around four or five in the overall sample, but less than one transaction, on average, occurs during work hours and for non-work-related reasons (see Table 1). This finding suggests studying the extensive margin (with the dummy variables) captures the first-order effect. In section 6.2, we also study the robustness of our results with respect to our shirking measure with several alternative definitions.

according to the internal bank classifications. Notably, the number (amount) of online transactions accounts for less than 0.6% (1.4%) of all credit card transactions in our dataset. This finding is consistent with the aggregate statistics showing online shopping only accounted for about 1% of the aggregate household expenditure in 2008–2009.[5] Throughout our analyses, we only consider onsite credit card transactions when constructing our shirking measure.

To account for the possibility that some credit card transactions do not reflect shirking behaviors, we focus on transactions of personal spending on goods and services. Specifically, we exclude the following categories of transactions: (1) spending on hotels, transportation, training expenses, dining, bars and clubs, gyms, and golf; (2) spending on medical services and other service categories (e.g., utility bill and tax payments), and transactions at financial institutions (e.g., OTC transactions at the bank). The first category is likely associated with work-related purposes. The second category could capture personal businesses that do not necessarily reflect shirking intentions. In the robustness check, we also consider a stricter definition of credit card transactions for personal needs by focusing on leisure-oriented spending transactions only, including spending at retailers, department stores, theatres, and spas.

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 high frequency.[6] On the other hand, we cannot detect the exhaustive list of shirking behavior, because our credit card data do not capture other shirking methods such as spending time on personal phone calls or social media at the workplace. 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), and others work at odd hours (e.g., doctors and nurses). As a result, comparing the measure across individuals may confound the interpretation. To alleviate the influence of these measurement errors on the interpretation, we rely on exogenous variations 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 January 2008 and October 2009. The reason is that those account holders unlikely rely on those credit cards for their daily activities, and any variation in the card use (or lack thereof) is uninformative of their economic behavior. We restrict our focus to the top 300 Chinese cities (by population) because the remaining cities are small and non-representative with few credit card accounts. To study the workplace incentive 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

[5] Source: http://news.iresearch.cn/Zt/89409.shtml

[6] 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.

house prices on the working population (we do not observe demographics and employment information for supplementary cardholders). Thus, the final sample comprises a monthly panel between January 2008 and October 2009 for 209,148 credit card holders. Among these cardholders, the main analysis focuses on 106,495 employed homeowners (corresponding to 5.0 million credit card transactions).

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, using the house price index of 120 Chinese cities estimated by Fang et al. (2015). In our sample, we can identify 115 of the 120 cities in Fang et al. (2015), after which we examine whether an employed homeowner's propensity to conduct a personal transaction during work hours is associated with the previous month's local house price. The preliminary results indeed suggest a significant positive relationship between local house prices and consumers' likelihood of using credit cards for personal purposes during work hours (see Table IA.2 in the Internet Appendix).

Although 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 movements. 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 into high house-growth areas that tend to have better amenities, leading to a downward bias of the effect of house prices on shirking. Our measure's inability to completely account for shirking can also contaminate the interpretation of this result, 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 announcements of Land Kings. Compared with many other high-price land auctions, the occasional instances of nation-record-breaking Land Kings are salient events that draw wide media coverage, triggering an immediate upward adjustment of expectation and house prices. Therefore, the announcement of a Land King is a plausibly exogenous shock to the house price of the winning land parcel's city.

Among the 2,291 land auctions held in 35 major Chinese cities during our sample period (January 2008—October 2009), three Land King events occurred: Shanghai (August 27, 2008), Hangzhou (August 18, 2009), and Xiamen (September 8, 2009).[7] Based on the house price index estimated by Fang et al. (2015), we find the three shocked cities experienced a significant increase in house prices during the same post-shock period (as our main analysis window), with an average monthly appreciation rate of 5%. We also conduct a diff-in-diff analysis and find the same result—the three Land King winning cities experienced a large and statistically significant growth in house price in the post-shock period, relative to the price change in unaffected cities (see Table IA.3 in the Internet Appendix for details).

[7] More details of these land auctions are described in Panel A of Table IA.3 in the Internet Appendix.

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 IA.2 shows the distribution of house price growth from 2003 to 2007 among 120 major Chinese cities. Shanghai, Hangzhou, and Xiamen are not among the highest house-price-growth cities in the four-year period before the Land King events. Furthermore, forecasting the exact month of these Land King announcements is arguably difficult. In this regard, our analysis in Table IA.3, Panel B, provides supporting evidence. Past house price levels or growth rates (up to three-month lag) cannot predict the occurrence of Land Kings in the three cities used in our analysis. In the diff-in-diff analysis on the city-level house prices (in Table IA.3, Panel C), we also confirm the parallel trend: prior to the Land King announcements, the price trajectory exhibits no difference between the three Land King cities and other major cities in China.

We further test the identifying assumption by (1) studying the parallel-trends assumption in the work-hour personal transaction behavior among the treatment group, (2) exploiting the high-frequency nature of our data to show the response in a short window after the shocks, and (3) performing two placebo tests. Lastly, in section 6.1, we verify the robustness of our causal interpretation by using an alternative identification strategy based on city-specific structural breaks in house prices (Ferreira and Gyourko, 2011; Charles, Hurst, and Notowidigdo, 2018).

3.4. Empirical specification

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

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

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

The dependent variable, $Y_{i,t}$, refers to our main measure, *Work-hour personal transaction dummy*, which is a dummy variable equal to 1 if individual *i* ever uses 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 1 in the post-shock months for treated individual *i*, and 0 otherwise.[8] 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 the time-varying trend in labor market conditions for each industry

[8] Because the Land Kings were announced in the middle of the month, we are unable to assign the event month as either a pre- or post-shock month. Therefore, unless stated otherwise, we exclude the month of announcement when estimating equation (1).

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.[9]

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

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

where $D_{i,(-1m,-1m)}$ is a dummy variable equal to 1 for the pre-shock month if individual *i* is in the treatment group, and 0 otherwise. Specifically, it takes a value of 1 for July 2008 if *i* is in Shanghai, or for July 2009 if individual *i* is in Hangzhou, or for August 2009 if individual *i* is in Xiamen. For the treatment group, the absorbed period is from the beginning of the sample period (January 2008) 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. The validity of our identification strategy requires parallel trends; that is, 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.

To visualize the dynamics of the shirking response and better examine the parallel-trends assumption, we also plot the estimates of the following distributed lag model:

$$Y_{i,t} = \delta_t + \alpha_i + \sum_{s=-1}^{12} \beta_s \times D_{i,s} + \epsilon_{i,t} , \quad (3)$$

where $D_{i,s}$ is a dummy variable equal to 1 for the s^{th} month after the Land King event for a treated individual *i*, with *s* ranging from -1 (i.e., the month before the Land King event) to 12 (i.e., the 12$^{\text{th}}$ month after the Land King event). The coefficient β_s measures the shirking response in the s^{th} month after the Land King events, relative to the benchmark period (i.e., from the beginning of the sample period to two months before the shock).[10]

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

3.5. Summary statistics

[9] One may be concerned about persistence in our shirking measure, which in turn biases our estimates. We formally examine this hypothesis by estimating the AR(1) process of the shirking measure, controlling for individual and year-month fixed effects and correcting for the Nickell's bias using the Arellano and Bond (1991) method. We find no evidence of persistence in our shirking measure.

[10] The staggered nature of the shocks, combined with our total sample period (January 2008–October 2009), suggests the longer-term response beyond the third month after the shocks is identified by comparing consumers from Shanghai with consumers from all other consumers in the control group, because the Land King events in Hangzhou and Xiamen occurred in August and September of 2009, respectively.

Table 1, Panel A, provides summary statistics of demographics and credit card activities for the treatment and control groups in our sample. The treatment group (employed owners in Shanghai, Hangzhou, or Xiamen) is noticeably different from the control group (employed owners in other cities). On average, the treatment group is 35.6 years old and 0.2 years older 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 (76% vs. 86%). Individuals in the treatment group have an average credit limit close to RMB 12,500 higher than the control group (in relative terms, the difference is 116% of the control group's average credit limit). The treatment group is also more likely to hold a college degree or above than the control group (45% vs. 40%), has a greater fraction of individuals working in the private sector (80% vs. 58%), or holds senior ranks (52% vs. 38%). The differences are economically meaningful and statistically significant. To the extent that labor market choices (e.g., shirking) plausibly differ by wealth and employment characteristics, one legitimate concern arises regarding whether the control group captures a valid counterfactual in the estimation.

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 and demographic characteristics, including age, a quadratic polynomial of credit limit, female dummy, marital status dummy, college dummy, a set of dummies for the employer type (government, SOE, or private sector), and a dummy indicating a senior-rank position at work. 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 1.

After matching, the differences between the treatment and control groups in all variables become statistically insignificant and economically small. In addition to the mean statistics, we also compare the distributions of the two continuous variables between the treatment and the matched control groups. Both age and the credit limit (at account opening) have a similar and comparable distribution between the treatment and the matched control group (see Figure IA.3 in the Internet Appendix). In sum, we have a panel of observationally similar treatment and control groups, which facilitates a more precise estimate of the counterfactuals and identification of the treatment effect in our analysis. We 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 group. In our analysis, we explicitly test for the parallel-trends assumption in the pre-shock period. In section 6.4, we also verify the robustness of our results with alternative counterfactual groups.

Finally, we compare the credit card activities between treatment and control groups in Panel C of Table 1. During our sample period, cardholders in the treatment group charge an average of 4.8 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 4.6 (in the matched sample). Thirteen 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 of 20% 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 estimate equation (1) and report the results in the first two columns of Table 2, Panel A.

Column 1 shows the regression results by including individual and year-month fixed effects. After the Land King shocks, employed homeowners in the three shocked cities become 2.5% more likely to use their credit cards for personal transactions during work hours. The coefficient estimate is statistically significant at the 1% level. The effect is economically meaningful: compared with the treatment group's pre-shock mean of 13%, the estimated average response is equivalent to a 19% increase in the propensity. To allow time trends to vary by industry or by employer type, we include industry-specific and employer-type-specific time fixed effects in column 2. We continue to find a significant response after the Land King shocks among the treatment group under this more restrictive specification.

