



## Commuting Costs and Urban Housing Prices: Insights from Tehran

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### Abstract

Sharp increases in real gasoline prices raise commuting costs and can alter residential location choices by changing accessibility to employment concentrations. In cities with strong central business districts but emerging polycentric patterns, such shocks may relocate housing demand across locations. This study examines that adjustment in Tehran, a highly car-oriented metropolis. We combine official monthly gasoline-price data with 1.4 million arm's-length apartment transactions recorded between Farvardin 1389 and Esfand 1398 (approximately March 2010 and March 2020), making this one of the largest housing transaction datasets analyzed for Iran to date. Exploiting three discrete fuel-price increases, we estimate a difference-in-differences model comparing housing-price dynamics in central and peripheral districts while controlling for dwelling characteristics, and location and month fixed effects. The preferred six-month-lag specification indicates that housing prices in peripheral areas were approximately 2.1% lower relative to central areas during the defined post-shock periods. Estimates are negative across alternative lag specifications, although their statistical precision is modest. Although small relative to overall housing-market volatility in Iran, this effect reflects a systematic spatial response to changes in commuting costs. The findings are consistent with a modest reallocation of housing demand toward districts with greater employment accessibility. Where housing supply adjusts gradually, such demand shifts may contribute to relative price changes across locations. More broadly, the results highlight the interaction between transportation costs, residential sorting, and housing-market adjustment in a large developing-country metropolis. They suggest that fuel-pricing reforms may generate unintended spatial consequences and should therefore be considered alongside broader transportation and housing policies.

**Keywords:** commuting costs, difference-in-differences, fuel shocks, housing prices, rent gradient.

**JEL Classification:** C23, L91, Q41, R21, R48.

### Introduction

In Iran the retail price of gasoline is set administratively, and every adjustment has posed a persistent challenge for economic policy-makers. Major gasoline price

increases occurred in 2010, 2014, and 2019, largely in connection with subsidy reform efforts and fiscal pressures. These episodes provide a useful setting for examining how changes in transportation costs affect urban housing markets.

Rent-gradient theory (Alonso, 1964; Coulson and Engle, 1987) predicts a systematic link between housing prices, distance from the city center, and commuting costs: as those costs rise, prices should fall more steeply with distance. Hence households in metropolitan Tehran are expected to factor commuting expenses into their location decisions, and gasoline, as a key component of those expenses, should influence the spatial pattern of housing prices.

While classical rent-gradient theory is derived from a monocentric city framework, a large body of evidence suggests that contemporary metropolitan areas increasingly exhibit polycentric spatial structures characterized by multiple employment subcenters and extensive public transportation networks (Anas et al., 1998). Alidadi and Dadashpoor (2018) show that employment activity has become increasingly decentralized and that several employment subcenters have emerged within the metropolitan region. Nevertheless, a limited number of districts continue to function as major destinations for commuting flows. Accordingly, the present study focuses on differences in employment accessibility rather than strict distance from a single central business district.

Although nominal gasoline prices have historically changed infrequently, real prices have fluctuated because of both occasional nominal adjustments and continuous movements in the general price level. Households ultimately respond to the real, not the nominal, price; the present study therefore focuses on real-price shocks.

Because many variables affect housing values, we employ an ordinary least-squares framework complemented by a difference-in-differences (DID) design to isolate the specific contribution of gasoline-price changes. Treatment and control areas are defined with reference to the “travel production” and “travel attraction” statistics reported in the 2010 Statistical Yearbook of Tehran Municipality<sup>1</sup>. Zones that attracted more trips than they generated in 2010—districts 3, 6, 7, 11, and 12—are classified as central, while the others are treated as

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<sup>1</sup>. Although the classification is based on the 2010 Statistical Yearbook of Tehran Municipality, we verified its stability using all available municipal statistical yearbooks through 2020. The same districts (3, 6, 7, 11, and 12) consistently exhibited travel-attraction rates exceeding travel-generation rates throughout the study period, indicating that the classification is stable and not driven by short-run fluctuations.

peripheral (Figure 1). It must be stressed that “peripheral” here refers to travel patterns rather than geography or residents’ income levels.

The underlying transaction data span 120 calendar months, from Farvardin 1389 (March/April 2010) to Esfand 1398 (March 2020). Because observations from Mehr 1395 (October 2016) are excluded owing to missing postal-code information, the final estimation sample contains 119 distinct months. Months in which the real gasoline price exceeded its long-run average are coded as high-price periods; the remaining months form the comparison group. A fuel-price increase is expected to shift housing demand towards central districts.

Although this study focuses on Tehran, the underlying question is relevant to many developing-country metropolitan areas characterized by rapid urban expansion, automobile dependence, and periodically administered fuel prices. Understanding how transportation-cost shocks affect housing-market outcomes contributes to broader debates on urban spatial structure, residential sorting, and the unintended consequences of energy-policy reforms.

Economic policies often generate unintended side effects. By documenting one such spillover from fuel-price policy to the housing market, this paper provides evidence that may help urban planners and energy officials design more coherent, mutually supportive regulations.

The remainder of the paper is organized as follows. Section 2 reviews the related literature. Section 3 outlines the methodology and identification strategy, Section 4 describes the data, Section 5 presents the main results, Section 6 reports robustness checks, and Section 7 concludes.

