# When Credit Expansion Backfires: The Unequal Impact of Loan Cap Relaxation on Kuwait's Housing Market


## Abstract

This study examines the impact of an increase in the total loan cap introduced by the Central Bank of Kuwait on private residential housing prices. Using a difference-in-differences methodology, we find that residential property prices increased by 10% overall, with smaller, more affordable properties experiencing a disproportionately larger rise. While increased credit availability expanded purchasing power for higher-income buyers, it simultaneously fueled price inflation in the more affordable housing segment, exacerbating challenges to affordability and inequality. Moreover, despite the increase in credit availability, transaction volumes remained unchanged, suggesting market liquidity did not improve. Our findings highlight the need to complement credit market reforms with supply-side interventions to ensure housing accessibility.



Keywords: Housing Market, Credit Policy, Loan Cap, Housing Affordability, Emerging Markets, Kuwait

JEL Classification: G21, R31, E51, R21, D12

# 1 Introduction

The relationship between credit markets, housing affordability, and regulatory policy presents a critical area of economic inquiry. A key question at this intersection is how policies designed to stimulate economic activity and expand access to credit interact with real-world market constraints. Standard economic theory suggests that easing borrowing constraints, ceteris paribus, should increase consumption and investment. However, in markets characterized by inelastic supply, information asymmetries, or speculative behavior, this seemingly straightforward relationship becomes more complex, often yielding unintended consequences.

Housing markets, in particular, illustrate these dynamics. When credit access expands in a supply-constrained market, the resulting demand surge is more likely to drive up prices than to increase housing availability. This can create a paradox where greater access to credit ultimately reduces affordability, disproportionately affecting lower-income households while benefiting those with higher borrowing capacity. By integrating theoretical insights with empirical analysis of borrowing constraint relaxations, we develop a framework to explain how well-intentioned policies can, under certain market conditions, exacerbate affordability challenges rather than alleviate them. Our findings underscore the need for market-specific considerations in regulatory design, ensuring that interventions do not inadvertently reinforce existing inequalities.

On November 14, 2018, Kuwait implemented a significant shift in its credit policy, relaxing the absolute caps on consumer and total loans and thereby increasing the potential for housing-related borrowing. The Central Bank of Kuwait (CBK) had announced this policy just four days earlier, on November 10, 2018. The short interval between announcement and implementation strengthens the identification of our policy shock, as it leaves little opportunity for the real estate market to respond to the announcement alone.

Kuwaiti consumers have access to two main categories of personal loans: consumer loans and housing loans. Consumer loans are general-purpose loans used to finance various personal expenditures, while housing loans are long-term loans intended for purchasing or constructing private residential properties. Unlike conventional mortgage loans, housing loans in Kuwait are not collateralized; rather, they are extended based solely on the borrower's net monthly salary.

The 2018 loan cap relaxation directly increased borrowing capacity for personal loans, enabling individuals to access larger amounts of credit. In contrast, no parallel regulatory adjustments were made for the investment residential sector, leaving financing conditions for real estate investors unchanged. To assess the impact of this policy shift, the study utilizes weekly-level data on all residential and investment real estate transactions between November 2017 and November 2019, capturing a full year of observations before and after the reform. A difference-in-differences (DiD) methodology is employed to estimate the causal effect of the policy on private residential housing prices, using investment residential properties as the control group.

Loan caps are typically imposed to protect consumers and maintain financial stability by preventing over-indebtedness and ensuring that borrowing remains manageable. Standard macroprudential policy tools, such as loan-to-value (LTV) ratios for collateral-based constraints and debt-to-income (DTI) ratios or debt burden ratio (DBR) caps for income-based constraints, are widely adopted across various economies to regulate credit markets. In Kuwait, a DBR cap of 40% for employees and 30% for retirees is combined with an absolute loan cap, a rare dual-constraint policy that contrasts with the more common reliance on LTV, DTI, or DBR limits alone.

Unlike liberalized housing markets where supply dynamically adjusts to demand, Kuwait's housing market is primarily influenced by government policy through the Public Authority for Housing and Welfare (PAHW). Established in 1974, PAHW ensures housing accessibility by providing newly married Kuwaiti couples either fully constructed residential units or residential plots, accompanied by subsidized government loans for home construction. PAHW manages housing applications, allocates properties, and supervises large-scale residential projects, including the development of essential infrastructure, with an emphasis on state responsibility over market-driven approaches.

However, limited land availability, bureaucratic delays, fiscal constraints, and restrictive zoning regulations significantly constrain housing supply. These factors have extended the average waiting time for government housing to approximately 10 years as of 2020.[1] Consequently, the imbalance between supply and Kuwait's growing population has driven residential property prices upward, intensifying affordability challenges.

In Kuwait, no study has examined the impact of credit expansion on housing, nor whether it affects different types of private residential properties differently. This paper addresses these gaps by analyzing the impact of increased credit availability on housing prices in Kuwait, offering new evidence on how regulatory lending caps affect housing affordability in a market characterized by inelastic supply. A unique natural experiment emerged in 2018, when CBK decoupled consumer and housing loans, effectively raising the total loan cap from 70,000 Kuwaiti Dinars (KD) to 95,000 KD, a 36% increase in borrowing capacity for eligible individuals with installment payments not exceeding 30–40% of income. This change occurred within a dual regulatory framework, where the CBK exercises strict oversight by imposing both an absolute monetary cap on total borrowing, regardless of a borrower's income, wealth, or property value, and a DBR limit that restricts installment payments to a fixed share of income.

Given the uniqueness of Kuwait's dual-cap lending framework, to the best of our knowledge, this paper represents the first empirical study in the literature to estimate the impact of easing absolute caps—uniform, fixed monetary borrowing limits—on the housing market. The research aims to address the following key questions regarding the effects of relaxing lending caps in Kuwait:

1. What is the overall impact of the policy change on the price per square meter ($m^2$) of private residential housing?
2. Does the credit limit increase affect small, more affordable homes, and large residential properties differently, and what are the implications for housing inequality?

[1] "Housing Problem in Kuwait and the Way Forward" report by Markaz https://www.markaz.com/getmedia/e0441a6c-e01d-4b2b-b33f-83d1d43ebbbb/Housing-Problem-in-Kuwait-and-The-Way-Forward-Eng_Final.pdf

3. How does the policy change impact liquidity in the private residential housing market, as measured by the number of transactions?

This paper makes two distinct contributions to the literature. First, housing market research in the Gulf region, particularly Kuwait, is sparse due to historically limited data access. This paper overcomes that barrier by leveraging a unique weekly transaction-level dataset that allows for granular analysis of policy effects over time. Most existing studies use quarterly or annual data, which may miss short-term dynamics or policy shocks. The study adds new high-frequency evidence from an underrepresented setting, broadening the housing and macroprudential policy literature across time and regions.

Second, while many studies document the average effects of credit expansion on house prices, few disaggregate these effects by property size or affordability segment, especially in emerging markets. By demonstrating that loan cap relaxation disproportionately increases prices for smaller, more affordable homes, this study shows that credit policy can unintentionally worsen inequality. This has significant implications for sustainable development, as equitable access to housing is a critical social and economic goal. Understanding these distributional dynamics is essential for designing credit policies that promote financial inclusion without reinforcing existing disparities.

The remainder of this paper is organized as follows. Section 2 reviews the literature on macroprudential policy instruments, credit constraints, and their effects on housing markets, and outlines the institutional context surrounding the 2018 CBK policy change. Section 3 develops a theoretical framework that formalizes the relationship between credit constraints, housing demand, and affordability. Section 4 describes the data and details the empirical methodology used to address the main research questions. Section 5 presents the results of the empirical analysis of the policy's impact. Finally, Section 6 concludes with a discussion of the key findings and their policy implications.

# 2 Literature Review and Setting

## 2.1 Literature Review

Different economies have implemented varying restrictions on household borrowing. The LTV ratio is primarily employed to curb credit expansion in the housing market by capping the size of a mortgage loan relative to the property's market value. For example, Stein (1995) model shows how requiring a minimum down payment (an LTV constraint) can amplify housing market cycles. This work illustrates that looser credit constraints lead to higher prices and trading volume, while tighter constraints reduce liquidity in housing markets. The DTI and DBR ratios serve a similar purpose as the LTV ratio, but focus on limiting the amount households can borrow relative to their income. All ratios aim to ensure that borrowers can afford the loans they take on.

The primary advantage of imposing such caps is that they enhance the resilience of banks and borrowers to economic shocks, control rapid growth in consumer and housing loans, and limit housing price inflation. This, in turn, reduces the risk of defaults in the financial market and ensures the economy's financial stability. For instance, Jacome and Mitra (2015) analyze data from Brazil, Hong Kong, South Korea, Malaysia, Poland, and Romania, finding that LTV and DTI caps were effective in reducing credit growth and improving loan repayment performance, although they had no clear effect on limiting house price growth. Other studies using micro-level data from one or multiple countries confirm that LTV and DTI caps successfully cooled rapidly growing housing markets (Acharya et al., 2022; Cerutti et al., 2017; Kuttner and Shim, 2016; Jung and Lee, 2017; Lim et al., 2011; Morgan et al., 2019; Poghosyan, 2020).