Next, we test the parallel-trends assumption by estimating equation (2). Results are reported in columns 3 and 4 of Table 2, Panel A. Using both specifications (with different sets of 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. A formal F-test of the difference between β_{post} and β_{pre} rejects the hypothesis that the two coefficients are equal (*p-value* < 0.001 in both columns). Collectively, the results provide support for our identifying assumption: the treatment group exhibits no difference in its pre-event behavior and only increased its propensity to have non-work-related credit card transactions during work hours in months *after* the Land Kings were announced.

To further sharpen our identification, we explore within-city variation by classifying treated homeowners based on their geographical proximity to the land parcels winning the Land King titles. The idea is that within the Land King city, districts of the winning land parcels or those adjacent to the Land King winning districts would experience a stronger house price increase. Thus, the Land King shocks should have a greater impact on owners close to the winning land parcels relative to the treated owners far from the winning districts. The within-city response variation also helps alleviate concerns of city-level unobservables contaminating our main finding. These three treated cities have 12 districts on average. We identify the district of the

Land King land parcel in each city as well as its bordering districts (within a 10 km radius) as the districts with greater exposure to the Land King shocks.

As shown in Panel B, treated owners located in or close to Land King districts show a significantly larger increase in their shirking propensity than owners in Land King cities but far from Land King districts. The differences in the response magnitude of these two groups of treated individuals (captured by the estimate of 1_{post}* *Land King District*) are statistically significant at the 1% level for all four columns. Based on the estimate in column 1, the response magnitude of Land-King-district consumers is 80% larger than that of treated consumers in non-Land-King-districts.

4.2 Dynamics of the response

We now investigate the timing of the response by estimating equation (3). We plot the estimated coefficients along with the 95% confidence intervals in Figure 1. In this analysis, we control for individual fixed effects and allow for industry- and employer-type-specific year-month fixed effects. Consistent with the static regression results, we observe no difference in the treatment group 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 0.9% more likely to use credit cards for personal transactions during work hours, equivalent to a 7% increase relative to the treatment group's pre-shock mean. In the next month (s=2), the coefficient estimate further increases to 0.016 and becomes statistically significant at the 1% level, implying a 12% increase in work-hour personal-transaction propensity relative to the treatment group's pre-shock mean. The fast and significant response among the treated individuals after the Land King shocks provides additional support for our identification strategy that relies on exogenous timing of the Land King events. Moreover, wages are unlikely to adjust in such a short window due to their stickiness, facilitating an interpretation of a decrease in effort incentives. Lastly, the effect remains persistent throughout the 12-month post-shock period.

4.3. Exploring confounding local economic factors

One may be concerned that winning the Land King title is correlated with positive local economic shocks, which may also increase workers' outside options and thus encourage shirking (Shapiro and Stiglitz, 1984; Lazear, Shaw, and Stanton, 2016). In this subsection, we conduct three sets of placebo tests to mitigate this concern.

4.3.1. Post-shock response in cities neighboring the Land King cities

The first placebo test looks at 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 are 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 in the same month of the Land King events, 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 the 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 January 2008 to July 2009 to avoid confounding effects around the second Land King announcement in August 2009 (in Hangzhou). We use the same specifications in equation (1) and report the results in Table 3, Panel A. Using both specifications (with different time fixed effects), we find no significant shirking response to the Shanghai Land King event among employed owners in the neighboring cities. To further alleviate the concern that house price increases arising from Land Kings may spill over to the neighboring cities, we also conduct a series of tests by including a shorter post-event window. Arguably, the spillover effects, if any, are absent or much weaker in the immediate post-event period. In unreported results, we confirm the absence of a response among employed owners of neighboring cities in the more immediate window after Shanghai’s Land King announcement.

4.3.2. Response to other high-price land auctions

The second placebo test exploits another set of high-price land auctions in the same three cities that set city-specific historic-high land prices yet did not break the nationwide record. Because these land auctions also feature high land prices, confounding factors related to high land prices (e.g., local economic shocks) should predict a positive effect as well. However, setting a local historical high is far less salient than winning the national Land King title and thus unlikely to trigger an immediate expectation update and price response.

Specifically, we trace the land auctions over time in Shanghai, Hangzhou, and Xiamen to identify the last land auctions, before the Land King events, that set the city-specific historic-high land prices. The three placebo events occurred shortly before the Land King events (i.e., all within seven months of the Land King shocks)—January 2008 for Shanghai, July 2009 for Hangzhou, and June 2009 for Xiamen.

We study the work-hour personal-transaction propensity for the treatment group after the placebo events (up until the month of the Land King shocks). The results, shown in Table 3, Panel B, reveal no response after the placebo land auction events. Regardless of the fixed-effects specification, the coefficient of interest is both statistically insignificant and economically negligible.

4.3.3. Response among employed renters

Lastly, we examine the response among employed renters in the Land King cities. Without housing asset holdings, renters are not directly exposed to house price shocks through the wealth channel. However, they face the same local economic environment as homeowners. If our main finding is driven by confounding local economic factors, they should impose a positive effect on renters as well.

As shown in column 1 of Table 3, Panel C, renters in the treatment sample on average were unresponsive, with a coefficient estimate close to zero. Interestingly, renters located in or close to Land King districts experienced a significant decrease in the propensity to have non-work-related credit card transactions during work hours after the Land King announcements, as shown in column 2. This result is consistent with the interpretation that a positive shock to the house price increases the cost of living for renters, which encourages them to work harder and reduces their non-productive use of work hours.

4.4. Exclusion restriction: Change in labor demand following Land King events

Another plausible confounding interpretation is a change in local labor demand following the Land King events. For example, after positive house price shocks, the labor demand curve likely shifts outward due to the development of real estate and related industries (e.g., Cappelli and Chauvin, 1991; Burda, Genadek, and Hamermesh, 2016; Charles, Hurst, and Notowidigdo, 2019). More employment opportunities reduce the cost of shirking, which could also explain the findings documented in Table 2. Alternatively, local non-real-estate companies may endogenously respond to the more optimistic housing market by changing their business focus, which may also affect the work effort of their employees (e.g., Deng et al., 2015; Chen et al., 2017). As a result, shirking may respond directly to labor market demand rather than to house price changes.

We describe two sets of results suggesting the labor demand channel is unlikely to explain our main findings. First, as shown in Figure 1, we observe an immediate response after the shocks, which is difficult to reconcile with the labor-demand-response interpretation due to the sluggishness of labor market adjustments. Second, we examine the heterogeneous response among workers in the real estate–related industries and the rest. The labor demand channel predicts a stronger effect for the real estate–related industries, which have greater exposure to the Land King shocks. However, as shown in Table 4, we find no difference in the shirking responses between these two groups of treated individuals. The *p*-values of the interaction-term coefficients are 0.951 and 0.898, respectively, in columns 1 and 2.

4.5. Does the result capture shirking response?

4.5.1. Behavioral change in credit card use?

We interpret the increasing propensity of non-work-related credit card transactions during work hours as the treatment group's work-effort response to house price shocks. However, the Land King shocks could have triggered other behavioral changes in credit card use among the treated individuals. For example, they or their spouses may start 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 those in Shanghai, Hangzhou, and Xiamen who are retired or unemployed. The control group comprises retirees and the unemployed in the full sample of the unaffected cities.[11] As shown in Table 5, the non-working population in the shocked cities experienced no change in their work-hour personal transaction propensity after the Land King events. The estimated coefficient for β_{post} is negative but statistically and economically indistinguishable from zero.

4.5.2 On-the-job shirking or completely quitting the job?

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. Refer to Appendix B for the 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 2. More than 68% of the treatment group display a propensity increase after the shocks, suggesting the positive post-shock response among the treated individuals is prevalent and not driven by outliers. Additionally, the average post-shock change is 2.0%, which is largely consistent with the regression coefficient reported in Table 2, column 1.

Figure 2 helps sharpen the interpretation of the documented effect. An alternative labor supply response arises from the decision to quit the jobs completely to enjoy life, given the housing wealth windfall. Then, the observed effect could be due to their off-work activity rather than a distraction on the job. However, this explanation seems implausible given the prevalence of the post-shock response. 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.

5. The Economic Mechanism

[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 scenario is less likely in our setting because close to 92% of cardholders 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; therefore, the analysis is less subject to the measurement error. We find consistent results both qualitatively and quantitatively.

In this section, we explore the economic mechanism underlying the significant post-shock response. On the one hand, an employee derives a higher utility from on-the-job leisure activity (i.e., workplace shirking). On the other hand, while elusive, shirking activities may eventually be detected by the employer, causing a reduction in the future labor income. An increase in (housing) wealth helps offset this potential labor income loss and thus enables an owner to shirk more. It follows that a higher level of initial housing asset holding implies a larger exposure to the positive house price shock, strengthening the wealth effect. A greater cost of shirking, for example, due to a larger labor income loss once detected shirking or a higher monitoring intensity by the employer, mutes the benefit of a given amount of housing wealth increase and thus dampens the shirking response. We formally examine these hypotheses in the subsections below.[12]

5.1. The role of housing wealth

We start with the heterogeneity in shirking response by the levels of initial housing wealth, which serves as a direct test on the wealth channel interpretation. Following a positive house price shock, owners with a greater housing asset holding enjoy a larger increase in wealth, which can better hedge the potential labor income loss induced by shirking. Therefore, we expect a stronger shirking response among owners with greater housing wealth.

We use the granted credit limit as a proxy for housing wealth, which is a composite measure of credit card applicants' creditworthiness by considering multiple demographic and socioeconomic characteristics such as income, education level, marital status, asset ownership, and employment information (Gross and Souleles, 2002; Agarwal and Qian, 2014). Column 1 of Panel A, Table 6, shows the differential responses by varying levels of credit limits. We find a statistically and economically more significant effect among owners with a higher credit limit. We also use ownership of multiple houses as an alternative proxy for housing wealth (details on the multiple-home-owner measurement are provided in Appendix A). Consistently, multiple-home owners exhibit a significantly stronger treatment effect (shown in Column 2).

5.2. The role of the cost of shirking

We then exploit the cross-sectional heterogeneity in the cost of shirking. As discussed above, when the cost of shirking is higher, the extent to which a given amount of housing wealth appreciation can help offset shirking costs become lesser. Therefore, we expect a weaker shirking response among employees facing a higher cost of shirking.