## **Literature Review**

Households generally factor commuting costs into residential choices, although no study has tested this behavior for Tehran. Evidence from other settings is instructive.

Kim et al. (2005), using a nested-logit framework, show that higher commuting costs and travel times raise the probability of relocating, while households choosing a new dwelling try to minimize those same costs. Chi and Boydstun (2016) reach a similar conclusion with American Housing Survey data: rising gasoline prices push households to move closer to work.

If commuting costs shape location, they should also influence housing prices. In a six-metro sample for 1974-79, Coulson and Engle (1987) find that higher commuting costs steepen the rent gradient, although they caution that the gasoline-price coefficient may be upward-biased for the 1970s. Anas and

Chu (1984) estimate a nested-logit model that yields a negative—but imprecise—elasticity of location demand with respect to commuting costs.

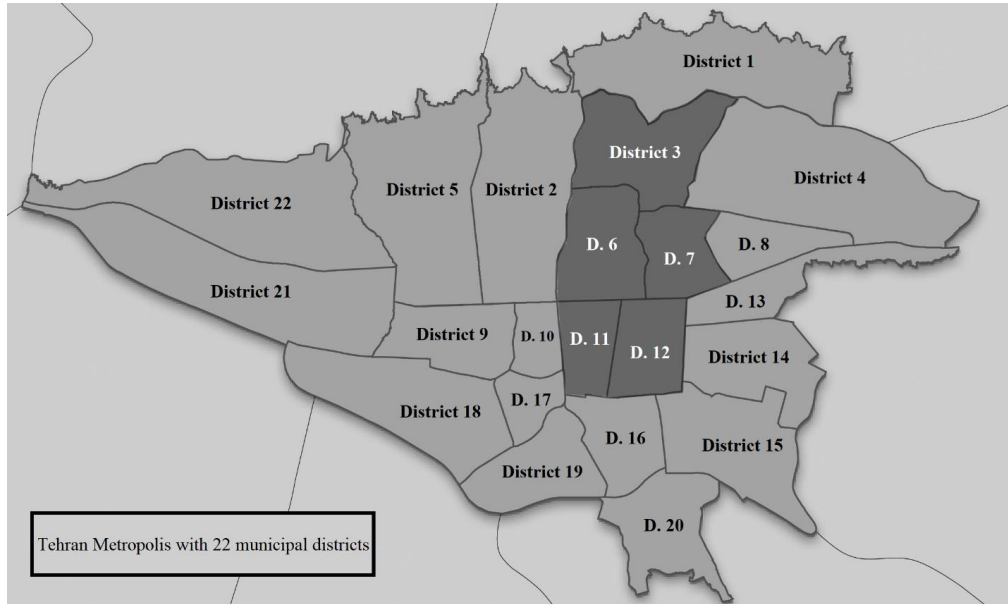


**Figure 1.** Tehran Metropolis with 22 Municipal Districts

**Source:** Research findings.

**Note:** Districts 3, 6, 7, 11, and 12 have higher travel attraction rates than travel production rates in 2010 and are identified as central areas.

Changes in commuting costs affect not only demand but also density and metropolitan size. Tanguay and Gingras (2011) document a significant negative link between gasoline prices and urban sprawl in 12 Canadian cities. Young et al. (2016) show that each 1 % rise in gasoline prices trims low-density fringe areas by 0.17 %. For the United States, Molloy and Shan (2011) combine DID with weighted least squares and find that a 10 % gasoline-price increase cuts new construction in peripheral zones by 10 % over four years, while in dense centres the main effect is higher prices rather than more building.

Evidence for Tehran itself is limited. Yazdanifard et al. (2021) use a DID design on apartment-transaction data and report that new metro stations raise nearby prices by 11% in northern, high-income districts but by only 6% (or not at all) in central and southern areas. The heterogeneity underscores how transport-cost shocks can have location-specific effects within the same city.

Together these studies support the working hypothesis that real-world shifts in commuting costs—whether via fuel prices or transit improvements—reallocate

housing demand between central and peripheral districts and alter the spatial structure of metropolitan areas.

### **Methodology**

This study examines whether increases in commuting costs, induced by gasoline-price shocks, affect the spatial distribution of housing prices within Tehran. The empirical analysis is based on a Difference-in-Differences (DID) design that exploits differential exposure to commuting-cost increases across districts.

The primary assumption of this study is that the majority of businesses in Tehran are concentrated in areas with higher travel attraction rates compared to travel production rates. Additionally, since metro and intra-city bus ticket prices are set annually by the Tehran City Council, we treat public transportation costs as exogenous. Furthermore, given the significant share of personal vehicles in intra-city travel in Tehran, we posit that an increase in real gasoline prices leads to higher commuting costs for residents. As discussed in the literature review, we assume that households in Tehran consider commuting costs when making decisions about their residential location. This research aims to answer whether positive shocks to real gasoline prices, through increased commuting costs, lead to a decrease in housing prices in the peripheral areas of the city.