However, several studies highlight the drawbacks of tightening these caps, as they restrict credit availability to households, potentially worsening financial constraints and leading to broader macroeconomic consequences. For example, De Jong and De Veirman (2019) employed a vector error correction model to demonstrate that tightening LTV caps in the Netherlands between 2011 and 2018 harmed real GDP and was associated with lower housing market prices. Similarly, Kinghan et al. (2022) identified negative heterogeneous impacts in Ireland, where an LTV cap with a stricter limit on houses valued over 220,000 euros distressed households, particularly first-time homebuyers. Higher-income borrowers, who tend to purchase more expensive homes, were also negatively impacted by stricter regulations that limited their access to loans.

Moreover, policies typically affect certain groups more than others, and restrictive policies may not bind all individuals equally. For instance, some studies find that women, young borrowers, individuals exposed to income and wealth shocks, and marginalized groups are more vulnerable to defaults and are more constrained by restrictive credit policies (Crook, 2001; Getter, 2003; Grant, 2007; Jappelli, 1990). Baziki and Capacioglu (2021) found evidence of credit spillover effects in different loan types when LTV caps were eased, with constrained borrowers increasing their general-purpose loans relative to unconstrained individuals with housing loans. This finding suggests that relaxing caps can, unexpectedly, prompt disadvantaged groups to borrow more to mitigate their inequality.

Furthermore, the relationship between credit expansion and housing prices has been extensively studied. Borio et al. (1994) argues that eased credit access, directly and indirectly, affects property prices. The direct effect arises from more individuals obtaining financing to purchase property, while the indirect impact occurs as increased economic activity raises purchasing power, further boosting housing demand. Carrington and Madsen (2011) provides additional insights, arguing that a positive credit shock initially raises house prices, but over time, increased credit availability leads to higher housing supply, ultimately reducing price pressures. Justiniano et al. (2019) suggests that the relaxation of lending constraints, rather than borrowing constraints, increased the supply of mortgage credit, primarily driving the U.S. housing boom from 2000 to 2006. Similarly, Su et al. (2019) examine China and shows that expansionary monetary policy and rising money supply (M2) exert a positive and time-varying impact on housing prices, particularly during periods of structural economic change, further confirming the broader link between liquidity conditions and real estate markets.

Empirical studies in Kuwait have attempted to quantify the factors driving housing price movements. Alfalah et al. (2021) identified key determinants, including housing demand, proxied by Kuwait's PAHW housing applicants, as well as inflation, interest rates, and oil prices. Albader and Alsabah (2025) found that PAHW backlogs, rising wages, and increased personal financing levels were primary contributors to rising house prices. Alternatively, Almutairi and El-Sakka (2016) analyzed absolute house prices in Kuwait, concluding that house prices primarily follow an autoregressive pattern, where current prices are largely determined by past prices rather than fundamental economic factors. However, despite these variations in explanatory models, all researchers agree that increasing housing supply significantly reduces property prices in Kuwait.

## 2.2 Policy Change in Lending Regulations

The CBK announced a significant change in lending regulations on November 10, 2018, which took effect four days later on November 14, 2018. The change affected two main caps: one for consumer loans and another for the combined total of consumer and housing loans.[2] The key changes are shown in Table 1.

Although the most direct impact of the policy change was on consumer loans, as the cap relaxation allowed for higher borrowing amounts, it also had indirect effects on housing loans. Prior to the policy, the total amount an individual could borrow, combining consumer and housing loans, was capped at 70,000 KD. The policy change eliminated this restriction by decoupling consumer and housing loans, increasing potential housing loan eligibility. As a result, borrowers could now access a combined total of up to 95,000 KD, representing a 36% increase in the overall borrowing cap.

[2] https://www.cbk.gov.kw/en/redirects/download?compId=143640&esIndex=reports

**Table 1.** Summary of Lending Policy Changes

| Policy | Before November 14, 2018 | Starting from November 14, 2018 onward |
|---|---|---|
| Cap on consumer loans | 15,000 KD or 15-fold net monthly salary (social security), whichever is lower | 25,000 KD or 25-fold net monthly salary (social security), whichever is lower |
| Cap on housing loans | 70,000 KD | No change |
| Cap on total loans | 70,000 KD | 95,000 KD |
| Maximum DBR rate | Employees: 40% of net monthly salary; Retirees: 30% of monthly social security | No change |
| Maximum loan tenor | Consumer loan: 5 years; Housing loan: 15 years | No change |

While the policy change may appear straightforward, its real-world implications vary significantly based on borrower behavior and income levels. Table 2 outlines three main scenarios illustrating how individuals allocate their borrowing capacity between consumer and housing loans. The analysis assumes a 40% DBR cap, a fixed annual interest rate of 6%, the maximum permissible rate at the time of the policy intervention, and standard loan tenures of five years for consumer loans and fifteen years for housing loans.

In Scenario 1, borrowers who had already maximized their consumer loans before the policy change and chose not to increase them afterward begin to benefit from the policy in the form of increased housing loan access once their monthly income exceeds 1,886 KD. Under the pre-policy framework, and given the binding absolute borrowing cap, the maximum housing loan available was 55,000 KD before the policy change. At an income of 2,202 KD, they become eligible for the whole 70,000 KD, representing a 27.3% increase over the previous cap.

Scenario 2 considers borrowers who continue to maximize their consumer loans before and after the policy change, increasing their consumer loan from 15,000 KD to 25,000 KD. These borrowers begin to access higher housing loan amounts under the expanded total cap once their monthly income reaches 2,369 KD, and they become eligible for the full 70,000 KD housing loan at 2,686 KD. Compared to the pre-policy cap, this represents a 27.3% increase in housing loan access and a 35.7% increase in total borrowing capacity.

In Scenario 3, borrowers who prioritize maximizing their housing loan and choose not to take on consumer loans experience no change in borrowing capacity. Their income threshold to access the full 70,000 KD housing loan remains at 1,477 KD, as they were already eligible prior to the policy change and do not benefit from the increased consumer loan limit.

**Table 2.** Three Main Borrower Scenarios After Policy Change

| **Scenario** | **Consumer Loan Behavior** | **Min Wage to Gain from Policy for Housing** | **Min Wage for Max Housing Loan** | **% Change** |
|---|---|---|---|---|
| **1. Fixed Consumer Loan (15,000 KD)** | Maximize before policy, no increase after | 1,886 KD | 2,202 KD | 27.3% |
| **2. Max Consumer Loans (15,000 to 25,000 KD)** | Maximize before and after policy | 2,369 KD | 2,686 KD | 27.3% |
| **3. Max Housing Loan Only** | Prioritize maximizing housing loan, regardless of consumer loan behavior | – | 1,477 KD | 0% |

These scenarios highlight how the policy disproportionately benefits higher-income households, as most low- and middle-income borrowers remain constrained by the DBR and thus unable to leverage the increased borrowing cap. Importantly, these thresholds must be interpreted in the context of income distribution. According to the labor force survey conducted one year before the policy intervention, only 16.3% of Kuwaiti workers earned 1,800 KD or more per month, and just 11.5% exceeded 2,000 KD. This underscores that the policy's benefits are concentrated mainly among higher-income earners.[3]

Kuwaiti households typically rely on multiple financing sources to purchase property, drawing from three main channels: an interest-free KD 70,000 loan from Kuwait Credit

[3] https://www.csb.gov.kw/Pages/Statistics?ID=64&ParentCatID=1

Bank, personal loans of up to 95,000 KD (70,000 KD before CBK 2018 policy change) from local commercial banks per spouse, and additional contributions from family or personal savings (Alfalah et al., 2022). Given these constraints, the maximum financing available after the policy change for a married Kuwaiti couple is capped at 260,000 KD, assuming neither spouse is constrained by the DBR, which, as previously explained, is not the case for most citizens. Moreover, since home prices often exceed this amount, many Kuwaiti families remain priced out of the private market, reinforcing their reliance on PAHW housing.

# 3 Theoretical Framework

Assume that individuals with income $Y$ allocate their spending between two commodities, housing and consumption products. They can borrow an amount $B$, while facing both an absolute borrowing cap $\bar{B}$ and a DBR cap. The prices of housing and consumption products are $P_H$ and $P_C$, respectively. Furthermore, assume that the individual's utility function follows a Cobb-Douglas form with positive preference parameters $\alpha$ and $\beta$, leading to the following constrained optimization problem:

$$\max_{H,C,B} U(H,C) = H^{\alpha}C^{\beta} \tag{1}$$

subject to the budget and borrowing constraints:

$$P_H H + P_C C = Y + B \tag{2}$$

$$B \leq \min(\bar{B}, B_{\text{DBR}}) \tag{3}$$

Where $\bar{B}$ represents the absolute cap on borrowing, and $B_{\text{DBR}}$ reflects the limit imposed by the DBR rule, which restricts monthly debt payments to a fixed proportion of income. We define $B_{\text{DBR}} = \theta Y$, where $\theta \in (0,1)$ represents the present value factor that translates a borrower's income $Y$ into the maximum allowable loan under DBR constraints. This factor depends on the DBR cap (e.g., 40% for employees, 30% for retirees), the interest rate, and the loan maturity.