We use the proximity to retirement age as a proxy for the cost of shirking. As one approaches retirement age, the upside potential for income growth and promotion diminishes quickly, rendering their workplace effort less relevant for their future labor income. Consistent with our

[12] We also lay out a simple theoretical framework that captures the key tradeoffs households face and characterizes their optimal decisions in response to wealth shocks (refer to Section A of the Internet Appendix for details). Although highly stylized, this model can generate comparative statistics consistent with our evidence on the heterogeneous shirking and spending responses with respect to housing asset holding, cost of shirking, and housing wealth access.

hypothesis, column 1 of Table 6, Panel B, shows a stronger treatment effect for the older individuals (defined as those in the top quartile of the age distribution among all cardholders in our matched owner sample). We further look at older owners employed in state-owned enterprises (SOEs), who arguably have even weaker career drive due to SOEs' weak pay-performance sensitivity. Consistently, results in column 2 of Table 6, Panel B, show older SOE owners are much more likely to have an increased propensity to use credit cards for personal transactions during work hours than the other older employees.

Another dimension of shirking cost is the likelihood of being detected shirking. Shirking increase is more likely when shirking is more difficult to monitor and thus less likely to be detected. We use occupation to proxy for the variation in the likelihood of being detected shirking. Professional occupations, such as lawyers, accountants, and teachers, typically have less stringent rules regarding work hours and thus entail higher monitoring costs from employers' perspectives. Arguably, they face a lower probability of being detected shirking. Consistently, we find an even greater response among employees in professional occupations (Table 6, Panel C, column 1). We also restrict the sample to workers who do not hold a high rank, to better isolate the monitoring-intensity channel (by focusing on a sample of individuals with more comparable wealth levels). As shown in column 2, we find a much greater effect among low-rank employees in those occupations as well.[13]

Moreover, results in this subsection also help mitigate the concern that the treatment group maintained their overall shirking level while increasing their work-hour personal credit card transactions (e.g., by reducing other shirking activities), rendering the overall productivity unchanged. If the treatment effect mainly reflected a substitution across different forms of shirking, the increase in work-hour personal transactions, in lieu of other shirking activities, would be stronger among those facing a higher cost of shirking, because they have the strongest incentive to maintain the overall level of productivity. This is inconsistent with what we observe from the data.

5.3. The role of housing wealth access

Finally, we discuss the role of asset illiquidity, given the difficulty in accessing housing wealth gains, especially in China's context in which cash-out refinancing is infeasible (Waxman et al., 2020). Conceptually, an increase in shirking does not require additional liquidity. Moreover, shirking behavior is unlikely to impose an immediate negative impact on the contemporaneous labor income due to its elusive nature and the rigidity of labor contracts. Therefore, the shirking response does not entail realizing the housing wealth appreciation. In contrast, an increase in spending immediately consumes cash on hand. A significant spending response thus hinges on a feasible access to cashing out the wealth gains.

[13] To further mitigate concerns that wealth-level differences might explain the heterogeneity result with respect to occupation and age, we test and verify that the stronger response among professionals or older (SOE) workers remains to hold in both subsamples of high- and low-credit-limit individuals. These heterogeneity results are robust in the subsample of single-home owners as well.

One natural proxy for the owners' ability to access housing wealth in our context is the multiple-home-owner status, because multiple-home owners can cash out the wealth gains through selling or renting out their property. However, single-home and multiple-home owners may be different from multiple dimensions. Importantly, multiple-home owners tend to hold more housing assets and thus have a larger exposure to the positive house price shock. To tease out the role of housing wealth access, we use propensity score matching to identify single-home owners who are comparable with multiple-home owners in the wealth level (proxied by credit limit) and other characteristics. After matching, the response difference between multiple-home owners and matched single-home owners can be attributable to the asset liquidity effect.

We separately study the shirking and spending response of the treated single-home owners and multiple-home owners. As shown in column 1 of Table 6, Panel D, single-home owners exhibit a significant shirking response, and, importantly, owning multiple houses does not lead to a larger shirking response. The spending responses display a different pattern. On the one hand, single-home owners show no spending response, consistent with the fact that homeowners in China have difficulty cashing out their housing wealth appreciation.[14] On the other hand, multiple-home owners increased their monthly credit card spending by 19% (=exp(-0.0023+0.1752)-1) after the Land King events (shown in column 2 of Table 6, Panel D), with the effect significant both economically and statistically (*p-value*=0.002).

6. Additional Analysis

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

6.1. Using structural breaks in local house prices to identify demand shocks

First, we exploit another empirical identification methodology to identify plausibly exogenous housing-demand shocks. Following prior work by Ferreira and Gyourko (2011) and Charles, Hurst, and Notowidigdo (2018), we estimate structural breaks in local house prices. For each of the 115 cities covered by the monthly city-level house-price-indices data by Fang et al. (2015), we identify the city-specific structural breaks in house prices during the period between January 2003 and August 2014 (i.e., the entire sample period of the constructed house price indices in Fang et al., 2015). Given the length of the sample period, we allow two structural breaks in each city. Such an analysis identifies 28 cities with positive structural breaks in house prices during our 22-month period.

Using both the magnitude of the structural break and the precise timing (year-month) of the break, we carry out an event study of workers' propensity to use work time to take care of personal needs. (The remaining 87 cities are included in the analysis to estimate the

[14] We also study the response at the extensive margin and find that the treatment group show no significant change in their overall propensity of credit card usage after the shock.

counterfactuals.) Because the key independent variables are estimated, we address the inference problem associated with generated regressors by bootstrapping the standard errors (with 1000 iterations). Consistent with our main results, we find a significant post-break increase in the likelihood of observing work-hour personal transactions for the employed homeowners in the treated cities (Table IA.4 in the Internet Appendix). We further confirm the results remain to hold if we exclude the three Land King cities, corroborating that our results are not specific to the three Land King cities. Collectively, these observations enhance the generalizability of our findings based on the identification strategy using Land King events.

6.2. Alternative measures of work-hour personal transactions

We replace our main shirking measure with several alternative proxies (see Table IA.5 in the Internet Appendix). We consider a stricter definition for credit card transactions for personal needs by focusing on leisure-oriented spending transactions, including spending at retailers, department stores, theatres, and spas. We also use work-hour personal-transaction frequency or amount to replace the dummy-variable measure in the main analysis. Using these different measures of work-hour personal transactions, we find the main results still hold.

Lastly, we also construct our main shirking measure using consumers' high-value and low-value transactions respectively. The idea is that more expensive transactions may take a longer time to complete, and thus offer a stronger signal of shirking. We find the increase in the propensity of high-value work-hour personal transactions is significantly larger than the response of the shirking measure based on low-value work-hour personal transactions (the difference between the two estimated responses is statistically significant at the 1% level).

6.3. Intraday decomposition of the shirking response

As discussed in section 5.2, our findings do not reflect the treated consumers substituting work-hour personal transactions for other forms of shirking activities. In this section, we also provide evidence that the treated workers do not shift their work hours during the day. As shown in Table IA.6 in the Internet Appendix, we do not find the treated individuals reduce the tendency to attend to personal spending needs during lunch hours, early and late hours, or overtime hours after the shock.[15] Additionally, the specific pattern of the response within work hours is consistent with an increase in the shirking level: 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 after positive house price shocks.

6.4. Alternative specifications

We consider two alternative control groups to estimate the counterfactuals in our main analysis. First, we use the employed homeowners in cities that are geographically close to the three shocked cities as the control group, including cities in Zhejiang, Jiangsu, Fujian, and

[15] To compare this result with the previous finding on a decrease in the overall credit card use in the non-work hours, we verify that the decrease is driven by credit card use on weekends (and public holidays).

Guangdong provinces. Individuals within proximate geographic regions are likely to share similar preferences regarding credit card use and face the same work-hour norms and other employer preferences. Second, we repeat our main analysis based on the unmatched full sample, using all employed homeowners in the unaffected cities as the control group. As shown in Table IA.7, Panels A and B, both methods produce estimates and inferences very similar to our main result, which validates our matched-sample approach.

We also use various lengths of the pre-event window (from two to four months) to further validate the parallel-trends assumption (reported in Table IA.7, Panel C). The responses during these alternative pre-event windows are economically negligible and statistically insignificant.

Lastly, we use non-linear models to re-estimate our main results in Table 2, Panel A. To avoid the potential incidental parameters problem, we reduce the number of nuisance parameters to be estimated by using city fixed effects and individual-level demographic characteristics (the same set that we include in the propensity-score-matching procedure) instead of individual fixed effects. We find statistically significant results in both logit and probit models at the 1% level, and both models deliver a marginal effect very close to that derived from our main specification using the linear probability model (OLS).

7. Concluding Remarks

In this paper, we study the impact of house price increases on individuals' shirking behavior at work. We use the type and actual *time stamps* of about 5 million credit card transactions of over 100,000 cardholders from a leading Chinese commercial bank 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 a Land King announcement, which serves as an exogenous positive shock to the local house price, employed homeowners in the Land King cities, particularly those close to the Land King districts, experienced a significant increase in the propensity to use credit cards for non-work-related transactions during work hours. As placebo tests, we examine and find no effect among workers in the neighboring, unaffected cities after Land King events, among workers in the treated cities after other high-price land auctions that only break the local records and not the nationwide records, or among employed renters in the treated cities after Land King events. In addition, workers employed in the real estate industry do not show a stronger response, inconsistent with the labor-demand-channel prediction. Our results are also unexplained by the change in credit-card-usage behavior, because non-workers (i.e., retirees and the unemployed) in the Land King cities did not exhibit responses.

Additional heterogeneity analysis shows a significantly stronger response among owners with a higher level of housing wealth, consistent with the wealth channel interpretation. We also find a significantly stronger response among workers facing poorer career potential or when the employer's monitoring cost is higher, suggesting our main findings correspond to a reduction in effort incentives. Finally, while the treatment group exhibited a significant shirking response regardless of the accessibility of the housing wealth appreciation, only multiple-home owners, who could sell and realize housing wealth gains, increased their spending.

The documented increase in shirking is economically significant. Our main estimate suggests a 19% monthly increase in shirking propensity in cities that experienced a 5% post-shock monthly increase in house prices. This estimate implies an elasticity of shirking propensity with respect to house price of 3.8. Overall, our paper points to an understudied yet important labor supply consequence of positive wealth shocks—for example, arising from house price increases—reflected in a distortionary effect on effort incentives.

Appendix A. Variable Definitions

Work-Time Transaction Variables (Derived from Credit Card Data)

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 0 otherwise. We classify credit card transactions based on the merchant categories provided by the bank. We exclude spending items that are potentially related to work, such as hotel, transportation, and training expenses. We further exclude spending on dining, bars and clubs, gyms, golf, medical services, and other service categories (e.g., utility bill and tax payments), and transactions at financial institutions (e.g., OTC transactions at the bank). To define work hours, we focus on 9 am –12 pm and 2 pm – 5 pm 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 at retailers, department stores, theatres, and spas during work hours in a month, and 0 otherwise.