### **Identification Strategy**

The identification strategy exploits the fact that gasoline-price increases represent city-wide shocks that affect all households simultaneously but are expected to generate heterogeneous effects across locations. Households residing in peripheral districts generally face longer commuting distances and depend more heavily on daily travel to employment centers. Consequently, increases in gasoline prices are expected to raise commuting costs more strongly for peripheral residents than for households located in districts with higher employment accessibility.

The DID framework compares changes in housing prices between peripheral and central districts before and after gasoline-price shocks. The identifying assumption is that, absent gasoline-price shocks, housing prices in the two groups would have followed similar relative trends. Under this assumption, the interaction between the treatment and post-shock indicators captures the differential effect of increased commuting costs on housing prices in peripheral districts relative to central districts.

Although gasoline-price shocks occurred amid inflation, exchange-rate volatility, and sanctions-related disruptions, these developments affected the

Tehran housing market as a whole, even if they may not have affected central and peripheral areas identically. Month fixed effects absorb their common citywide component, while the DID estimates may still be affected by unobserved shocks with differential spatial effects. The empirical strategy therefore exploits differences in exposure to commuting-cost increases across locations rather than aggregate housing-market movements. The transaction-level specification includes sub-district and month fixed effects. Sub-district fixed effects account for time-invariant differences in housing quality, accessibility, and neighborhood amenities, whereas month fixed effects absorb shocks common to the entire Tehran housing market, including aggregate inflation, exchange-rate movements, and sanctions-related macroeconomic developments. Because treatment status is spatially defined and transaction outcomes may be correlated within sub-districts over time, all reported standard errors are clustered at the sub-district level. The estimation therefore permits arbitrary heteroscedasticity and within sub-district serial correlation across the 63 sub-districts.

The treatment indicator equals one for transactions located in peripheral areas and zero for transactions located in central areas. The post-shock indicator varies across alternative lag specifications and identifies months in which the real gasoline price remained elevated following a gasoline-price shock. The coefficient of interest is the interaction between the peripheral-area indicator and the post-shock indicator. It measures the change in housing prices in peripheral areas relative to central areas during the specified post-shock period, conditional on observable dwelling characteristics, sub-district fixed effects, month fixed effects, and frame-type fixed effects. The DID design does not require peripheral and central housing units to be identical; rather, it requires that their relative price trends would have evolved similarly in the absence of gasoline-price shocks.

Figure 2 plots average log real housing prices in central and peripheral districts from the beginning of the sample until immediately before the second gasoline-price shock. Prior to the first gasoline-price shock, the two series exhibit broadly similar movements and a relatively stable gap, providing preliminary visual support for the parallel-trends assumption underlying the DID framework. Following the shock, housing prices in peripheral districts evolve less favorably relative to central districts. While visual inspection cannot establish the parallel-trends assumption definitively, the figure suggests no obvious violation of the identifying assumption.

To complement the graphical evidence, we conduct a joint test of the interactions between the peripheral-area indicator and eight pre-shock month

indicators for the first gasoline-price shock. The test fails to reject the null hypothesis of no differential pre-trend ( $F(8,62)=0.69$ ,  $p=0.699$ ). Thus, in the uncontaminated pre-shock period, we find no statistical evidence that housing-price trends differed systematically between peripheral and central areas before the first shock. This result is consistent with, but does not prove, the parallel-trends assumption. For the second shock, the nominal pre-shock period overlaps with the lagged exposure period following the first shock, leaving only a short clean interval. A comparable joint pre-trend test for the second shock would therefore have limited power and is not emphasized in the analysis.

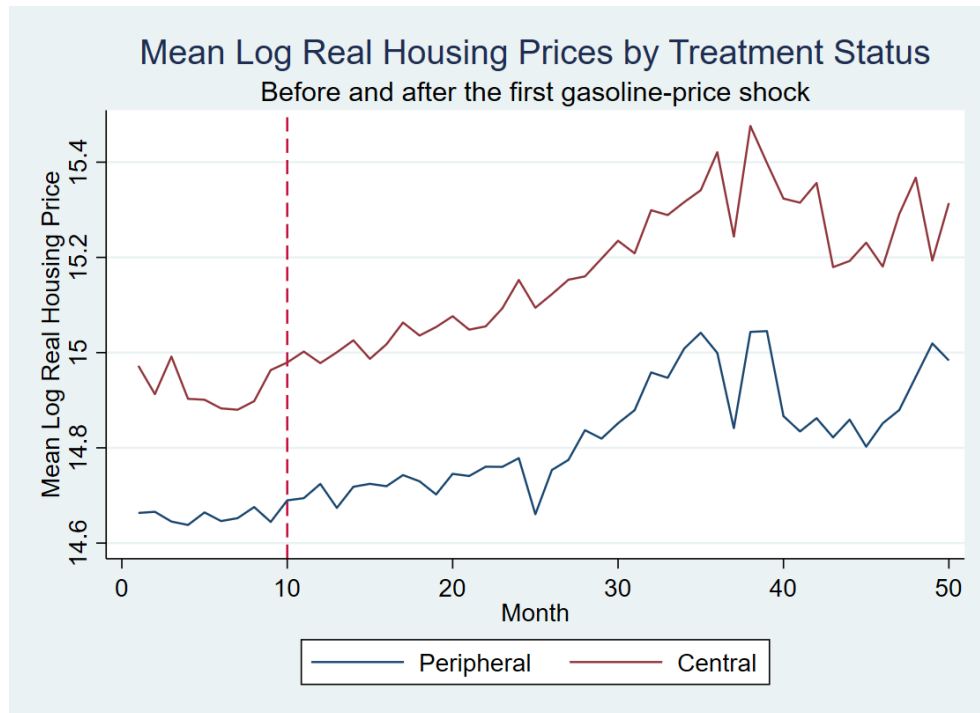


**Figure 2.** Mean Log Real Housing Prices in Central and Peripheral Districts Before and After the First Gasoline-Price Shock