Solving for the optimal housing level from the first-order conditions yields:

$$H^* = \frac{\alpha(Y+B)}{\alpha+\beta}\frac{1}{P_H} \tag{4}$$

Where $H^*$ is the amount of housing the individual will purchase, which is directly proportional to their income and borrowing capacity, and inversely proportional to the price of housing.

Thus, the net effect on housing purchases depends on the relative changes in borrowing capacity and housing prices. To quantify this relationship, we take the partial derivatives of $H^*$ for each variable:

$$dH^* = \frac{\alpha}{(\alpha+\beta)P_H}dB - \frac{\alpha(Y+B)}{(\alpha+\beta)P_H^2}dP_H \tag{5}$$

When the borrowing cap increases, high-income individuals whose borrowing is constrained by the cap rather than DBR gain access to more credit. As borrowing capacity expands, housing demand rises, putting upward pressure on prices. This creates a tradeoff between increased borrowing capacity and higher housing costs. The net effect on high-income individuals' housing purchases ultimately depends on whether the gains from increased borrowing outweigh the rise in housing prices.

In contrast, as discussed in Section 2.2, most individuals do not benefit from easing the borrowing cap because the DBR constraint already binds them. However, they still face higher housing prices, effectively making them worse off.

From Equation (5), the condition for housing consumption to increase is:

$$dH^* > 0 \quad \text{only if} \quad dB > \frac{Y+B}{P_H}dP_H \tag{6}$$

This condition highlights that loosening borrowing constraints will only increase housing consumption for high-income individuals if the increase in borrowing capacity is large enough to offset the affordability-adjusted rise in housing prices. Otherwise, if borrowing does not grow fast enough relative to housing prices, the price effect dominates, making housing less affordable, even for high-income individuals.

While the model provides valuable insights into credit constraints and housing demand by capturing the immediate distributional effects of credit policy changes, it is essential to acknowledge its limitations. It is intentionally static and focused on short-run comparative statics, abstracting from forward-looking behavior, intertemporal optimization, and speculative dynamics that may shape housing decisions in practice. Additionally, the model treats the housing price, $P_H$, as exogenous. This assumption facilitates closed-form solutions and analytical clarity, but it omits feedback effects that would arise in a general equilibrium setting. In reality, housing prices may respond endogenously to aggregate shifts in credit conditions, as explored in the empirical section. We therefore interpret the model's predictions as partial equilibrium results, conditional on a short-run, supply-inelastic environment. In sum, the model suggests that credit policy can have unequal effects, as borrowing constraints bind differently across the income distribution.

# 4 Methodology

## 4.1 Data and Sample Selection

The data for this study was manually collected from the Kuwait Ministry of Justice (MOJ) website, which publishes all real estate transactions. The MOJ dataset includes the start and end dates of the transaction collection period, which are typically one week. It provides additional information, including transaction prices in KD, property area in $m^2$, and the type of real estate (e.g., residential, investment, commercial). Additionally, it provides descriptions of the properties, including classifications such as land, houses, and buildings.

On November 14, 2018, the CBK increased the individual loan cap from 70,000 KD to

95,000 KD, marking the event date for our study.[4] Our sample period extends from November 12, 2017, to November 14, 2019, covering one year before and one year after the event.[5] We filtered the data to include only transactions involving properties with a minimum area of 280 $m^2$ and a price of at least 70,000 KD. This threshold aligns with the criteria for the Kuwait Credit Bank's interest-free housing loan of 70,000 KD, which requires properties to be at least 280 $m^2$.[6]

The treated group comprises private residential housing properties, including houses and land, because the policy change affects only housing loans for individuals purchasing or building private residences. The control group includes investment housing properties, such as multi-unit buildings intended for leasing and land designated for investment, typically owned by investors or real estate firms. Our final dataset includes 8,232 residential real estate transactions and 1,236 investment real estate transactions. This cross-market sample of residential and investment properties is used to identify the overall causal effect of the policy change on private residential housing prices.

Table 3, Panel A, shows that investment properties tend to have larger areas ($Area_{i,t}$) and higher prices per $m^2$ ($\frac{P_{i,t}}{m^2}$) compared to residential properties, which is expected.[7] Throughout the paper, i indexes individual property transactions and t denotes the week in which the transaction occurs. Investment properties typically include apartment buildings and residential complexes. These properties often have a higher floor area ratio (FAR) and generate income, which drives their prices higher than similarly sized residential real estate. Additionally, we observe that transactions in the post-period constitute 53% of the total for both the

[4]The announcement took place on November 10, 2018, just four days before the implementation on November 14, 2018. We therefore focus on the implementation date as the event, as the two dates cannot be distinguished using weekly data.

[5]Our results remain qualitatively similar when expanding the sample period from July 2017 to March 2020 or narrowing it to February 2018 through August 2019.

[6]The unfiltered dataset includes 8,698 transactions involving private residential houses and land; applying the minimum area (280 $m^2$) and price (70,000 KD) criteria yields 8,232 observations, implying that the excluded segment represents about 5% of total transactions. Residential properties in Kuwait are structurally larger than in many global markets, with 400 $m^2$ being the most common plot size (see Appendix Table A3). Transactions below these thresholds are often *Musha* (partial-ownership) sales, which are not comparable to credit-financed housing purchases.

[7]$\frac{P_{i,t}}{m^2}$ is winsorized at the 1% level to mitigate the influence of outliers.

treated and control groups. This suggests that market liquidity remained comparable across the two groups, conditional on the pre-period.

**Table 3.** Summary Statistics

Panel A: Primary Sample

| | Obs. | min | max | median | mean | std.dev |
|---|---|---|---|---|---|---|
| | | | Investment Properties | | | |
| $Area_{i,t}$ | 1,236 | 303 | 231,803 | 798 | 1,383 | 9,356 |
| $P_{i,t}$ | 1,236 | 180,000 | 98,996,085 | 1,300,000 | 2,046,567 | 4,436,235 |
| Post | 1,236 | 0 | 1 | 1 | 0.53 | 0.5 |
| $\frac{P_{i,t}}{m^2}$ | 1,236 | 653.3 | 5,132.4 | 1,671.0 | 1,828.1 | 760.7 |
| $\ln(\frac{P_{i,t}}{m^2})$ | 1,236 | 6.48 | 8.54 | 7.42 | 7.44 | 0.37 |
| | | | Residential Properties | | | |
| $Area_{i,t}$ | 8,232 | 280 | 13,075 | 400 | 514 | 315 |
| $P_{i,t}$ | 8,232 | 70,000 | 9,000,000 | 280,000 | 336,561 | 270,081 |
| Post | 8,232 | 0 | 1 | 1 | 0.53 | 0.5 |
| $\frac{P_{i,t}}{m^2}$ | 8,232 | 156.3 | 1,573 | 625 | 661.2 | 268.44 |
| $\ln(\frac{P_{i,t}}{m^2})$ | 8,232 | 5.05 | 7.36 | 6.44 | 6.41 | 0.43 |

We next examine the heterogeneous impact of the policy within the residential market. For this analysis, we construct a subsample comprising only residential properties, segmented by size. In this residential subsample, the treated group consists of properties with areas equal to or below 400 $m^2$. This classification is motivated by the institutional lending framework discussed in Section 2.2: while middle- and lower-income households remain unable to enter the private housing market even after the reform, high-income households at the margin of borrowing capacity can enter the market by purchasing smaller residential units.

We therefore hypothesize that the credit limit expansion will have a greater impact on this segment, as marginal increases in borrowing capacity translate into meaningful increases in housing consumption for these constrained high-income entrants. Specifically, the average price per $m^2$ in the residential subsample is 661.2 KD. Assuming both spouses qualify for the increased borrowing limit, the additional 50,000 KD in available credit would allow a household to afford approximately 75 more square meters.

The credit limit expansion increase is substantial for first-time homebuyers, who are more likely to be concentrated in the 280–400 $m^2$ range. Many of these households were previously priced out of the market due to affordability constraints. The expanded credit limit enables such households to enter the market or purchase homes that better meet their needs.

The 400 $m^2$ plot size is not an arbitrary cutoff. It is the most actively traded plot size in both our dataset and the broader Kuwaiti housing market, representing 1,302 pre-policy transactions, more than double the next most common size (500 $m^2$, with 523 transactions).[8] This dominance reflects its role as the standard residential plot size for first-time homebuyers in Kuwait, making 400 $m^2$ a natural and policy-relevant benchmark for defining affordable housing.

This prevalence is rooted in institutional design. PAHW is the primary public housing provider have set 400 $m^2$ as the minimum and most frequently allocated plot size under PAHW Law No. 27 of 1993. Consequently, 400 $m^2$ has become the de facto baseline for public housing distribution, further reinforcing its relevance as the key affordability threshold.

Our selection of 400 $m^2$ is influenced by the larger permissible FAR for these properties, as areas between 250 $m^2$ and 349 $m^2$ are allowed a FAR of 800 $m^2$. In comparison, properties with areas between 350 $m^2$ and 400 $m^2$ have a FAR of 210% plus an additional 120 $m^2$. In contrast, properties with an area of 401 $m^2$ or more are restricted to a FAR of 210%. Consequently, a 400 $m^2$ and a 450 $m^2$ property are permitted the exact 960 $m^2$ of building area.