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

Work-hour personal transaction amount provides the amount of non-work-related credit card transactions that occur during work hours in a month for each credit card holder.

Demographic Variables

Age is the individual cardholder's age at the transaction year. ***Older*** is a dummy equal to 1 if the credit card holder's age is in the top quartile of the distribution among all cardholders in our matched owner sample (i.e., above 42), and 0 otherwise.

Female is a dummy variable that equals 1 if the credit card holder is female, and 0 otherwise.

Married is a dummy variable that equals 1 if the credit card holder is married, and 0 otherwise.

College is a dummy variable that equals 1 if the credit card holder obtains a college degree or above, and 0 if below college.

Own is a dummy variable equal to 1 for homeowners, and 0 otherwise. ***Multiple houses*** 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 more than one mortgage or 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 0 otherwise.

Credit limit is the total credit line (in RMB) of all 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 above RMB 10,000, which is the median of the distribution among all cardholders in our matched owner sample.

Employment-Related Variables

SOE is a dummy variable equal to 1 if the credit card holder works in a state-owned enterprises, and 0 otherwise. ***Government*** is a dummy variable equal to 1 if the credit card holder works in a government agency, and 0 otherwise. ***Private*** is a dummy variable equal to 1 if the credit card holder works in a private enterprise, joint venture, or is self-employed, and 0 otherwise.

Professional is a dummy variable equal to 1 if the credit card holder works in professional occupation including lawyers, accountants, and teachers, and 0 otherwise.

High-rank is a dummy variable equal to 1 if the credit card holder holds a senior-rank position at work such as CEO, director, department manager, chief physician, and full professor, and 0 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 0 otherwise.

Unemployed is a dummy variable equal to 1 when the credit card holder enters "unemployed" as the employment status at account opening, and 0 otherwise.

Appendix B. Definition of Change in Propensity of Work-Hour Personal Transactions (in Figure 2)

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.

A. In the matched sample, we residualize the work-hour personal transaction dummy for every in-sample cardholder with respect to individual and year-month fixed effects. The purpose of this step is to remove the time-invariant individual heterogeneity and common time trend.

B. For each month in our sample period, we compute the monthly average of the residualized work-hour personal transaction dummy across individuals in the matched control group.

C. For each individual in the treatment group, we adjust their monthly residualized work-hour personal transaction propensity (computed from step A) by subtracting the same-month average derived from step B.

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

E. 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.

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FIGURE 1. ESTIMATED RESPONSE DYNAMICS

This figure plots the entire path of estimated coefficients b_s, s= -1, 0, ..., 11, 12, from estimating equation (3), along with their corresponding 95% confidence intervals. The x-axis denotes the s^{th} month after the Land King auction, and the y-axis shows the estimated response.

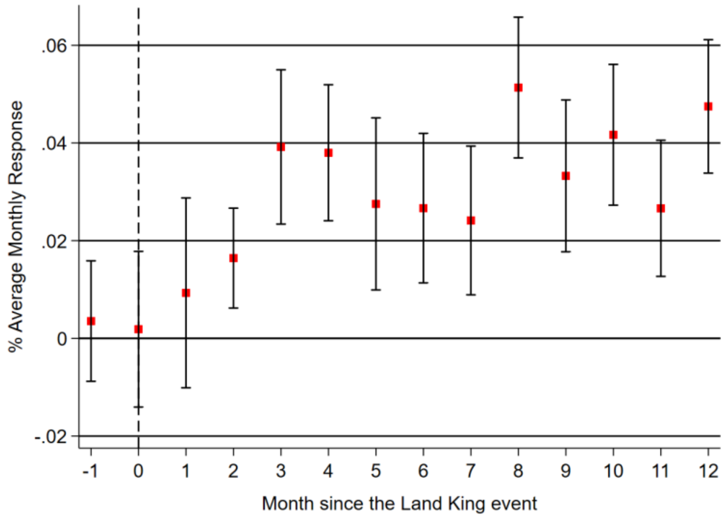


FIGURE 2. DISTRIBUTION OF CHANGE IN WORK-HOUR PERSONAL TRANSACTIONS

This figure plots the distribution of the post-shock change in the propensity of work-hour credit card personal transactions among the employed homeowners in the treatment group. The x-axis shows the change in the propensity of work-hour personal transactions. Refer to Appendix B for a detailed description of the variable construction.

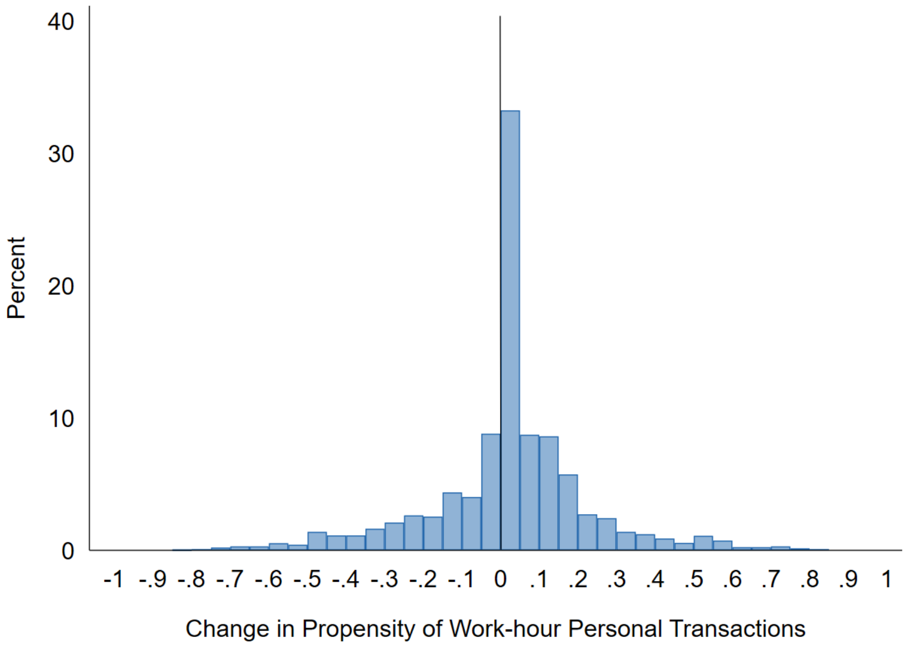


TABLE 1. SUMMARY STATISTICS

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 homeowners in any of the three "shocked" cities—Shanghai, Hangzhou, and Xiamen—and the control group comprises employed homeowners 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 January 2008 and October 2009 (or half of the months since card opening). We also restrict our analysis to individuals between the age of 22 and 80. Panels 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-spending transactions that occur during work hours (based on the monthly average during the six-month period before the shocks). 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	35.6	8.1	35.4	7.6	-0.2*
Female (%)	41.5	49.3	42.1	49.4	0.6
Married (%)	76.1	42.6	86.4	34.2	10.3***
College (%)	45.4	49.8	40.0	49.0	-5.4***
Private (%)	80.1	39.9	58.4	49.3	-21.8***
High-rank (%)	52.2	50.0	37.6	48.4	-14.7***
Credit limit (RMB)	23,151	27,416	10,723	11,800	-12,428***
N	4,272		102,223		

Panel B: After matching comparison

	Matched treatment group		Matched control group		Diff.
	Mean	SD	Mean	SD	(Control-Treatment)
	(1)	(2)	(3)	(4)	(5)
Age	35.6	8.1	35.4	7.9	-0.2
Female (%)	41.5	49.3	40.2	49.0	-1.3
Married (%)	76.1	42.6	76.3	42.3	0.2
College (%)	45.4	49.8	45.1	49.8	-0.3
Private (%)	80.1	39.9	80.8	39.4	0.7
High-rank (%)	52.2	50.0	53.1	49.9	0.8
Credit limit (RMB)	23,133	27,394	22,570	26,422	-563
N	4,271		4,271		

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	4.78	4.02	4.11	4.27	4.78	4.02	4.55	4.53
Work-hour personal transaction dummy	0.13	0.24	0.20	0.25	0.13	0.24	0.20	0.24
# work-hour personal transactions	0.21	0.46	0.31	0.49	0.21	0.46	0.31	0.48

TABLE 2. THE AVERAGE POST-SHOCK RESPONSE

This table shows the shirking response to the house price shock by the treatment group—employed homeowners in Shanghai, Hangzhou, or Xiamen, based on the matched sample from January 2008 to October 2009. *Work-hour personal transaction 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 0 otherwise. 1_{0m} is a dummy that equals 1 for the shock month among the treatment group, and 0 otherwise. 1_{post} is a dummy that equals 1 for the post-shock months among the treatment group, and 0 otherwise. *Land King District* is a dummy equal to 1 for treated homeowners close to the Land King District (within a 10 km radius). In columns 1 and 2, we exclude the event months for the treatment group from our analysis. In columns 3 and 4, we include the event months and directly test the response in the event month. 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			
Panel A: All homeowners				
$1_{-1m,-1m}$			0.0041	0.0021
			(0.84)	(0.38)
1_{0m}			-0.0029	-0.0016
			(-0.39)	(-0.20)
1_{post}	0.0247***	0.0277***	0.0249***	0.0273***
	(4.25)	(4.56)	(4.00)	(4.10)
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	103,451	103,451	106,810	106,810
R- squared	0.280	0.283	0.280	0.283
Panel B: Land-King-district homeowners				
$1_{-1m,-1m}$			0.0040	0.0020
			(0.83)	(0.36)
1_{0m}			-0.0030	-0.0017
			(-0.40)	(-0.22)
1_{post}	0.0176***	0.0194***	0.0202***	0.0213***
	(3.00)	(3.15)	(3.19)	(3.17)
1_{post}* Land King District	0.0140***	0.0166***	0.0093***	0.0119***
	(18.35)	(12.31)	(11.32)	(9.00)
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	103,451	103,451	106,810	106,810
R-squared	0.280	0.283	0.280	0.283