**Source:** Research findings.

### Model Specification

A simple regression equation to estimate the desired effect of residential unit  $i$  at time  $t$  can be as follows:

$$\ln \text{Price}_{it} = \alpha + \beta_1 \text{Treatment}_{it} + \beta_2 \text{Post}_{it} + \beta_3 (\text{Treatment}_{it} * \text{Post}_{it}) + \epsilon_{it} \quad (1)$$

By adding independent variables and fixed effects, we will have:

$$\begin{aligned} \ln Price_{it} = & \alpha + \beta_1 Treatment_{it} + \beta_2 Post_{it} + \beta_3 (Treatment_{it} * Post_{it}) \\ & + \lambda_1 Age_{it} + \lambda_2 Age_{it}^2 + \gamma_1 Area_{it} + \gamma_2 Area_{it}^2 \\ & + \sigma_1 subdistrict + \sigma_2 month + \sigma_3 Frame + \epsilon_{it} \end{aligned} \quad (2)$$

The variables in the regression equation are defined as follows:

1. Logarithm Housing Prices ( $\ln Price_{it}$ ): This is the dependent variable in the regression model. Since price values cannot be negative, we utilize the logarithm of real housing prices instead of actual values. This transformation reduces the influence of extreme price observations and allows the estimated coefficients to be interpreted approximately as proportional changes. It does not, by itself, guarantee homoscedastic errors; therefore, inference is based on sub-district-clustered standard errors.
2. Treatment Variable ( $Treatment_{it}$ ): This binary variable distinguishes peripheral from central locations. Following the Tehran Municipality Statistical Yearbook for 2010, municipal districts with travel attraction greater than travel production—Districts 3, 6, 7, 11, and 12—are classified as central areas and assigned a value of zero. Districts with travel production greater than travel attraction are classified as peripheral areas and assigned a value of one. The treatment indicator is time-invariant and captures differential exposure to increases in commuting costs rather than differential exposure to the citywide gasoline-price shocks themselves.
3. Post Variable ( $Post_{it}$ ): The post-shock indicator is not defined as a permanent dummy that takes the value one after each nominal gasoline-price increase. Instead, it identifies months in which the real gasoline-price increase is expected to affect commuting costs after allowing for alternative adjustment lags. A month is classified as post-shock only when two conditions are met: first, the relevant adjustment lag following a gasoline-price shock has elapsed; and second, the real gasoline price remains above its ten-year average.

The ten-year average is calculated using the full study period and serves as a common empirical benchmark for distinguishing sustained periods of elevated real gasoline prices from periods in which the real value of a nominal price increase has been substantially eroded by inflation. This benchmark uses the full variation in real gasoline prices observed in the available data rather than selecting a particular pre-shock month or price level as the reference point. Although no single threshold is uniquely determined by theory, alternative reference points would also require a discretionary choice. The ten-year average provides a transparent, consistently applied, and reproducible rule across all three gasoline-price shocks.

We estimate separate specifications using three-, six-, nine-, and twelve-month adjustment lags. Each specification therefore has its own post-shock indicator rather than relying on a single common Post variable. Appendix Table A1 reports the exact month indices classified as post-shock under each lag specification.

4. Age of Housing Unit ( $Age_{it}$ ): This variable is included to control for the effects of the age and condition of the housing structure. The squared term of this variable is also incorporated to estimate potential nonlinear effects.

5. Surface Area of Housing Unit ( $Area_{it}$ ): It is anticipated that the area of the housing unit correlates with variables such as the number of rooms and parking spaces. By including this variable in the regression equation, we aim to reduce bias stemming from omitted variables affecting model coefficients. Additionally, the area itself is a significant determinant of housing prices; thus, its squared term is also included.

6. Fixed Effects for Location (Sub-district): To control for location-specific effects in Tehran, fixed effects at the sub-district level are utilized. Detailed information on how these geographical units were established can be found in the appendix.

7. Fixed Effects for Time (Month): Month fixed effects are included for all observed transaction months to absorb temporal variation common to all Tehran locations, including aggregate housing-market movements, inflation, exchange-rate changes, and other citywide shocks. The original study period spans 120 calendar months, Farvardin 1389 (March 2010) through Esfand 1398 (March 2020). October 2016 is excluded because postal-code information is unavailable, leaving 119 distinct transaction months for estimation.

8. Fixed Effects for Frame Structure (Frame): The dataset reports nine different frame structures for housing units. Given that this variable takes on only nine distinct values and over 98% of observations pertain to metal and concrete frames, it is treated as a fixed effect in the model.