[8] Table A3 shows the most common plots transacted.

**Table 3.** Summary Stats (continued)

Panel B: Residential Subsample

| | Obs. | min | max | median | mean | std.dev |
|---|---|---|---|---|---|---|
| | | | Residential Area > 400 $m^2$ | | | |
| $Area_{i,t}$ | 4,082 | 400 | 13,075 | 532 | 649.3 | 403.7 |
| $P_{i,t}$ | 4,082 | 75,000 | 9,000,000 | 315,000 | 407,126 | 356,414 |
| Post | 4,082 | 0 | 1 | 1 | 0.55 | 0.5 |
| $\frac{P_{i,t}}{m^2}$ | 4,082 | 156.25 | 1,573.05 | 573.55 | 624.19 | 289.7 |
| $\ln(\frac{P_{i,t}}{m^2})$ | 4,082 | 5.05 | 7.36 | 6.35 | 6.33 | 0.48 |
| | | | Residential Area ≤ 400 $m^2$ | | | |
| $Area_{i,t}$ | 4,150 | 280 | 400 | 400 | 380.73 | 34.3 |
| $P_{i,t}$ | 4,150 | 70,000 | 1,050,000 | 255,000 | 267,152 | 100,212 |
| Post | 4,150 | 0 | 1 | 1 | 0.52 | 0.5 |
| $\frac{P_{i,t}}{m^2}$ | 4,150 | 175 | 1,573.05 | 662.5 | 697.6 | 240.27 |
| $\ln(\frac{P_{i,t}}{m^2})$ | 4,150 | 5.16 | 7.36 | 6.5 | 6.49 | 0.35 |

Panel B of Table 3 shows that the control group has larger areas, which is expected given that the division between the treated and control groups is based on area. Additionally, the control group properties were sold more frequently in the post-period, comprising 55% of control group transactions, compared to 52% for treated group transactions.

The treated group exhibits a higher price per $m^2$, averaging 697 KD, compared to 624 KD for the control group. This difference may arise from several factors: individuals may be willing to pay a premium for smaller areas due to increased competition, smaller properties have a higher FAR per $m^2$, and, as hypothesized, the impact of the credit limit increase is more pronounced on smaller properties.

Although the primary sample and the residential subsample are designed to address different research questions, they complement each other in terms of econometric robustness. A key limitation of the primary sample is the fundamental difference between the treated (residential) and control (investment) groups, which raises concerns about potential omitted variable bias and divergent underlying market dynamics.

The residential subsample mitigates these concerns by restricting the analysis to residential properties for both treatment and control groups, thereby enhancing comparability. However, it introduces a different econometric concern. Under DiD framework, the assumption of no treatment effect on the control group may be violated, since the policy change potentially affects both groups. In contrast, the primary cross-market sample is less susceptible to this particular violation. Importantly, any bias introduced by this violation in the residential subsample is likely to attenuate the estimated effects, making the identification strategy more conservative, as elaborated in Section 4.2.

Finally, we replicated all previous analyses using a placebo event on November 14, 2016. We analyze a placebo sample from November 2015 to November 2017 for this robustness test. This exercise aims to confirm that the observed effects are attributable to the CBK loan cap increase and not influenced by omitted variable bias or violations of the parallel trends assumption.[9]

## 4.2 Empirical Model

Our primary model for estimating the causal impact of the loan cap increase on housing prices employs a DiD approach, as specified in Equation 7.

$$\begin{aligned} ln(\frac{P_{i,t}}{m^2}) = \beta_0 + \beta_1 * Treated_i + \beta_2 * Post_t + \beta_3 * Treated_i * Post_t + \\ City_{FE} + FAR_{FE} + Prop_{FE} + Month_{FE} + \epsilon \end{aligned} \tag{7}$$

Our dependent variable is $\frac{P_{i,t}}{m^2}$, which is log-transformed to allow interpretation of the coefficients in percentage terms. Definitions for all variables are provided in Table 4. The variable $Treated_i$ is a dummy variable that equals one for a residential property transaction in the primary sample ($ResidProp_i = 1$). In the residential subsample, where all observations are classified as "residential" (as described in subsection 4.1), $Treated_i$ equals one for a

[9]Table A1 in the appendix shows the summary statistics for these placebo samples.

smaller, more affordable home ($Small_i = 1$ if $Area_{i,t} \leq 400$).[10] The variable $Post_t$ is a dummy variable that takes the value of one for transactions occurring on or after November 14, 2018.

**Table 4.** Variable Definitions

| Variable | Description |
|---|---|
| $ResidProp_i$ | A dummy variable that takes the value of one in the primary sample if the property is categorized as residential (our $Treated_i$ group in the primary sample). |
| $Small_i$ | A dummy variable that takes the value of one in the residential subsample if the property area is $\leq 400$ $m^2$ (our $Treated_i$ group in the residential subsample). |
| $Post_t$ | A dummy variable that equals one if the weekly observation falls on or after November 14, 2018. |
| $\frac{P_{i,t}}{m^2}$ | The price in KD per $m^2$ for transaction $i$ in week $t$. |
| $P_{i,t}$ | The price in KD for transaction $i$ in week $t$. |
| $Area_{i,t}$ | The area in $m^2$ for transaction $i$ in week $t$. |
| $City_{FE}$ | City fixed effect to control for unobserved factors that vary between cities. |
| $FAR_{FE}$ | Floor area ratio (FAR) fixed effect to control for unobserved factors that vary between different FAR regulations. |
| $Month_{FE}$ | Month fixed effect to control for unobserved factors that vary between months. |
| $Prop_{FE}$ | Property fixed effects to control for unobserved factors that vary between different property types (e.g., house or land). |

The coefficient $\beta_0$ represents the average $\frac{P_{i,t}}{m^2}$ for investment properties in the primary sample or for residential properties greater than 400 $m^2$ in the residential subsample before November 14, 2018. The coefficient on $Treated_i$ captures the difference in $\frac{P_{i,t}}{m^2}$ between investment and residential properties in the primary sample or between residential properties greater than and less than 400 $m^2$ in the residential subsample before November 14, 2018.

The coefficient on $Post_t$ captures the difference in $\frac{P_{i,t}}{m^2}$ between the pre- and post-November 14, 2018, periods, accounting for any time-varying factors that might influence prices. Our primary variable of interest is the interaction term $Treated_i \times Post_t$, which estimates the

[10]The classification of "small" residential units in this subsample is market-relative and specific to the Kuwaiti housing market. While properties between 280 and 400 $m^2$ may be considered medium-to-large by international standards, they represent the lower tail of the eligible residential size distribution in Kuwait.

DiD impact of the new regulation. This term indicates whether the treated observations experienced a differential effect compared to the controls.

We hypothesize that $\frac{P_{i,t}}{m^2}$ for treated observations will increase following the regulation. This is expected because individuals are likely to have more financial resources to allocate toward residential real estate in the primary cross-market sample and particularly toward smaller residential properties in the residential subsample, given the limited supply of residential real estate. The coefficient estimate for the interaction term, $(ResidProp_i \times Post_t)$ and $(Small_i \times Post_t)$, captures the causal effect of the loan cap increase on treated real estate, assuming the DiD assumptions hold (Blundell and Dias, 2009).

The first assumption of the DiD approach is the assumption of parallel trends. This assumption asserts that in the absence of the loan cap regulation, then $\frac{P_{i,t}}{m^2}$ for treated real estate would have followed a similar trajectory as that of control real estate. The parallel trends assumption is satisfied as long as the implementation of the loan cap regulation was exogenous to changes in $\frac{P_{i,t}}{m^2}$. During a press conference, the CBK explained that the last adjustment to the credit limit occurred in 2004. The rationale for the 2018 increase included factors such as population growth, inflation, and rising salaries.[11] To validate the assumption of parallel trends, it is essential to confirm that the treated and control groups did not exhibit different price trajectories before November 14, 2018.