TABLE 3. EXPLORING CONFOUNDING LOCAL ECONOMIC CONDITIONS

This table explores the confounding local economic conditions. Panel A presents the response, after the Land King event in Shanghai (i.e., 2008:08), by employed homeowners in the neighboring cities of Shanghai—cities in the provinces of Jiangsu and Zhejiang, based on the unmatched full sample. In this analysis, we exclude the treated cities—Shanghai, Hangzhou, and Xiamen—from the sample and focus on the period from January 2008 to July 2009 (before the second Land King event in the sample). In Panel B, we show the response, for employed homeowners in the three Land King cities, after the last land auctions before the Land King events that set city-specific historic-high prices but did not break the nationwide record (i.e., did not win the Land King titles). The analyses are based on the matched sample. These three auctions occurred in January 2008 for Shanghai, July 2009 for Hangzhou, and June 2009 for Xiamen. We restrict the post-period for the three cities to the Land King months. Panel C shows the average response to the house price shock (i.e., the three Land King events) among employed renters. We use the same propensity score matching procedure as in the main analysis to identify a matched observation for each treated renter. Column 1 includes all employed renters, and column 2 includes all control renters and treated renters close to the Land King district (within a 10 km radius). The event month observations for the treatment group are excluded for all analyses in this table. 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)
	Work-hour personal transaction dummy	
Panel A: Post-shock response in the neighboring cities		
1_{post}	0.0028	0.0042
	(0.59)	(0.84)
Individual FE	Y	Y
Year-month FE	Y	N
Industry year-month FE	N	Y
Employer type year-month FE	N	Y
Observations	819,665	819,665
R-squared	0.287	0.288
Panel B: City-specific record-high land auctions as placebo events		
$1_{\text{max land price before land king}}$	-0.0011	0.0013
	(-0.10)	(0.10)
Individual FE	Y	Y
Year-month FE	Y	N
Industry year-month FE	N	Y
Employer-type year-month FE	N	Y
Observations	74,052	74,052
R-squared	0.296	0.300

	(1)	(2)
	Work-hour personal transaction dummy	
Panel C: Response among renters		
	All renters	Land King district renters
1_{post}	-0.0016	-0.0164*
	(-0.24)	(-1.89)
Individual FE	Y	Y
Year-month FE	N	N
Industry year-month FE	Y	Y
Employer-type year-month FE	Y	Y
Observations	28,005	20,298
R-squared	0.271	0.278

TABLE 4. SHIRKING RESPONSE: REAL ESTATE VS NON-REAL ESTATE INDUSTRIES

This table compares the response among homeowners employed in the real estate industry and the response among other employed homeowners, based on the matched sample. *Employed in the real estate industry* is a dummy equal to 1 if the credit card holder is employed in the real estate industry, and 0 otherwise. The event-month observations for the treatment group are excluded. 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)
	Work-hour personal transaction dummy	
1_{post}	0.0247***	0.0276***
	(4.47)	(4.84)
1_{post}*Employed in the real estate industry	-0.0005	0.0030
	(-0.06)	(0.13)
Individual FE	Y	Y
Year-month FE	Y	N
Industry year-month FE	N	Y
Employer-type year-month FE	N	Y
Observations	103,451	103,451
R-squared	0.280	0.283

TABLE 5. WORK HOUR PERSONAL TRANSACTIONS AMONG NON-WORKING RESIDENTS

This table shows the results of the same specification as in Table 2 with the sample restricted to the non-working population, which includes retirees and the unemployed. The control group comprises retirees and the unemployed of the unaffected cities in the full sample. The event-month observations for the treatment group are excluded from the analyses. 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)
	Work-hour personal transaction dummy	
1_{post}	-0.0143	-0.0111
	(-0.95)	(-0.73)
Individual FE	Y	Y
Year-month FE	Y	N
Industry year-month FE	N	Y
Employer-type year-month FE	N	Y
Observations	21,665	21,665
R-squared	0.370	0.381

TABLE 6. HETEROGENEITY ANALYSIS

This table shows the response heterogeneity based on the matched sample. Panel A shows the shirking response heterogeneity by the level of housing wealth as proxied by the granted credit card limit or multiple-home ownership. *High Credit Limit* is a dummy variable equal to 1 if an individual's credit limit (at account opening) is above the median of the distribution among all cardholders in our matched owner sample (i.e., RMB 10,000). *Multiple houses* is a dummy variable equal to 1 if the individual owns multiple houses in our sample, and 0 otherwise. Panel B shows the shirking response heterogeneity by age and employer type. *Older* is a dummy equal to 1 if the credit card holder's age is in the top quartile of the distribution among all cardholders in our matched owner sample (i.e., above 42). *Older SOE employee* is a dummy equal to 1 if the credit card holder works in a state-owned enterprise and is older than 42 years old. Panel C shows the shirking response heterogeneity by occupation. *Professional* is a dummy variable equal to 1 if the credit card holder works in a professional occupation, including lawyers, accountants, and teachers. In Panel C, column 1 includes all observations in the matched sample; column 2 excludes individuals holding high-rank positions at work such as CEO, director, department manager, chief physician, and full professor. Panel D shows the shirking (column 1) and credit-card-spending (column 2) response heterogeneity by the the ability to access housing wealth. We match each multiple-home treated owner with a single-home treated owner using nearest-neighbor propensity score matching without replacement. The scores are derived from a logistic regression using age, a quadratic polynomial of credit limit, female dummy, marital status dummy, college dummy, a set of dummies for the employer type, and a dummy indicating a senior-rank position at work. The remaining single-home treated owners who are not matched with a multiple-home treated owner are excluded from the analysis. The event-month observations for the treatment group are excluded for all analyses in this table. Refer to Table 2 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)
	Work-hour personal transaction dummy	
Panel A: By the levels of credit card limit and multiple-home owners		
1_{post}	0.0200***	0.0272***
	(2.78)	(4.49)
1_{post}* High credit limit	0.0153***	
	(3.14)	
1_{post}* Multiple houses		0.0081***
		(4.47)
Individual FE	Y	Y
Industry year-month FE	Y	Y
Employer-type year-month FE	Y	Y
Observations	103,451	103,451
R-squared	0.283	0.283
Panel B: By age and employer type		
1_{post}	0.0240***	0.0241***
	(3.85)	(3.84)
1_{post}* Older	0.0151***	0.0118***
	(5.37)	(3.43)
1_{post}* Older SOE employee		0.0592***
		(4.29)
Individual FE	Y	Y
Industry year-month FE	Y	Y
Employer-type year-month FE	Y	Y
Observations	103,451	103,451
R-squared	0.283	0.283

Panel C: By occupation

	Work-hour personal transaction dummy	
	(1)	(2)
	All owners	Low-rank positions only
1_{post}	0.0279***	0.0221***
	(4.98)	(3.16)
1_{post}* Professional	0.0386***	0.0323***
	(10.39)	(5.72)
Individual FE	Y	Y
Industry year-month FE	Y	Y
Employer-type year-month FE	Y	Y
Observations	91,099	41,823
R-squared	0.280	0.291

Panel D: By housing wealth access

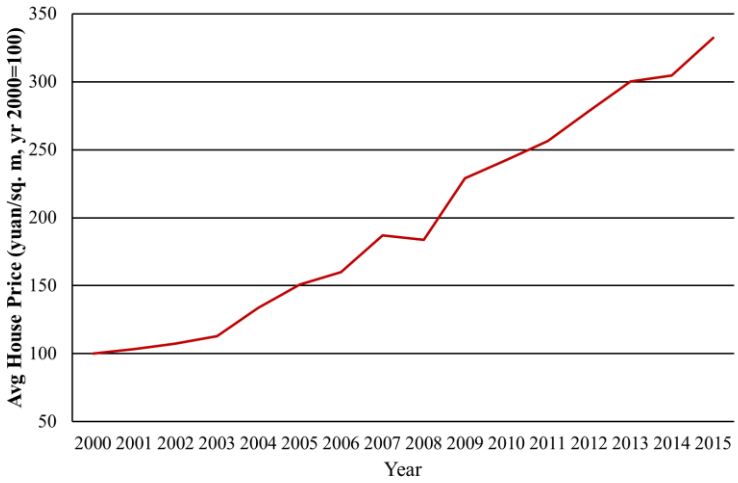
	(1)	(2)
	Work-hour personal transaction dummy	Log(1+credit card spending)
1_{post}	0.0432***	-0.0023
	(4.36)	(-0.03)
1_{post}* Multiple houses	-0.0027	0.1752***
	(-0.61)	(3.34)
Individual FE	Y	Y
Industry year-month FE	Y	Y
Employer-type year-month FE	Y	Y
Observations	54,431	62,366
R-squared	0.269	0.309

INTERNET APPENDIX
(NOT INTENDED FOR PUBLICATION)

FIGURE IA.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.

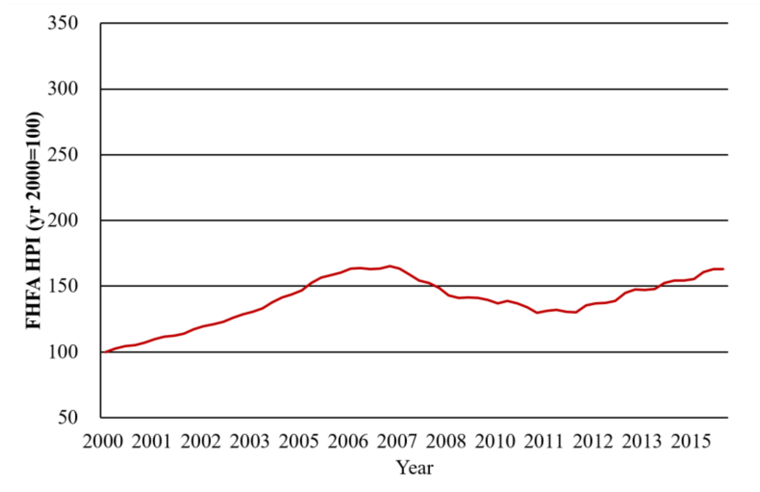


FIGURE IA.2. HOUSE PRICE GROWTH IN MAJOR CHINESE CITIES

This figure plots the heatmap characterizing the house price growth across 120 major Chinese cities. We use the house price index at the end of 2007 estimated by Fang et al. (2015), 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 1). 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 indicates states for which we do not have (enough) data for estimation.