The original dataset is best characterized as repeated cross-sectional transaction data rather than a conventional longitudinal housing panel. Although some residential units are observed more than once, most units appear only once during the study period. The baseline analysis therefore estimates a hedonic DID model at the transaction level using pooled OLS with dwelling characteristics, sub-district fixed effects, month fixed effects, frame-type fixed effects, and sub-district-clustered standard errors. This choice is not based on the unbalanced nature of the transaction records. Rather, it follows from the fact that the same dwelling

is generally not observed repeatedly over time. A pseudo-panel analysis based on sub-district-by-month averages is subsequently used as a robustness exercise.

### Data

The Housing Planning and Economic Office of the Ministry of Roads and Urban Development has published raw data on residential apartment transactions across the country. The transaction data cover Farvardin 1389 (March 2010) through Esfand 1398 (March 2020), corresponding approximately to spring 2010 through winter 2020. This dataset includes transaction codes, transaction dates, provinces, counties, municipal districts, land use types, property types (apartments), structural types, postal codes, total property prices, price per square meter, unit surface areas, building ages, and the ratio of transactions to units sold.

The empirical analysis combines transaction-level housing data with information on real free-market gasoline prices and the geographic location of properties. Three major positive gasoline-price shocks occurred during the study period: Dey 1389 (T=10; December 22, 2010–January 20, 2011), Ordibehesht 1393 (T=50; April 21–May 21, 2014), and Aban 1398 (T=116; October 23–November 21, 2019). The month index is defined relative to Farvardin 1389 (T=1). Thus, the third shock occurred in Aban 1398 and corresponds consistently to month 116 of the sample.

The lag-specific post-shock periods used in the DID specifications are reported in Appendix Table A1. The table identifies the exact month indices classified as post-shock for the three-, six-, nine-, and twelve-month adjustment windows.

**Table 1.** Summary Statistics: The Average of Price, Age and Surface Area of Residential Units in terms of Treatment and Control Groups

|                                | Control |        | Treatment |        | Total     |        |
|--------------------------------|---------|--------|-----------|--------|-----------|--------|
|                                | Mean    | S.D.   | Mean      | S.D.   | Mean      | S.D.   |
| Price<br>(M. Touman*)          | 529.40  | 1800   | 440.50    | 2130   | 456.90    | 2073   |
| Age                            | 11.59   | 11.55  | 8.56      | 8.65   | 9.12      | 9.33   |
| Surface Area<br>(Square Meter) | 107.92  | 310.62 | 106.17    | 364.02 | 106.49    | 354.77 |
| Observations                   | 265,600 |        | 1,172,603 |        | 1,438,203 |        |

**Source:** Research findings.

The table above summarizes information on the prices of transacted residential units and their characteristics, including area and building age. Notably, the dataset consists of approximately 1.4 million observations derived from 1.16 million residential units. During the 120-calendar-month study period, most residential units were transacted only once, with an average of approximately 1.2 transactions per unit. The transaction-level dataset is therefore best characterized as repeated cross-sectional data rather than a conventional longitudinal housing panel.

To classify central and peripheral areas, we use travel production and travel attraction rates reported in the 2010 Statistical Yearbook of Tehran Municipality. Travel production denotes trips originating in a traffic zone, whereas travel attraction denotes trips ending in that zone. Districts in which travel attraction exceeds travel production are used as a proxy for relatively employment-accessible central areas; districts in which travel production exceeds travel attraction are classified as peripheral areas. This classification is intended to capture relative exposure to commuting-cost increases and does not imply that all employment is located exclusively in central districts.

In this study, we identified shocks using the price of free-market gasoline, excluding subsidized gasoline. Thus, "gasoline price" in this research specifically refers to free-market gasoline prices. Over the 10-year study period, nominal gasoline prices increased sharply on three occasions: December 2010-January 2011, April-May 2014, and October-November 2019, corresponding respectively to Dey 1389 (T=10), Ordibehesht 1393 (T=50), and Aban 1398 (T=116). The analysis focuses on changes in the real price of gasoline because the commuting-cost implications of nominal price increases erode under sustained inflation.

The fluctuations in real gasoline prices are illustrated in the accompanying graph, with the ten-year average price indicated by a dashed line (see Figure 3).