[11] CBK 10/11/2018 press release: https://www.cbk.gov.kw/en/cbk-news/announcements-and-press-releases/press-releases/2018/11/201811100832-press-release-cbk-issued-instructions-regarding-the-rules-regulations-for

## Table 5. Parallel Trend Validation

To test for any violations of the parallel trends assumption before November 14, 2018, we run the following regression:

$$ln(\frac{P_{i,t}}{m^2}) = \beta_1 * Treated_i + \beta_2 * YM_t + \beta_3 * Treated_i * YM_t + City_{FE} + FAR_{FE} + Prop_{FE} + \epsilon$$

Columns 1 and 2 of Table 5 present the results for Samples 1 and 2, respectively. The definitions of the variables used in the analysis are provided in Table 4. Statistical significance is denoted by *, **, and *** for two-sided significance at the 10%, 5%, and 1% levels, respectively. The t-statistics are calculated using standard errors clustered by week $t$.

| | (1) | (2) |
|---|---|---|
| Treated | -0.95<br>t = -12.00*** | 0.06<br>t = 2.03** |
| $Treated_i * YM_{17-12}$ | 0.05<br>t = 0.65 | -0.10<br>t = -1.25 |
| $Treated_i * YM_{18-1}$ | -0.12<br>t = -1.47 | -0.01<br>t = -0.20 |
| $Treated_i * YM_{18-2}$ | -0.03<br>t = -0.37 | -0.01<br>t = -0.12 |
| $Treated_i * YM_{18-3}$ | -0.18<br>t = -2.49** | 0.03<br>t = 0.75 |
| $Treated_i * YM_{18-4}$ | -0.17<br>t = -2.14** | 0.05<br>t = 1.54 |
| $Treated_i * YM_{18-5}$ | -0.09<br>t = -1.18 | 0.03<br>t = 0.68 |
| $Treated_i * YM_{18-6}$ | 0.04<br>t = 0.44 | 0.02<br>t = 0.53 |
| $Treated_i * YM_{18-7}$ | -0.17<br>t = -1.67* | 0.04<br>t = 1.19 |
| $Treated_i * YM_{18-8}$ | -0.16<br>t = -2.09** | 0.02<br>t = 0.51 |
| $Treated_i * YM_{18-9}$ | -0.06<br>t = -0.75 | 0.07<br>t = 1.82* |
| $Treated_i * YM_{18-10}$ | -0.14<br>t = -1.61 | 0.03<br>t = 0.80 |
| City FE | X | X |
| FAR FE | X | |
| Prop FE | X | X |
| Observations | 4,301 | 3,731 |
| Adjusted R2 | 0.78 | 0.64 |

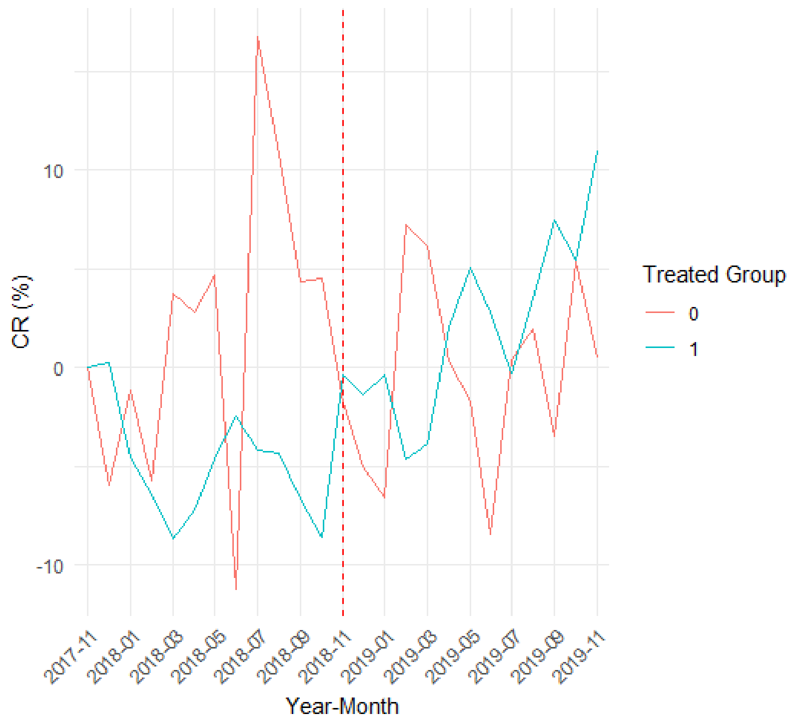
Table 5 formally tests the parallel trends assumption. The variable $YM_t$ represents month-specific dummies, while $Treated_i$ is a dummy indicating whether an observation belongs to the treated group. The interaction term, $Treated_i \times YM_t$, captures whether treated observations exhibit different trends in the months leading up to the credit limit increase.

In Column 1, only four out of eleven interaction terms are statistically significant, suggesting that residential property prices declined relative to investment properties in March, April, July, and August 2018. These negative coefficients suggest that any subsequent price increase following the policy change is unlikely to be driven by pre-existing trends. In Column 2, where the treated and control groups consist of residential properties, most interaction terms remain statistically insignificant, further supporting the parallel trends assumption.[12]

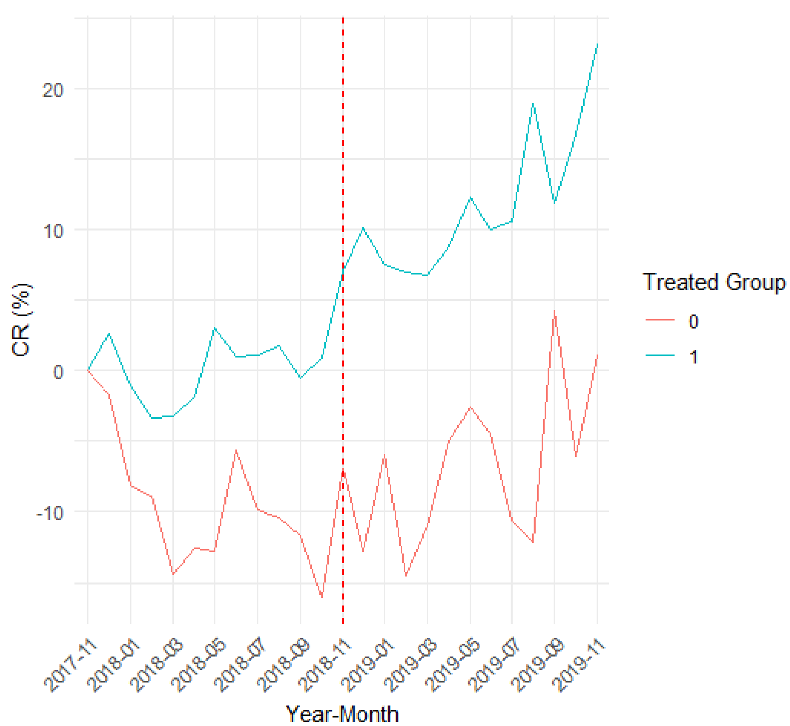
We present Figure 1 to examine the parallel trend assumption visually. Figure 1 presents cumulative returns (CR%) relative to the first observation in November 2017, with all series starting at 0%. Using CR% allows for a more intuitive visual interpretation since it normalizes price changes across groups, enabling a direct comparison between residential and investment properties, which inherently have different price scales.

In the primary sample, prior to the event (indicated by the red dashed line), there is no clear trend in CR% for either the treated or control groups, providing support for the parallel trends assumption. However, the control group (investment properties) exhibits higher volatility, likely due to fewer monthly transactions and the varying characteristics of properties sold in each period. After the event, the treated group (residential properties) shows a clear upward trend in CR%, which is not observed in the control group. This provides preliminary evidence that the credit limit increase exerted upward pressure on residential property prices.

[12]For brevity, coefficients for $YM_t$ are omitted in Table 5.




**(a)** Primary Sample




**(b)** Residential Subsample

**Figure 1.** Visual representation of the parallel trend for Samples 1 and 2.

For the residential subsample, the pre-event period shows no distinct trends for either group, confirming the assumption of parallel trends. Post-event, both treated and control groups exhibit an upward trend in CR%, but the treated group (properties $\leq$ 400 $m^2$) experiences a steeper increase compared to the control group (properties $>$ 400 $m^2$). This suggests that the credit limit increase had a more pronounced impact on smaller residential properties. Overall, these figures collectively provide preliminary evidence supporting the hypothesis that the credit limit increase had unintended consequences, including a rise in residential property prices, with a particularly significant impact on smaller properties.

The second assumption in the DiD framework is the stable unit treatment value assumption (SUTVA), which requires that the treatment status of one group does not affect the outcomes of any other group. In our context, SUTVA could be violated if the increase in the credit limit impacts residential housing directly and influences investment properties through economic spillovers. For instance, the increased spending power of individuals with higher credit limits might boost local businesses, which could indirectly affect investment property prices. If such a violation occurs, it would work against finding a positive effect of the credit limit increase on residential properties, as any indirect benefit would elevate prices across both treated and control groups.

In the residential subsample, where both treated and control observations are residential areas, SUTVA is likely to be violated. Any economic benefits stemming from the increased credit limit would affect both small and larger residential properties, potentially leading to price increases. Nevertheless, this violation would work against documenting a differential impact, as treated and control properties would experience upward pressure on prices.

The final assumption in the DiD framework is perfect compliance, which requires that no units receive the treatment before the policy change and that only the treated group is affected in the post-treatment period. This assumption is likely to hold in the primary sample but not in the residential subsample, where the treated and control groups consist of residential properties.

When perfect compliance does not hold, the monotonicity assumption becomes relevant

(Angrist et al., 1996). This assumption ensures that the treatment does not have an inverse effect on any treated units, meaning there are no cases where an increase in credit limits leads to a decline in property prices. Given that greater credit availability is expected to increase demand rather than suppress it, there is no reasonable mechanism through which monotonicity would be violated in our context. Thus, while compliance is imperfect, our estimates remain informative about the policy's effects.

# 5 Results

## 5.1 Overall Impact on Private Residential Housing Prices

The results in Table 6 indicate that residential property prices increased following the credit limit expansion on November 14, 2018. Column 1 reports estimates based on Equation 7 without fixed effects, while Column 2 includes fixed effects. Column 3 mirrors the specification in Column 2 but employs a placebo date of November 14, 2016.