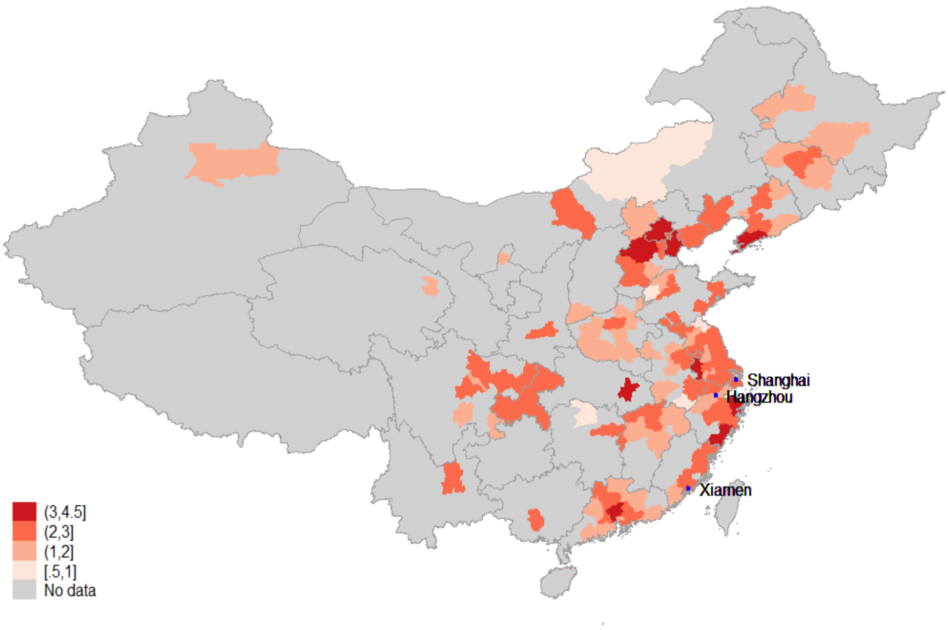
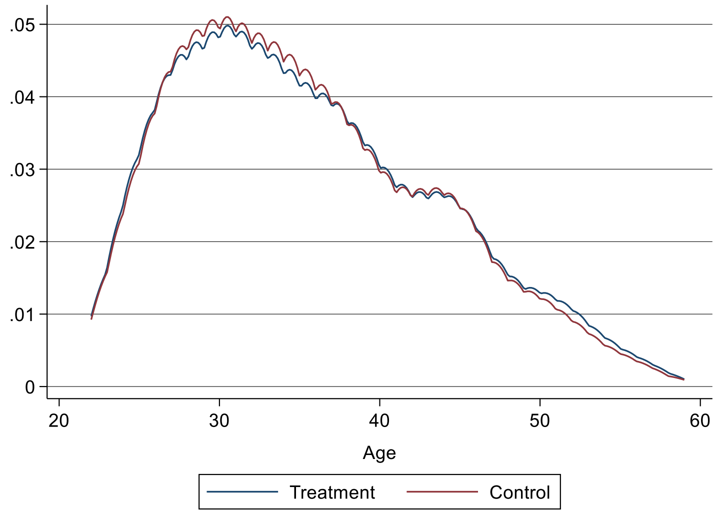


FIGURE IA.3. KERNEL DENSITY PLOTS

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

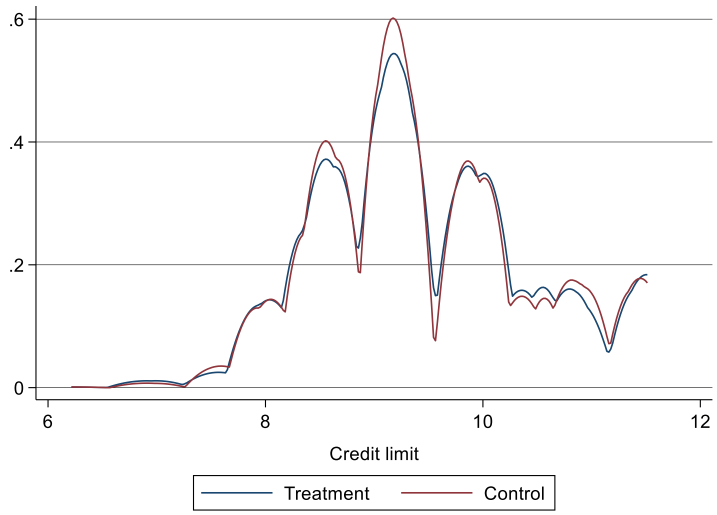


TABLE IA.1. TYPE OF CREDIT CARD TRANSACTION

This table provides a breakdown of all the card transactions in our credit-card-transaction dataset. 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=10,635,099)
Panel A: Types of credit card transactions	
Spending on goods and services	68.32
Payment of financial services, government fees, and utility bills	31.68
Panel B: Top 5 transaction types	
Warehouse retailer	23.62
(Onsite) payment of financial services	18.33
Department store	11.86
Fee payment	9.03
Restaurant	3.89

TABLE IA.2 CORRELATION BETWEEN HOUSE PRICE AND WORK-TIME CREDIT-CARD-TRANSACTION BEHAVIOR

This table shows the results on the correlation between an (employed) homeowner's propensity to have work-hour personal transactions (using credit cards) in a given month and the past month's local house price, based on the unmatched full sample from January 2008 to October 2009. The monthly house price index is developed by Fang et al. (2015). The analysis sample covers credit card holders in 115 Chinese cities where house-price-index data can be merged with our data. Refer to Appendix A for variable definitions. To address the inference problem associated with generated regressors (i.e., estimated house price index), we bootstrap the standard errors with 1000 iterations. Z-statistics are reported in parentheses under the coefficient estimates. Significant at *** 1%, **5%, and *10%.

	(1)	(2)
	Work-hour personal transaction dummy	
Lagged house price index	0.0105**	0.0105**
	(2.53)	(2.54)
Individual FE	Y	Y
Year-month FE	Y	N
Industry year-month FE	N	Y
Employer-type year-month FE	N	Y
Observations	762,072	762,072
R-squared	0.277	0.277

TABLE IA.3 LAND KING AND VALIDITY OF THE SHOCKS

Panel A describes the three residential land parcels that broke the nationwide record of unit price in land auctions between January 2008 and October 2009 in China. Panel B shows the 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. (2015). *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 the house price index for the last month, and we define the two other price change variables in a similar way. Panel C shows the regression results of the event-study analyses of the housing price change in Shanghai, Hangzhou, and Xiamen during the two years after the Land King events, relative to the price change in the untreated cities. The dependent variable in column 1 is the monthly average transaction price (per square meters) obtained from the National Bureau of Statistics, and the dependent variable in column 2 is the monthly house price index constructed by Fang et al. (2015). $1_{-1m,-1m}$ is a dummy that equals 1 in the month before the shocks among the treatment group, and 0 otherwise. 1_{0m} is a dummy that equals 1 for the shock month among the treatment group, and 0 otherwise. 1_{post} is a dummy that equals 1 for the post-shock months among the treatment group, and 0 otherwise. 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

Panel C

	(1)	(2)	(3)	(4)
	Log (avg. transaction price)		Price index	
$1_{-1m,-1m}$		-0.0356		-0.0817
		(-0.56)		(-0.73)
1_0		-0.0265		-0.0395
		(-0.41)		(-0.40)
1_{post}	0.0957***	0.0947***	0.1549***	0.1533***
	(6.10)	(6.00)	(3.63)	(3.59)
Constant	8.2210***	8.2211***	1.8500***	1.8500***
	(4,397.24)	(4,384.79)	(598.49)	(598.51)
City FE	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y
Observations	3,383	3,383	12,534	12,534
R-squared	0.964	0.964	0.847	0.847

TABLE IA.4 EVENT-STUDY ANALYSIS AROUND STRUCTURAL BREAKS IN LOCAL HOUSE PRICES

This table shows the difference-in-differences results using structural breaks in local house prices to identify housing-demand shocks. 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 employed owner cardholders in 115 Chinese cities where house-price-index data can be merged with our data. We follow Ferreira and Gyourko (2011) and Charles, Hurst, and Notowidigdo (2018) to estimate structural breaks in house prices of the 115 cities during the period between January 2003 and August 2014. Twenty-eight cities in our sample experienced a positive structural break during the sample period (January 2008–October 2009). The main independent variable is the interaction between the magnitude of the estimated structural break in city k (λ_k) and the timing of the structural break ($1_{t_k} = 1$ for all months during and after the structural break for city k). Refer to Appendix A for other variable definitions. To address the inference problem associated with generated regressors, we bootstrap the standard errors with 1000 iterations. Z-statistics are reported in parentheses under the coefficient estimates. Significant at *** 1%, **5%, and *10%.

	(1)	(2)
	Work-hour personal transaction dummy	
Interaction between magnitude & timing of structural break, $\lambda_k \times 1_{t_k}$	0.2431***	0.2500***
	(2.98)	(3.07)
Individual FE	Y	Y
Year-month FE	Y	N
Industry year-month FE	N	Y
Employer-type year-month FE	N	Y
Observations	762,323	762,323
R-squared	0.277	0.277

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

This table repeats the analysis in Table 2 by changing the measurement of work-hour personal transactions. All analyses are based on the matched sample. Panel A uses *Work-hour leisure spending* as the dependent variable, defined based on work-hour spending at retailers, department stores, theatres, and spas. Panel B uses log one plus work-hour personal transaction frequency (amount), or the ratio of work-hour personal transaction frequency (amount) over the total credit card transaction frequency (amount) as the dependent variable. Panel C examines the heterogeneous response of high-value versus low-value work-hour personal transactions. The dependent variable in column 1 is a dummy equal to 1 if the credit card holder ever uses credit cards for high-value personal transactions during work hours in a month, and 0 otherwise. A transaction is defined as a high-value transaction if its amount is above the city-specific median of all transactions within the same MCC. The dependent variable in column 2 is a dummy equal to 1 if the credit card holder ever uses credit cards for low-value personal transactions during work hours in a month, and 0 otherwise. Refer to Appendix A and Table 2 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: Alternative definition of personal transaction

	(1) Work-hour leisure spending dummy
1_{post}	0.0354***
	(5.38)
Individual FE	Y
Industry year-month FE	Y
Employer-type year-month FE	Y
Observations	103,451
R-squared	0.288

Panel B: Using transaction frequency and amount as the dependent variable

	(1) Log(1+#work-hour personal transactions)	(2) Log(1+$work-hour personal transactions)	(3) #work-hour personal transactions/total #CC transactions	(4) $work-hour personal transactions/total $CC transactions
1_{post}	0.0209***	0.1259***	0.0084***	0.0068**
	(4.41)	(3.80)	(2.97)	(2.00)
Individual FE	Y	Y	Y	Y
Industry year-month FE	Y	Y	Y	Y
Employer-type year-month FE	Y	Y	Y	Y
Observations	120,429	120,429	103,451	102,981
R-squared	0.313	0.260	0.237	0.202