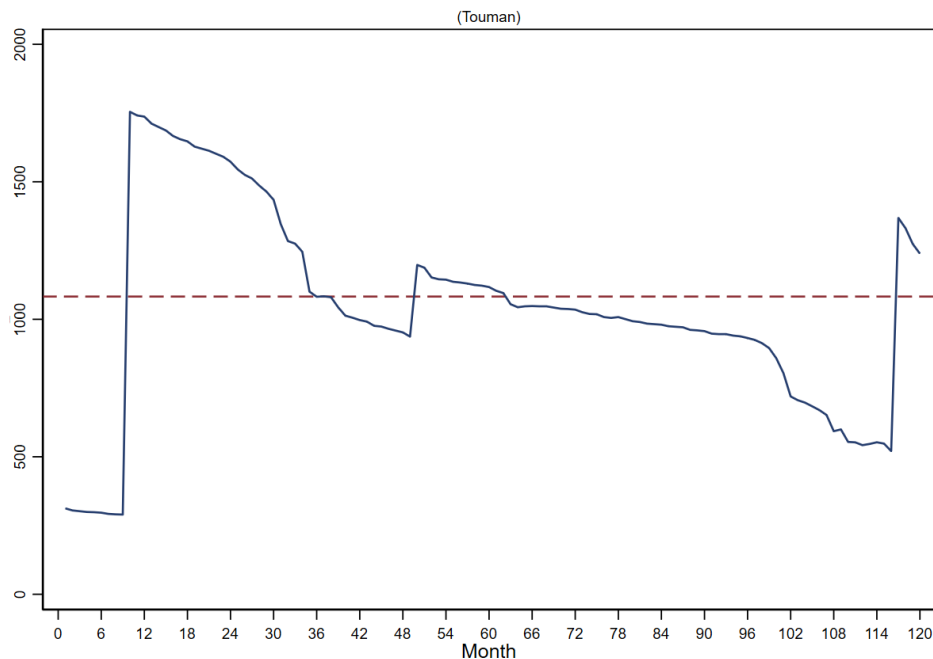


**Figure 3.** Gasoline Real Prices, Compared with its Mean

**Source:** Research findings.

**Note:** The Touman is the informal but widely used unit of currency in Iran that is equivalent to 10 Iranian Rials.

This study utilizes data from the Ministry of Roads and Urban Development. It is important to note, however, that the dataset includes transactions for both residential and non-residential units. Since our primary objective is to investigate the impact of gasoline price shocks on housing prices, all non-residential transaction data was excluded.

The data collected by the Ministry is submitted by real estate agents through a registration system. Consequently, there may be instances where the recorded prices are lower than the actual transaction prices due to concerns about potential government taxation. Any underreporting of transaction prices would threaten the estimates only if it changed systematically across central and peripheral areas in a manner correlated with gasoline-price shocks. Although the available data do not permit a direct test of this possibility, the analysis assumes that reporting practices do not change differentially across the two area types during the relevant shock periods.

The surface-area variable contains extreme observations that are likely to reflect recording errors or atypical transactions. To reduce the influence of such

observations, we exclude the top and bottom 0.5 percent of the surface-area distribution before estimation.

Postal-code information is unavailable for transactions recorded in October 2016. Because postal codes are required to assign transactions to sub-districts and construct the geographic treatment measure, all transactions from this month are excluded. The original 120-calendar-month study period therefore yields 119 usable transaction months.

The central-peripheral classification is constructed from travel production and attraction rates for Tehran's 22 municipal districts. District 9 is classified as peripheral despite the presence of Mehrabad Airport because the available district-level trip statistics do not distinguish airport passengers from airport employees. Consequently, the airport's high trip-attraction measure cannot be interpreted as evidence of ordinary employment accessibility for district residents.

## **Results**

The baseline results use the transaction-level DID specification and a six-month adjustment lag. The study period contains 120 calendar months, of which 119 are usable transaction months after the exclusion of October 2016. The first and second gasoline-price shocks are included in the baseline specification. Because the third shock occurred in Aban 1398 (T=116), only four calendar months remained before the end of the study period, and the available data do not permit estimation of a six-month adjustment window for that shock. The lag-specific post-shock periods are defined in Appendix, Table A1.

### **Average Effect of the First Two Gasoline-Price Shocks**

Table 2 presents the results of five regression specifications, with successive columns incorporating additional controls and fixed effects. The table estimates the relative change in housing prices in peripheral areas compared with central areas during the elevated-real-gasoline-price periods associated with the first two gasoline-price shocks, allowing for a six-month adjustment lag. The preferred specification is Column (5), which includes dwelling characteristics, location fixed effects, month fixed effects, and building-frame controls. The simpler specifications are presented primarily to demonstrate the stability of the estimated treatment effect across alternative model specifications. Standard errors are clustered at the sub-district level in all columns, allowing for heteroscedasticity and arbitrary serial correlation among transactions within each of Tehran's 63 sub-districts.

Column (1) presents the simple DID specification, including the treatment indicator, the post-shock indicator, and their interaction, without sub-district or month fixed effects. The estimated DID coefficient is  $-0.0387$  and is not statistically significant. Column (2) adds sub-district and month fixed effects. The coefficient decreases in magnitude to  $-0.0257$  and is statistically significant at the 5% level. The R-squared increases from 0.013 to 0.516, reflecting the substantial explanatory contribution of location and time fixed effects.

Columns (3) through (5) progressively add dwelling characteristics and building-frame controls. The preferred specification in Column (5) includes age, surface area, their squared terms, building-frame controls, sub-district fixed effects, and month fixed effects. The estimated DID coefficient is  $-0.0208$  ( $SE=0.0121$ ,  $p=0.091$ ), which is statistically significant at the 10% level under sub-district-clustered inference. The estimate implies that, during the defined six-month-lag post-shock periods, real housing prices in peripheral areas were approximately 2.1% lower relative to central areas than during other observed months. The model explains approximately 57% of the variation in log real housing prices.

The estimated interaction coefficient remains negative across all five specifications and ranges from  $-0.0257$  to  $-0.0208$  in Columns (2) through (5).