The coefficient on $Post$ in our main analysis (Columns 1 and 2) is negative but not statistically significant, indicating no significant difference in overall price trends between the pre- and post-periods.

Second, the coefficient on $ResidProp_i$ is negative and statistically significant for Columns 1 and 2, indicating that residential properties are sold on average 65.3% lower than investment properties.[13] This result is expected, given that investment properties have a higher FAR, allowing for more usable space, and they generate income, making them intrinsically more valuable.

[13] $e^{-1.059} - 1 =$ -65.3%.

## Table 6. Primary Sample DiD

This table employs a difference-in-differences methodology to assess the differential behavior of residential properties $\ln(\frac{P_{i,t}}{m^2})$ compared to investment properties $\ln(\frac{P_{i,t}}{m^2})$ after the increase of credit limit on November 14, 2018. In Columns 1 and 2, we make use of the primary sample of residential and investment properties. In Column 1, the analysis follows Equation (7) without incorporating any fixed effects. Column 2 adds the fixed effects. Column 3 repeats the analysis in Column 2 but uses the placebo event of November 2016. Standard errors are clustered by date. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively. Definitions for the variables used are available in Table 4.

| | Main Analysis | | Placebo |
|---|---|---|---|
| | (1) | (2) | (3) |
| Post | -0.055<br>t = -1.63 | -0.035<br>t = -1.23 | -0.06<br>t = -2.60*** |
| $ResidProp_i$ | -1.09<br>t = -37.28*** | -1.059<br>t = -26.03*** | -0.999<br>t = -27.98*** |
| $Post * ResidProp_i$ | 0.113<br>t = 2.93*** | 0.095<br>t = 3.16*** | -0.048<br>t = -2.00** |
| Constant | 7.468<br>t = 281.34*** | | |
| City FE | | X | X |
| FAR FE | | X | X |
| Month FE | | X | X |
| Prop FE | | X | X |
| Observations | 9,468 | 9,468 | 7,343 |
| Adjusted $R^2$ | 0.41 | 0.78 | 0.77 |

Third, our primary variable of interest, the interaction term $Post{*}ResidProp_i$, is positive and statistically significant across Columns 1 and 2. The coefficient in Column 2 indicates that after the credit limit increase, residential property prices increased by 9.97%. The median residential property before the event was sold for 625 $\frac{KD}{m^2}$. Hence, a 9.97% is equivalent to 62.29 $\frac{KD}{m^2}$ increase. For a typical 400 $m^2$ house, this translates to an increase in price of approximately 24,914 KD. This finding suggests that the credit limit increase of 25,000 KD (or 50,000 KD for a married couple) had a substantial upward pressure on residential property prices.[14]

In Column 3, we conduct a placebo test to examine whether our findings are driven by unobserved time trends affecting residential but not investment properties. The placebo results show that the coefficient on $Post * ResidProp_i$ is now negative, indicating that residential prices declined by 4.7% relative to investment properties between November 2016 and November 2017, compared to the prior year. This suggests that prior trend biases may have led to an underestimation of the positive effect in the primary analysis of Columns 1 and 2.

## 5.2 Differential Effects on Small and Large Properties

In this subsection, we focus exclusively on residential properties, excluding investment properties, to assess the differential impact of the credit limit increase on smaller, more affordable homes versus larger, less affordable ones. As discussed in Section 4.1, we hypothesize that smaller residential properties ($\leq 400\ m^2$) would be more affected by the credit limit increase compared to larger properties, as the additional 25,000 KD represents a larger proportion of the base price of smaller properties.

The results in Table 7 support this hypothesis, showing that smaller residential properties increased in price relative to larger properties following the credit limit increase, as evidenced by the main analysis in Columns 1 and 2.

[14]The robustness of our main findings is further supported when restricting the analysis to cities in which both residential and investment properties are present. Because many Kuwaiti cities consist almost entirely of residential plots, the matched samples become smaller; nonetheless, the estimated effects remain stable when implementing exact city matching or propensity-score matching by city and lot size, as reported in Columns 3 and 4 of Table A2. In addition, the results continue to hold even when restricting the matching procedure to Land, only observations, ensuring that treated and control units are directly comparable empty plots, which provides an even more stringent robustness check as reported in Columns 1 and 2 of Table A2.

## Table 7. Residential Subsample DiD

This table employs a difference-in-differences methodology to assess the differential behavior of residential properties $\ln(\frac{P_{i,t}}{m^2})$ with areas $\leq$ 400 $m^2$ compared to $\ln(\frac{P_{i,t}}{m^2})$ of areas $>$ 400 $m^2$ after the increase of credit limit on November 14, 2018. In Columns 1 and 2, we make use of the subsample of residential properties, comparing smaller, more affordable homes ($Area_{i,t} \leq 400$) to larger, less affordable ones ($Area_{i,t} > 400$). In Column 1, the analysis follows Equation (7) without incorporating any fixed effects. Column 2 adds the fixed effects. Column 3 repeats the analysis in Column 2 but uses the placebo event in November 2016. Standard errors are clustered by date. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively. Definitions for the variables used are available in Table 4.

| | Main Analysis | | Placebo |
|---|---|---|---|
| | (1) | (2) | (3) |
| Post | 0.038<br>t = 2.03** | 0.036<br>t = 3.06*** | -0.125<br>t = -7.61*** |
| $Small_i$ | 0.139<br>t = 10.22*** | 0.086<br>t = 7.73*** | 0.066<br>t = 4.59*** |
| Post*$Small_i$ | 0.05<br>t = 2.465** | 0.049<br>t = 3.674*** | 0.026<br>t = 1.42 |
| Constant | 6.305<br>t = 474.87*** | | |
| City FE | | X | X |
| Month FE | | X | X |
| Prop FE | | X | X |
| Observations | 8,232 | 8,232 | 6,417 |
| Adjusted $R^2$ | 0.04 | 0.67 | 0.64 |

First, the *Post* coefficient in Columns 1 and 2 is positive and statistically significant, indicating that residential property prices increased overall after the credit limit increased by 3.7%. While the *Post* coefficient may reflect the impact of the credit limit increase, it could also capture other market-wide factors unrelated to the policy.

Second, the $Small_i$ coefficient in Table 7 is positive and statistically significant. The positive $Small_i$ coefficient suggests that smaller residential properties are sold at a premium of 9% on average, based on Column 2, compared to larger properties. This premium could be due to increased competition for smaller properties, as they are more affordable for a larger pool of potential buyers, or due to zoning regulations that allow a larger FAR for properties below 400 $m^2$ compared to those above 400 $m^2$.[15]

Third, and most importantly, the interaction term $Post * Small_i$ is positive and statistically significant across Columns 1 and 2. The coefficient in Column 2 indicates that, after the credit limit increase, the prices of smaller residential properties rose by 5% relative to those of larger residential properties. The median residential property before the event was sold for 600 $\frac{KD}{m^2}$. Hence, a 5% is equivalent to 30 $\frac{KD}{m^2}$ increase. For a typical 400 $m^2$ house, this translates to an increase in price of approximately 12,000 KD. This finding suggests that the credit limit increase had a substantial impact on the prices of smaller residential properties.[16]

In Column 3, we conduct a placebo analysis using a hypothetical event on November 14, 2016, to test whether unobserved time trends might drive our results. The placebo results show that the $Post * Small_i$ coefficient decreases, from 5% to 2.6%, and is no longer statistically significant. This supports that the observed post-policy price increase in smaller residential properties is not due to broader time trends unrelated to the credit limit increase.

## 5.3 Liquidity in the Housing Market

The preceding sections examined the impact of the 2018 credit expansion on housing prices. In this section, we shift focus to its effect on the number of real estate transactions. While the policy led to higher prices, it may have also boosted transaction volumes. This could reflect a positive outcome: the policy may have enabled families previously priced out of the

[15] We omit FAR fixed effects in Table 7 because including them would absorb all variation in $Small_i$, preventing identification of its coefficient. To address this, we perform several alternative specifications where the definition of "small" is expanded to include plots below 480, 470, 460, and 450 $m^2$, as reported in Columns (1)–(4) of Table A4. In these cases, FAR fixed effects can be included, since the treatment group now spans multiple FAR bands. The estimated effects remain positive and statistically significant across all thresholds, confirming that the results are not mechanically driven by FAR differences. The choice of the 480 $m^2$ upper bound in the sensitivity analysis is guided by Table A3, which shows that plot sizes up to 480 $m^2$ have median pre-policy prices at or below 300,000 KD, whereas larger plots are typically priced above this threshold.

[16] Our conclusions also hold in a restricted sample of plots between 400 and 750 $m^2$, where 400 $m^2$ is defined as the small-unit benchmark (Column 5 of Table A4).

market to purchase homes for the first time.[17]

To examine liquidity, we construct the variable $Transactions_{c,p,t}$, defined as the total number of property transactions aggregated at the weekly level for each city–property-type cell. Each observation corresponds to a specific city (c), property category (p), and week (t). For example, separate observations are recorded for transactions involving land plots in Nuzha (Nuzha–Land-Week X) and for residential houses in the same city (Nuzha–House-Week X) and in the same week X.[18]

Additionally, we control for $Days_t$, which represents the number of business days in a week. As shown in Table 8, a typical week comprises five business days, but this number can vary due to public holidays. We hypothesize that, all else being equal, a higher number of business days correlates with an increased number of transactions, since there is more time to conduct business. Table 8 indicates that the average value of *Post* is 0.5, signifying that the dataset is evenly split, with half of the weeks occurring before the event and the other half following it.