Panel C: Heterogeneous response of high-value versus low-value work-hour personal transactions

	Work-hour personal transaction dummy	
	High-value transactions	Low-value transactions
	(1)	(2)
	0.0212***	0.0101***
	(4.94)	(2.78)
Individual FE	Y	Y
Industry year-month FE	Y	Y
Employer-type year-month FE	Y	Y
Observations	103,451	103,451
R-squared	0.243	0.254

TABLE IA.6. CHANGE OF CREDIT-CARD-TRANSACTION BEHAVIOR IN OTHER HOURS

This table shows the response in non-work-related transactions during other hours of workdays, based on the matched sample. The dependent variable in columns 1 and 2 is a dummy variable equal to 1 if the credit card holder ever uses credit cards for non-work-related transactions between 12 pm and 2 pm of workdays in a given month, and 0 otherwise. The dependent variable in columns 3 and 4 is a dummy variable equal to 1 if the credit card holder ever uses credit cards for non-work-related transactions between 8 am and 9 am or between 5 pm and 6 pm of workdays in a given month, and 0 otherwise. The dependent variable in columns 5 and 6 is a dummy variable equal to 1 if the credit card holder ever uses credit cards for non-work-related transactions between 6 pm and 9 pm of workdays in a given month, and 0 otherwise. We exclude the event-month observations for the treatment group from our analysis. Refer to Table 2 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 transaction dummy					
	Lunch hours (12-2 pm)		Early and late hours (8-9 am, 5-6 pm)		Overtime hours (6-9 pm)	
1_{post}	0.0051***	0.0053***	0.0158***	0.0170***	0.0223***	0.0223***
	(4.35)	(4.34)	(3.19)	(3.53)	(6.85)	(6.24)
Individual FE	Y	Y	Y	Y	Y	Y
Industry year-month FE	Y	Y	Y	Y	Y	Y
Employer-type year-month FE	Y	Y	Y	Y	Y	Y
Observations	103,451	103,451	103,451	103,451	103,451	103,451
R-squared	0.117	0.120	0.198	0.202	0.265	0.269

TABLE IA.7. ALTERNATIVE CONTROL GROUPS AND PRE-SHOCK WINDOWS

This table repeats the main analysis in Table 2 using alternative control groups or pre-shock windows. In Panel A, we use employed homeowners from the geographically proximate cities in the unmatched full sample as the control group. These cities include cities in Zhejiang, Jiangsu, Fujian, and Guangdong provinces. In Panel B, we use employed homeowners in all unaffected cities in our unmatched full sample as the control group. In Panel C, we repeat the same analysis as in Table 2, column 4, 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. Refer to Appendix A and Table 2 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			
Panel A: Nearby unaffected cities as the control group				
$1_{-1m,-1m}$			0.0036	0.0044
			(1.00)	(1.34)
1_{0m}			0.0037	0.0052
			(0.46)	(0.62)
1_{post}	0.0226***	0.0251***	0.0224***	0.0251***
	(6.75)	(7.14)	(6.50)	(6.94)
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	383,129	383,129	386,489	386,489
R-squared	0.276	0.277	0.276	0.277
Panel B: All unaffected cities as the control group (unmatched full sample)				
$1_{-1m,-1m}$			-0.0019	-0.0010
			(-0.60)	(-0.30)
1_{0m}			-0.0049	-0.0014
			(-0.69)	(-0.20)
1_{post}	0.0239***	0.0264***	0.0229***	0.0256***
	(6.47)	(7.02)	(6.47)	(7.04)
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	1,090,706	1,090,706	1,094,066	1,094,066
R-squared	0.276	0.276	0.276	0.276

	(1)	(2)	(3)
	Work-hour personal transaction dummy		
Panel C: Alternative Pre-shock Windows			
$1_{-2m,-1m}$	0.0010		
	(0.22)		
$1_{-3m,-1m}$		0.0004	
		(0.09)	
$1_{-4m,-1m}$			0.0000
			(0.01)
1_{0m}	-0.0017	-0.0019	-0.0020
	(-0.20)	(-0.24)	(-0.23)
1_{post}	0.0273***	0.0271***	0.0269***
	(4.05)	(4.16)	(3.67)
Individual FE	Y	Y	Y
Industry year-month FE	Y	Y	Y
Employer-type year-month FE	Y	Y	Y
Observations	106,810	106,810	106,810
R-squared	0.283	0.283	0.283

Section A. A stylized model

To understand the economic mechanism that explains our empirical findings, we formulate a conceptual framework that captures the key tradeoffs households face. Specifically, we consider an agent who lives for two periods (*t=0* or *1*) and maximizes the expected utility by choosing consumption c_t and the level of leisure l_t. Similar to Prescott (2004), the preferences of this agent are ordered by

$$U_0 = logc_0 + \alpha logl_0 + \beta E_0[logc_1 + \alpha logl_1]. \quad (A1)$$

The discount factor $0 < \beta < 1$ specifies the degree of patience. The parameter α specifies the value of leisure relative to consumption. One important feature of this model is that we consider two types of leisure: (1) off-the-job leisure, that is, the non-working time according to the official labor contract; (2) on-the-job leisure, that is, shirking from work during the contractual working time. The contractual work hours are typically fixed in the short run due to the frictions in labor-contract adjustments (Chetty, Friedman, Olsen, and Pistaferri, 2011). Thus, we assume the agent adjusts leisure by varying the level of shirking while working. Specifically, we define $l_t = \tilde{O} * s_t$, where $\tilde{O}$ is off-the-job leisure, which is set constant; $s_t \in [0, \bar{s}]$ is a factor that captures the agent's shirking intensity. A higher s_t means the agent spares more time for leisure during the contractual work hours.

Next, we specify the labor market environment that the agent faces. Frictions embedded in labor contracts imply shirking does not immediately affect the contemporaneous labor income. We model this feature by allowing the agent to be caught shirking and fired by the current employer in the next period, with the detection probability proportional to the current-period shirking intensity. The time lag between shirking and adverse labor market consequences is aimed to capture the elusive nature of shirking, implying wages are determined in advance based on workers' expected productivity (e.g., Farber and Gibbons, 1996). Specifically, the probability of getting caught is given by $p\frac{s_t}{\bar{s}}$, where $p \in [0,1]$ captures the employer's monitoring intensity. If not caught, the agent can earn an after-tax unit wage of w_t. Once caught, the agent will be fired by the current employer and earn $\tilde{w}$ from an alternative employer in period $t+1$. In a stable equilibrium, an agent must earn more in the current match than s/he could earn at a different employer (after considering search costs and frictions in the labor market). Therefore, we assume $\tilde{w} < w_t$, with the loss in labor income if being caught, $w_t - \tilde{w}$, denoted as θ. The wage trajectory is exogenously given and perfectly foreseeable conditional on whether the agent is caught.

The last component of the model setup characterizes the asset holding and wealth shock. The agent has an endowment of asset A. For simplicity, we assume the asset has a constant return r. Without loss of generality, we normalize r to be 0. We model the shock to the agent's asset endowment in the following way. At time $t = 0$, the agent experiences a change in initial asset value from A to GA, where $G > 0$ captures the magnitude of the wealth shock.

By far, our model is set up to reflect the key response heterogeneity in our empirical analysis. A captures the exposure to the wealth shock, which corresponds to the empirical tests in Table 6 Panel A. θ captures the loss in future labor income caused by shirking, echoing the tests in Table 6 Panel B; p is related to employers' monitoring costs (a higher monitoring cost implies a lower p), corresponding to Table 6 Panel C. To rationalize the empirical pattern in Table 6 Panel D, we consider two scenarios by varying the tradability of the asset in period $t = 0$.

A.1 Benchmark model: allowing for free asset trading

In this benchmark model, we assume the asset can be freely traded in both periods. The agent's budget constraint is specified as follows. Without loss of generality, we standardize the agent's official work hours to 1. In each period, the agent uses the labor income and proceeds from asset trading to fund consumption. Specifically, the budget constraint at time $t = 0$ is

$$c_0 = w_0 + xGA, \tag{A2}$$

where x is the proportion of asset the agent sells at time $t = 0$. The budget constraint at time $t = 1$ depends on the shirking outcome. With probability $p\frac{s_0}{\bar{s}}$, the agent is caught shirking and earns the reservation wage $\widetilde{w}$, with the following budget constraint:

$$c_{1,fired} = \widetilde{w} + (1 - x)GA. \tag{A3}$$

With probability $1 - p\frac{s_0}{\bar{s}}$, the agent does not get caught shirking and continues to earn w_1 at the current employer:

$$c_{1,notfired} = w_1 + (1 - x)GA. \tag{A4}$$

Note that in the last period, the agent will choose the highest possible level of shirking, because shirking will not be followed by any adverse consequence in the labor market. Therefore, $s_{1,fired}$=$s_{1,notfired}$=$\bar{s}$.

Collectively, the agent's optimization problem can be expressed as

$$\max_{c_0, c_{1,fired}, c_{1,notfired}, s_0} logc_0 + \alpha log(\tilde{O} * s_0) +$$

$$\beta \left[\frac{ps_0}{\bar{s}} logc_{1,fired} + \left(1 - \frac{ps_0}{\bar{s}}\right) logc_{1,notfired} + \alpha log(\tilde{O} * \bar{s})\right]$$

$$\text{s.t.} \begin{cases} c_0 + c_{1,fired} = w_0 + \widetilde{w} + GA \\ c_0 + c_{1,notfired} = w_0 + w_1 + GA \\ c_0, c_{1,fired}, c_{1,notfired}, s_0 \geq 0 \\ s_0 \leq \bar{s} \end{cases}.$$

The first-order conditions for interior solutions are

$$\frac{\partial U_0}{\partial c_0} = \frac{1}{c_0} - \beta \left[p\frac{s_0}{\bar{s}} \frac{1}{w_0 + \widetilde{w} + GA - c_0} + \left(1 - p\frac{s_0}{\bar{s}}\right) \frac{1}{w_0 + w_1 + GA - c_0}\right] = 0 \tag{A5}$$

$$\frac{\partial U_0}{\partial s_0} = \frac{\alpha}{s_0} - \frac{\beta p}{\bar{s}} log\left(1 + \frac{w_1 - \widetilde{w}}{w_0 + \widetilde{w} + GA - c_0}\right) = 0\,. \tag{A6}$$

Assume GA is sufficiently large so that $w_0 + \widetilde{w} + GA - c_0$ is significantly larger than $w_1 - \widetilde{w} = \theta$. Then, we apply log-linearization on $log\left(1 + \frac{w_1 - \widetilde{w}}{w_0 + \widetilde{w} + GA - c_0}\right)$ and approximate equation (A6) as[15]:

$$\frac{\alpha}{s_0} - \frac{\beta p}{\bar{s}} \frac{\theta}{w_0 + \widetilde{w} + GA - c_0} = 0\,. \tag{A7}$$

[15] To study the model-prediction robustness with respect to the log-linearization approximation choice, we verify that the key comparative statics in Proposition 1 hold based on numerical solutions under standard parameters.