After sub-district and month fixed effects are introduced, its magnitude remains broadly similar as dwelling and building characteristics are added. This pattern indicates that the negative estimate is not highly sensitive to these alternative observable controls. However, the preferred estimate is statistically significant only at the 10% level under clustered inference and should therefore be interpreted as suggestive rather than conclusive evidence of a relative housing-price response.

Table 3 examines sensitivity to alternative adjustment lags of three, six, nine, and twelve months. These specifications assess whether the estimated relative price effect depends materially on the assumed time required for commuting-cost changes to be reflected in residential location and housing-market adjustment.

The estimated DID coefficients are negative across all four adjustment lags and range from approximately  $-1.5\%$  to  $-2.1\%$ . Their statistical precision varies: the six- and twelve-month estimates are statistically significant at the 10% level using sub-district-clustered standard errors, whereas the three- and nine-month estimates are not statistically significant at conventional levels. The broadly similar signs and magnitudes are consistent with the hypothesized relative decline in peripheral-area housing prices, although the evidence remains statistically modest.

**Table 2.** Transaction-Level DID Estimates for the First Two Gasoline-Price Shocks: Six-Month Lag

| Regression Equations        | (1)                  | (2)                   | (3)                         | (4)                           | (5)                           |
|-----------------------------|----------------------|-----------------------|-----------------------------|-------------------------------|-------------------------------|
| Treatment=1                 | -0.168<br>(0.219)    | 1.795***<br>(0.00534) | 1.775***<br>(0.00516)       | 1.607***<br>(0.0194)          | 1.617***<br>(0.0197)          |
| Post=1                      | Post=1               | -0.116***<br>(0.0218) | 0.200***<br>(0.0231)        | 0.150***<br>(0.0222)          | 0.150***<br>(0.0218)          |
| Treatment*Post=1            | -0.0387<br>(0.0263)  | -0.0257**<br>(0.0128) | -0.0243*<br>(0.0124)        | -0.0221*<br>(0.0122)          | -0.0208*<br>(0.0121)          |
| Age                         | -                    | -                     | -0.0128***<br>(0.001)       | -0.0138***<br>(0.0009)        | -0.0127***<br>(0.0009)        |
| Age <sup>2</sup>            | -                    | -                     |                             | 0.0000191**<br>(0.00000878)   | 0.0000177**<br>(0.00000817)   |
| Surface Area                | -                    | -                     | 0.0000604***<br>(0.0000107) | 0.00260***<br>(0.000291)      | 0.00258***<br>(0.000292)      |
| (Surface Area) <sup>2</sup> | -                    | -                     |                             | -0.000000419***<br>(4.55e-08) | -0.000000416***<br>(4.55e-08) |
| Constantt                   | 19.685***<br>(0.003) | 19.614***<br>(0.003)  | 17.687***<br>(0.012)        | 17.535***<br>(0.026)          | 17.748***<br>(0.026)          |
| Sub-district FE             | No                   | Yes                   | Yes                         | Yes                           | Yes                           |
| Time Fixed Effect           | No                   | Yes                   | Yes                         | Yes                           | Yes                           |
| Frame                       | No                   | No                    | No                          | No                            | Yes                           |
| N                           | 1,438,203            | 1,438,203             | 1,438,203                   | 1,438,202                     | 1,438,201                     |
| R-squared                   | 0.0133               | 0.516                 | 0.534                       | 0.570                         | 0.571                         |

**Source:** Research findings.**Note:** \*Significant at 10%, \*\* Significant at 5%, \*\*\* Significant at 1%, Standard errors are clustered at the sub-district level.

The consistently negative DID coefficients are also consistent with the theoretical argument developed in Section 2. Higher real gasoline prices increase commuting costs, which may reduce the relative attractiveness of locations

requiring longer travel to major employment concentrations. As households and the housing market require time to adjust to changes in commuting costs, this mechanism may be reflected gradually in relative housing prices. The estimates are therefore consistent with a modest relative decline in peripheral-area housing prices during the defined post-shock periods, rather than a large or immediate market response.

**Table 3.** Transaction-Level DID Estimates under Alternative Adjustment Lags

|                      | Three<br>Months          | Six<br>Months            | Nine<br>Months           | Twelve<br>Months         |
|----------------------|--------------------------|--------------------------|--------------------------|--------------------------|
| Treatment=1          | 1.616***<br>(0.0198)     | 1.617***<br>(0.0197)     | 1.617***<br>(0.0196)     | 1.617***<br>(0.0195)     |
| Post=1               | 1.031***<br>(0.0234)     | 0.142***<br>(0.0218)     | 0.229***<br>(0.0212)     | 0.243***<br>(0.0204)     |
| Treatment=1 * Post=1 | -0.0154<br>(0.0124)      | -0.0208*<br>(0.0121)     | -0.0188<br>(0.0116)      | -0.0199*<br>(0.0102)     |
| Age                  | -0.0127***<br>(0.000955) | -0.0127***<br>(0.000955) | -0.0127***<br>(0.000955) | -0.0127***<br>(0.000955) |
| Area                 | 0.00258***<br>(0.000292) | 0.00258***<br>(0.000292) | 0.00258***<br>(0.000292) | 0.00258***<br>(0.000292) |
| Constant             | 18.57***<br>(0.0668)     | 18.57***<br>(0.0670)     | 18.57***<br>(0.0673)     | 18.57***<br>(0.0674)     |
| Sub-district FE      | Yes                      | Yes                      | Yes                      | Yes                      |
| Month FE             | Yes                      | Yes                      | Yes                      | Yes                      |
| Observations         | 1438201                  | 1438201                  | 1438201                  | 1438201                  |
| R-squared            | 0.571                    | 0.571                    | 0.571                    | 0.571                    |

**Source:** Research findings.

**Note:** Standard errors in parentheses, \*  $p < 0.10$ , \*\*  $p < 0.05$ , and \*\*\*  $p < 0.01$ . SE are clustered at sub-district level.