Although residential and investment properties span the same weekly sample period, the number of observations differs because the liquidity analysis is conducted at the city–property-type–week level. Specifically, the panel includes 4,488 observations for investment properties and 15,141 for residential properties, reflecting that investment properties are permitted in fewer cities due to zoning restrictions. Consequently, there are fewer city–week cells in which investment transactions can occur. The average liquidity is lower for investment properties. The mean number of transactions, $Transactions_{c,p,t}$, is 0.28 transactions per week for investment properties, compared to 0.54 transactions per week for residential properties at the same city–property-type level.

Further division of residential property transactions by property size reveals that al-

[17] Kuwait's residential real estate market includes many investors who own multiple plots or units, often holding them as a store of value or informally renting them out. When credit conditions improve, and prices rise, as they did following the 2018 policy, these investors may be more inclined to sell, especially if capital gains begin to outweigh expected rental returns. This increased supply creates entry points for first-time buyers, who may acquire properties that were previously used solely for investment purposes.

[18] Nuzha is a residential city in Kuwait.

though the average $Transactions_{c,p,t}$ is the same at 0.27 for both property groups, those less than 400 $m^2$ and those greater than, the variance within these groups differs. Specifically, residential properties with an area greater than 400 $m^2$ exhibit a higher standard deviation in transaction counts.[19]

To formally test the impact of the 2018 credit expansion, we run the following regression:

$$\begin{aligned} Transactions_{i,t} = \beta_1 * Treated_i + \beta_2 * Post_t + \beta_3 * Treated_i * Post_t + \\ Days_t + City_{FE} + Prop_{FE} + Month_{FE} + \epsilon \end{aligned} \tag{8}$$

**Table 8.** Volume Summary Statistics

This table reports summary statistics for the number of property transactions aggregated at the weekly level for each city–property-type cell. Each observation corresponds to a specific city, property category (e.g., land or house), and week. The number of observations reflects the cross-section of cities and property types observed over time.

| | Obs. | min | max | median | mean | std.dev |
|---|---|---|---|---|---|---|
| | | | Investment Properties | | | |
| Post | 4,488 | 0 | 1 | 0.5 | 0.5 | 0.5 |
| $Days_t$ | 4,488 | 2 | 5 | 5 | 4.87 | 0.54 |
| $Transactions_{i,t}$ | 4,488 | 0 | 16 | 0 | 0.28 | 0.86 |
| | | | Residential Properties | | | |
| Post | 15,141 | 0 | 1 | 1 | 0.5 | 0.5 |
| $Days_t$ | 15,141 | 2 | 5 | 5 | 4.84 | 0.6 |
| $Transactions_{i,t}$ | 15,141 | 0 | 77 | 0 | 0.54 | 1.61 |
| | | | Residential Area > 400 $m^2$ | | | |
| $Transactions_{i,t}$ | 15,141 | 0 | 63 | 0 | 0.27 | 1.18 |
| | | | Residential Area ≤ 400 $m^2$ | | | |
| $Transactions_{i,t}$ | 15,141 | 0 | 22 | 0 | 0.27 | 0.94 |

[19]The number of observations remains unchanged when residential properties are divided into small and large units because the liquidity analysis is conducted at the city–week–property level. In a given city and week, transactions may occur for both small and large residential units; therefore, the panel structure (city × property × week) is preserved, while the transaction counts and their distribution differ across size categories.

Table 9 presents results from Equation 8. Column 1 contrasts investment and residential properties. The positive and significant coefficient for $Post$ suggests a 2.6% increase in $Transactions_{i,t}$ during post-November 14, 2018, relative to pre-November 14, 2018. The significant positive value for $Treated_i$ indicates that residential properties typically have a higher transaction volume. Additionally, $Days_t$ shows a significant positive effect, indicating that transaction volumes increase by 0.114 for each additional day in a week. However, the interaction term Post*$Treated_i$ is insignificant, indicating that the credit limit increase did not significantly affect the liquidity of residential properties compared to investment properties.

**Table 9. Liquidity DiD**

This table employs a difference-in-differences methodology using Equation (8) to assess the differential behavior of the weekly number of transactions at the City-Description level, $Transactions_{i,t}$, after the increase of credit limit on November 14, 2018. Column 1 uses the primary sample and tests the difference between residential properties (treated) and investment properties. Column 2 uses the residential subsample and tests for the difference between residential properties with areas less than or equal to 400 $m^2$ (treated) and residential properties with areas greater than 400 $m^2$. Robust standard errors are used. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively. Definitions for the variables used are available in Table 4.

| | Primary Sample | Residential Subsample |
|---|---|---|
| Post | 0.042 | 0.056 |
| | t = 1.762* | t = 3.198*** |
| $Treated_i$ | 0.156 | 0.023 |
| | t = 4.754*** | t = 1.665* |
| $Days_t$ | 0.114 | 0.065 |
| | t = 7.243*** | t = 7.390*** |
| Post*$Treated_i$ | 0.032 | -0.038 |
| | t = 0.978 | t = -1.651* |
| City FE | X | X |
| Month FE | X | X |
| Prop FE | X | X |
| Observations | 19,629 | 30,282 |
| Adjusted $R^2$ | 0.219 | 0.128 |

Column 2 applies the same analysis to the residential subsample, comparing residential properties by size. The positive and significant $Post$ coefficient again indicates an increase in transactions following the policy change. The $Treated_i$ coefficient, though lower than in

Column 1, is still positive and significant, suggesting that smaller properties ($\leq 400\ m^2$) are more liquid. The interaction Post*$Treated_i$ is negative and significant at the 10% level, suggesting a decrease in liquidity for smaller properties after expansion, likely due to the disproportionate price increase making these homes less attainable.

The results suggest that the credit expansion did not enhance market liquidity as intended, particularly for the segment it aimed to assist most, as evidenced in Column 2. The increase in credit availability may have benefited a narrow group of individuals, injecting more capital into a housing market with limited supply, thus driving up prices and reducing liquidity for smaller, more affordable homes.

# 6 Conclusion

The results of this study demonstrate that the credit limit expansion policy implemented in Kuwait on November 14, 2018, led to a significant increase in residential property prices, particularly affecting smaller, more affordable properties. By utilizing a DiD approach, the findings show an approximately 10% rise in overall residential housing prices, with smaller properties experiencing disproportionately larger increases. Moreover, the anticipated improvement in market liquidity, as measured by transaction volumes, was not realized, highlighting the complexity and potential unintended consequences of credit policy interventions in housing markets.

These findings highlight a core paradox in credit-based housing reforms: while expanded borrowing capacity benefited higher-income households constrained by loan caps, it exacerbated affordability challenges for lower-income groups bound by DBR limits. High-income buyers used the additional credit to bid up prices, especially in the affordable housing segment. Low- and middle-income households, unable to access more credit, were left facing higher prices without corresponding gains in purchasing power. This dynamic widened the affordability gap. Addressing such disparities requires robust supply-side measures, including land releases, zoning reforms, smart urban planning, and partnerships with the private sector to accelerate the delivery of affordable housing. These interventions are critical to ensure that credit expansion translates into genuine improvements in housing access and equity.

The findings in this paper align with those of other studies, which illustrate the unintended consequences of well-intentioned policies. For instance, Carozzi et al. (2024) evaluates the U.K. 'Help to Buy' policy that expanded credit access for home purchases by offering government equity loans, effectively allowing higher loan-to-value ratios for first-time buyers of new-build homes. They find that in supply-constrained regions, such as Greater London, credit expansion primarily inflated house prices without boosting construction activity. Similarly, Park and Kim (2023) found that tightening loan-to-value ratios in Korea widened wealth inequality, echoing our results, where the relaxation of lending constraints disproportionately harmed lower-income groups, as smaller home prices increased the most.

Our paper complements the recent work of Chen et al. (2024), who examine how credit easing influences housing prices across 36 countries using cross-country panel data from 1970 to 2018. While both studies find that credit expansion leads to higher house prices, our analysis highlights a different transmission channel. Specifically, Chen et al. (2024) attributes rising house prices to increased borrowing capacity among low-income households, enabled by relaxed credit constraints and greater leverage. In contrast, we show that the relaxation of an absolute loan cap primarily benefited higher-income borrowers in Kuwait, as the DBR rules remained binding for lower-income households. Our findings thus underscore how the institutional design of credit policy, particularly the coexistence of absolute and relative borrowing constraints, shapes the distributional outcomes of credit expansion.