It follows that the interior solutions of the optimal shirking ${s_0}^*$ and consumption ${c_0}^*$ are

$$ {s_0}^* = \bar{s}\alpha \frac{(\alpha+\beta)(GA+w_0+\widetilde{w})-\theta}{\beta p\theta(1+\alpha+\beta)} \tag{A8} $$

$$ {c_0}^* = \frac{GA+w_0+w_1}{1+\alpha+\beta}. \tag{A9} $$

Based on the solution of ${s_0}^*$, we derive the key comparative statics about the agent's optimal level of shirking:

$$ \frac{\partial {s_0}^*}{\partial G} = \frac{\bar{s}\alpha(\alpha+\beta)A}{\beta\theta p(1+\alpha+\beta)} > 0 \tag{A10} $$

$$ \frac{\partial^2 {s_0}^*}{\partial G\partial A} = \frac{\bar{s}\alpha(\alpha+\beta)}{\beta\theta p(1+\alpha+\beta)} > 0 \tag{A11} $$

$$ \frac{\partial^2 {s_0}^*}{\partial G\partial p} = -\frac{\bar{s}\alpha(\alpha+\beta)A}{\beta\theta(1+\alpha+\beta)p^2} < 0 \tag{A12} $$

$$ \frac{\partial^2 {s_0}^*}{\partial G\partial \theta} = -\frac{\bar{s}\alpha(\alpha+\beta)A}{\beta p(1+\alpha+\beta)\theta^2} < 0. \tag{A13} $$

We also derive the key comparative statics about the agent's optimal level of consumption based on the solution of ${c_0}^*$:

$$ \frac{\partial {c_0}^*}{\partial G} = \frac{A}{1+\alpha+\beta} > 0. \tag{A14} $$

The above comparative statics are summarized in Proposition A1.

Proposition A1: When the asset can be freely traded, a positive wealth shock is associated with a higher level of shirking in period $t = 0$ ($\frac{\partial {s_0}^*}{\partial G} > 0$). This effect increases in the level of initial asset holdings ($\frac{\partial^2 {s_0}^*}{\partial G\partial A} > 0$) and decreases in the labor income loss if detected shirking or in the employer's monitoring intensity ($\frac{\partial^2 {s_0}^*}{\partial G\partial \theta} < 0$; $\frac{\partial^2 {s_0}^*}{\partial G\partial p} < 0$). Lastly, a positive wealth shock is associated with a higher level of consumption in period $t = 0$ ($\frac{\partial {c_0}^*}{\partial G} > 0$).

Proof: See equations (A10)-(A14).

Intuition of Proposition A1. A higher level of shirking increases the likelihood of getting caught and fired, leading to a lower labor income (in expectation) in period $t = 1$. However, an increase in the wealth level helps offset the cost of losing the job, enabling the agent to shirk more, all else equal. A higher level of asset holding implies a larger exposure to the wealth shock, strengthening the wealth effect (consistent with the empirical findings in Table 6 Panel A). Nevertheless, a greater cost of shirking, for example, due to a larger wage reduction if detected shirking (Table 6 Panel B) or a higher monitoring intensity by the employer (Table 6 Panel C), mutes the benefit of a given amount of wealth increase and thus dampens the shirking response. Lastly, when the asset can be freely traded, consumption will experience a positive response to the wealth increase.

A.2 Extension: asset trading not allowed in period t=0

Next, we consider an extension of the model by imposing an additional assumption that the agent cannot trade the asset in period $t = 0$. This assumption is aimed to capture the illiquidity of some asset classes, such as housing wealth. For example, in the housing market, households cannot freely sell their housing asset due to housing's role as a consumption good, and they have limited means to access gains in their housing wealth. To illustrate the role of asset tradability, we shut down the option to buy or sell the asset in period $t = 0$; the asset will be held until period $t = 1$ and then liquidated. We denote the corresponding consumption and shirking choice in this case as $\widehat{c_0}$, $\widehat{c_{1,fired}}$, $\widehat{c_{1,notfired}}$, and $\widehat{s_0}$.

The dynamic budget constraints in this case are

$$\widehat{c_0} = w_0 \tag{A15}$$

$$\widehat{c_{1,notfired}} = w_1 + GA \tag{A16}$$

$$\widehat{c_{1,fired}} = \tilde{w} + GA. \tag{A17}$$

The agent's optimization problem can be expressed as

$$\max_{\widehat{c_0},\widehat{c_{1,fired}},\widehat{c_{1,notfired}},\widehat{s_0}} log\widehat{c_0} + \alpha log\left(\tilde{O} * \widehat{s_0}\right) +$$

$$\beta\left[\frac{p\widehat{s_0}}{\bar{s}} log\widehat{c_{1,fired}} + \left(1 - \frac{p\widehat{s_0}}{\bar{s}}\right) log\widehat{c_{1,notfired}} + \alpha log\left(\tilde{O} * \bar{s}\right)\right]$$

$$\text{s.t.}\begin{cases} \widehat{c_0} = w_0 \\ \widehat{c_{1,notfired}} = w_1 + GA \\ \widehat{c_{1,fired}} = \tilde{w} + GA \\ 0 \leq \widehat{s_0} \leq \bar{s}. \end{cases}$$

Due to the lack of tradable assets, $\widehat{c_0}^*$ is equal to the agent's labor income w_0; $\widehat{c_{1,notfired}}^*$ and $\widehat{c_{1,fired}}^*$ equal the labor income plus the value of the asset in period $t = 1$. Therefore, $\widehat{s_0}$ becomes the only remaining choice variable. Its first-order condition is

$$\frac{\partial U_0}{\partial \widehat{s_0}} = \frac{\alpha}{\widehat{s_0}} - \frac{\beta p}{\bar{s}} log\left(1 + \frac{\theta}{\tilde{w} + GA}\right) = 0\,. \tag{A18}$$

Similar to equation (A6), equation (A18) can be approximated by

$$\frac{\partial U_0}{\partial \widehat{s_0}} = \frac{\alpha}{\widehat{s_0}} - \frac{\beta p}{\bar{s}} \frac{\theta}{\tilde{w} + GA} = 0. \tag{A19}$$

Therefore, the interior solution of $\widehat{s_0}$ is

$$\widehat{s_0}^* = \frac{\alpha \bar{s}(\tilde{w} + GA)}{\beta p \theta}. \tag{A20}$$

We require $\frac{\alpha(\tilde{w}+GA)}{\beta p \theta} < 1$ to guarantee an interior solution of $\widehat{s_0}^*$. Based on equation (A20), we derive the key comparative statics about the agent's optimal level of shirking when the asset is not tradable in period $t = 0$:

$$\frac{\partial \widehat{s_0}^*}{\partial G} = \frac{\alpha \bar{s} A}{\beta p \theta} > 0 \tag{A21}$$

$$\frac{\partial^2 \widehat{s_0}^*}{\partial G \partial A} = \frac{\alpha \bar{s}}{\beta p \theta} > 0 \tag{A22}$$

$$\frac{\partial^2 \widehat{s_0}^*}{\partial G \partial p} = -\frac{\alpha \bar{s} A}{\beta \theta p^2} < 0 \tag{A23}$$

$$\frac{\partial^2 \widehat{s_0}^*}{\partial G \partial \theta} = -\frac{\alpha \bar{s} A}{\beta p \theta^2} < 0. \quad (A24)$$

Lastly, due to the lack of tradable assets, $\widehat{c_0}^*$ is equal to the agent's labor income w_0. Therefore, $\widehat{c_0}^*$ is unresponsive to the wealth shock G; that is, $\frac{\partial \widehat{c_0}^*}{\partial G} = 0$.

The key comparative statics results are summarized in the following proposition.

Proposition A2: When the asset cannot be traded in period $t = 0$, a positive wealth shock is associated with a higher level of shirking in period $t = 0$ (denoted as $\widehat{s_0}^*$). That is, $\frac{\partial \widehat{s_0}^*}{\partial G} > 0$. The same comparative statics regarding the shirking response hold: $\frac{\partial^2 \widehat{s_0}^*}{\partial G \partial A} > 0$, $\frac{\partial^2 \widehat{s_0}^*}{\partial G \partial \theta} < 0$, $\frac{\partial^2 \widehat{s_0}^*}{\partial G \partial p} < 0$. Under the no-tradability assumption, consumption in period $t = 0$ (i.e., $\widehat{c_0}^*$) is irresponsive to wealth shocks ($\frac{\partial \widehat{C_0}^*}{\partial G} = 0$).

Proof: See equations (A21)-(A24).

Intuition of Proposition A2. When the asset cannot be traded in period $t = 0$, the responses of shirking and consumption show different patterns. The agent requires liquidity to increase consumption. When the agent cannot realize wealth gains in period $t = 0$ to fund their immediate spending needs, consumption can barely adjust even when the asset value goes up significantly. On the other hand, the shirking (or leisure consumption) response is not constrained by the agent's current liquidity. Moreover, due to the elusive nature of shirking, current labor income is unaffected by the higher level of shirking, because the cost of shirking—in the form of reduced labor income—is only realized later in period $t = 1$ and with a probability of less than one. As a result, asset tradability does not affect the agent's ability to adjust shirking behavior in period $t = 0$. The agent will still optimally increase shirking in response to a positive wealth increase that is not accessible in period $t = 0$, as long as, in expectation, the wealth increase (realized in period t =1) can offset the cost of shirking. Similarly, the same intuition holds for the comparative statics results of the shirking response with respect to the level of initial asset holdings A, the labor income loss if detected shirking θ, and the employer's monitoring intensity p.

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