This study offers a key policy lesson: in supply-constrained markets, credit expansion alone is not only insufficient, but it can also be counterproductive. Without accompanying supply-side interventions, such reforms risk intensifying inequality by fueling price inflation rather than improving access to essential goods and services. This concern is particularly timely, as of 2025, the Kuwaiti government is reportedly preparing a new housing finance initiative offering up to 200,000 KD in loans for first-time homebuyers, 70,000 KD interest-free from the government and 130,000 KD from banks, secured against the property. The proposal also extends repayment terms to 25 years, raises the DBR cap to 50%, and lowers interest rates to 2% above the CBK discount rate, reviewed every five years. Unless matched with policies to expand housing supply, these changes risk repeating the very outcomes this study cautions against.[20]

This research makes several contributions to existing literature. It provides empirical evidence on the impact of relaxing absolute loan caps, a relatively uncommon macroprudential policy, highlighting how such measures can exacerbate affordability challenges in markets characterized by inelastic housing supply. The study underscores the importance of examining distributional impacts, emphasizing how policy interventions affect various housing market segments differently.

The findings carry important policy implications for policymakers worldwide, particularly in countries with economic structures and regulatory environments similar to Kuwait. They suggest that easing credit constraints alone may not be sufficient to address housing affordability issues. Instead, policy measures should be complemented with supply-side reforms that improve housing market responsiveness to increased demand. Policymakers should consider integrated policy frameworks that simultaneously address credit availability, housing supply, and market regulation to prevent unintended adverse outcomes such as exacerbated inequality and diminished affordability.

**Declaration of generative AI and AI-assisted technologies in the writing process**

During the preparation of this work the authors used ChatGPT in order improve readability. After using this tool, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

[20] https://www.alraimedia.com/article/1715884/

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# A Appendix: Tables

**Table A1.** Placebo Summary Statitistcs

Panel A: Placebo Test – Primary Sample

| | Obs. | min | max | median | mean | std.dev |
|---|---|---|---|---|---|---|
| | | | Investment Properties | | | |
| $Area_{i,t}$ | 926 | 285 | 6,173 | 772 | 849 | 494 |
| $P_{i,t}$ | 926 | 150,000 | 10,500,000 | 1,350,000 | 1,582,573 | 1,031,143 |
| Post | 926 | 0 | 1 | 0 | 0.47 | 0.5 |
| $\frac{P_{i,t}}{m^2}$ | 926 | 608.72 | 4,169.91 | 1,799.28 | 1,883.65 | 625.8 |
| $\ln(\frac{P_{i,t}}{m^2})$ | 926 | 6.41 | 8.34 | 7.5 | 7.49 | 0.34 |
| | | | Residential Properties | | | |
| $Area_{i,t}$ | 6,417 | 281 | 15,500 | 400 | 535 | 449 |
| $P_{i,t}$ | 6,417 | 70,000 | 12,000,000 | 265,000 | 346,955 | 388,008 |
| Post | 6,417 | 0 | 1 | 1 | 0.56 | 0.5 |
| $\frac{P_{i,t}}{m^2}$ | 6,417 | 156.25 | 1,681.38 | 595.61 | 647.62 | 284.5 |
| $\ln(\frac{P_{i,t}}{m^2})$ | 6,417 | 5.05 | 7.43 | 6.39 | 6.38 | 0.44 |

Panel B: Placebo Test – Residential Subsample

| | Obs. | min | max | median | mean | std.dev |
|---|---|---|---|---|---|---|
| | | | Residential Area $> 400\ m^2$ | | | |
| $Area_{i,t}$ | 3,031 | 400 | 15,500 | 560 | 707 | 607 |
| $P_{i,t}$ | 3,031 | 74,374 | 12,000,000 | 343,000 | 454,796 | 535,037 |
| Post | 3,031 | 0 | 1 | 1 | 0.54 | 0.5 |
| $\frac{P_{i,t}}{m^2}$ | 3,031 | 156.25 | 1,681.38 | 562.5 | 639.12 | 324.63 |
| $\ln(\frac{P_{i,t}}{m^2})$ | 3,031 | 5.05 | 7.43 | 6.33 | 6.33 | 0.52 |
| | | | Residential Area $\leq 400\ m^2$ | | | |
| $Area_{i,t}$ | 3,386 | 281 | 400 | 400 | 382 | 33.4 |
| $P_{i,t}$ | 3,386 | 70,000 | 850,000 | 230,000 | 250,421 | 96,844 |
| Post | 3,386 | 0 | 1 | 1 | 0.57 | 0.5 |
| $\frac{P_{i,t}}{m^2}$ | 3,386 | 175 | 1,681.38 | 607.16 | 655.24 | 242.81 |
| $\ln(\frac{P_{i,t}}{m^2})$ | 3,386 | 5.16 | 7.43 | 6.41 | 6.42 | 0.35 |

## Table A2. Matching Robustness

This table reports difference-in-differences estimates of $\ln\left(\frac{P_{i,t}}{m^2}\right)$ after the November 14, 2018 credit-limit increase. Columns (1)–(2) restrict the sample to properties classified as "Land" by implementing exact matching on *Description*; Column (2) additionally matches treated units to the nearest control units in lot size using Mahalanobis distance. Columns (3)–(4) implement exact-by-city matching, with Column (4) adding nearest matching on lot size. Standard errors are clustered by date. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Post | -0.191** | -0.193** | -0.029 | -0.027 |
| | $t = -2.205$ | $t = -2.230$ | $t = -0.723$ | $t = -0.586$ |
| $ResidProp_i$ | -1.182*** | -1.178*** | -0.970*** | -0.971*** |
| | $t = -16.627$ | $t = -16.589$ | $t = -30.099$ | $t = -25.265$ |
| Post × $ResidProp_i$ | 0.224** | 0.226*** | 0.105** | 0.102** |
| | $t = 2.561$ | $t = 2.587$ | $t = 2.251$ | $t = 2.051$ |
| $Area_2$ | -0.004*** | | -0.004*** | |
| | $t = -6.300$ | | $t = -16.038$ | |
| Constant | 7.480*** | 7.474*** | 7.434*** | 7.433*** |
| | $t = 102.256$ | $t = 102.359$ | $t = 235.089$ | $t = 196.011$ |
| Description match (exact) | Yes | Yes | No | No |
| City match (exact) | No | No | Yes | Yes |
| Nearest on Area | No | Yes | No | Yes |
| Observations | 3,488 | 3,486 | 1,782 | 1,607 |
| Adjusted $R^2$ | 0.295 | 0.295 | 0.659 | 0.661 |

## Table A3. Median Pre-Policy Prices by Plot Size

This table reports the median property price for each plot size with at least ten pre-policy observations (i.e., prior to the November 14, 2018 credit-limit increase). The table shows the total number of transactions ($n$) and the corresponding median prices across all eligible area categories.

| **Area ($m^2$)** | $n$ | **Median Price (KD)** |
|---|---|---|
| 400 | 1,302 | 255,000 |
| 500 | 523 | 320,000 |
| 600 | 257 | 250,000 |
| 750 | 253 | 550,000 |
| 375 | 231 | 300,000 |
| 1,000 | 154 | 1,000,000 |
| 300 | 140 | 175,000 |
| 450 | 87 | 180,000 |
| 301 | 50 | 155,000 |
| 300.1 | 33 | 180,000 |
| 297.7 | 26 | 158,000 |
| 357.5 | 26 | 151,000 |
| 357 | 25 | 187,000 |
| 392 | 24 | 218,000 |
| 800 | 19 | 1,000,000 |
| 480 | 17 | 172,000 |
| 640 | 17 | 153,000 |
| 540 | 13 | 402,500 |
| 875 | 13 | 969,570 |
| 487.5 | 11 | 450,000 |

## Table A4. Affordability-Threshold Robustness

This table reports difference-in-differences estimates of $\ln\left(\frac{P_{i,t}}{m^2}\right)$ around the November 14, 2018 credit-limit increase using alternative affordability thresholds. Columns (1)–(4) define *Small* as plots with Area $\leq$ 480, 470, 460, and 450 $m^2$, respectively (full private-sample); Column (5) restricts the sample to plots in the 400–750 $m^2$ band and defines *Small* only for 400 $m^2$ plots. All specifications include City, Month, and Property fixed effects; Columns (1)–(4) also include FAR fixed effects. Standard errors are clustered by date. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

| | Full Private Sample | | | | 400–750 $m^2$ Only |
|---|---|---|---|---|---|
| | (1) Small $\leq$480 | (2) Small $\leq$470 | (3) Small $\leq$460 | (4) Small $\leq$450 | (5) Small = 400 |
| Post | 0.036*** | 0.036*** | 0.037*** | 0.036*** | 0.036*** |
| | $t = 2.806$ | $t = 2.881$ | $t = 2.904$ | $t = 2.799$ | $t = 3.098$ |
| $Small_i$ | 0.130*** | 0.121*** | 0.114*** | 0.113*** | 0.074*** |
| | $t = 6.312$ | $t = 5.558$ | $t = 4.836$ | $t = 4.788$ | $t = 5.276$ |
| Post × $Small_i$ | 0.041*** | 0.041*** | 0.040*** | 0.043*** | 0.065*** |
| | $t = 2.723$ | $t = 2.824$ | $t = 2.709$ | $t = 2.925$ | $t = 4.438$ |
| City FE | X | X | X | X | X |
| Month FE | X | X | X | X | X |
| Prop FE | X | X | X | X | X |
| FAR FE | X | X | X | X | |
| Observations | 8,232 | 8,232 | 8,232 | 8,232 | 5,682 |
| Adjusted $R^2$ | 0.676 | 0.675 | 0.674 | 0.674 | 0.683 |