### Results for the First Gasoline-Price Shock

Table 4 examines the first gasoline-price shock separately. To avoid overlap with the second shock, observations from month 50 onward are excluded. The table reports estimates using three-, six-, nine-, and twelve-month adjustment lags, with the corresponding post-shock periods defined in Appendix Table A1.

The isolated first-shock estimates are imprecisely estimated. Although the coefficient becomes negative at longer adjustment lags, none of the four lag-specific estimates is statistically significant using sub-district-clustered standard errors. The first-shock analysis therefore does not provide precise standalone evidence of a peripheral-relative housing-price response. This lack of precision

may reflect the limited information available when the analysis is restricted to a single shock, rather than evidence of no effect.

A plausible institutional explanation for the imprecise estimates of the first gasoline-price shock is the compensatory cash-transfer program introduced simultaneously with the 2010 Targeted Subsidy Reform. At the beginning of the reform, households received IRR 455,000 per month for each household member, equivalent to approximately US\$45 at the contemporaneous market exchange rate. The payment was not indexed to inflation or exchange-rate depreciation. From late 2011 and throughout 2012, the Obama administration's nuclear-related sanctions—particularly the tightening of restrictions on Iran's petroleum sector and financial transactions involving the Central Bank—substantially constrained Iran's access to foreign exchange and international financial channels. These sanctions were a major driver of the sharp depreciation of the Rial, although domestic macroeconomic weaknesses also contributed. As the market exchange rate deteriorated, the dollar equivalent of the unchanged nominal transfer fell to roughly US\$15–16 by late 2012, while its domestic purchasing power was further eroded by inflation. Nevertheless, the initially substantial transfer may have cushioned the increase in household transport expenditures and enabled households to maintain routine intra-urban travel despite the first gasoline-price increase. If so, the relative capitalization of higher commuting costs into peripheral housing prices may have been weaker after the first shock. This mechanism remains an interpretation rather than a tested result, because the transaction data do not observe households' receipt or use of transfers, travel behavior, or residential-location decisions.

**Table 4.** Transaction-Level DID Estimates for the First Gasoline-Price Shock

|                     | Three<br>Months      | Six<br>Months        | Nine<br>Months       | Twelve<br>Months     |
|---------------------|----------------------|----------------------|----------------------|----------------------|
| Treatment=1         | 1.591***<br>(0.010)  | 1.599***<br>(0.011)  | 1.604***<br>(0.014)  | 1.607***<br>(0.014)  |
| Post=1              | 0.300***<br>(0.021)  | 0.227***<br>(0.021)  | 0.249***<br>(0.023)  | 0.241***<br>(0.025)  |
| Treatment=1* Post=1 | 0.007<br>(0.011)     | -0.004<br>(0.014)    | -0.012<br>(0.019)    | -0.017<br>(0.021)    |
| Age                 | -0.031***<br>(0.002) | -0.031***<br>(0.002) | -0.031***<br>(0.002) | -0.031***<br>(0.002) |
| Area                | 0.001***<br>(0.000)  | 0.001***<br>(0.000)  | 0.001***<br>(0.000)  | 0.001***<br>(0.000)  |
| Constant            | 18.675***<br>(0.055) | 18.669***<br>(0.055) | 18.665***<br>(0.056) | 18.663***<br>(0.056) |
| Sub-district FE     | Yes                  | Yes                  | Yes                  | Yes                  |
| Month FE            | Yes                  | Yes                  | Yes                  | Yes                  |
| Observations        | 603619               | 603619               | 603619               | 603619               |
| R-squared           | 0.495                | 0.495                | 0.495                | 0.495                |

**Source:** Research findings.

**Note:** Standard errors in parentheses, \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . SE are clustered at sub-district level. All specifications include age squared, surface-area squared, and building-frame controls.

### Results for the Second and Third Gasoline-Price Shocks

Table 5 reports shock-specific estimates for the second and third gasoline-price shocks. For the second-shock analysis, observations from month 38 and earlier and from month 116 onward are excluded. This restriction focuses the analysis on the period surrounding the second shock and excludes the third shock. However, because the lagged post-shock period associated with the first shock overlaps with part of the nominal pre-period for the second shock, these estimates should be interpreted as complementary sensitivity analyses rather than as fully independent causal estimates of the second shock.

For the second gasoline-price shock, the estimated DID coefficient is negative and statistically significant under sub-district-clustered inference for all four lag specifications. Its magnitude ranges from approximately  $-2.5\%$  to  $-3.1\%$ , with no clear monotonic pattern across adjustment lags. The three-, six-, and twelve-month estimates are statistically significant at the 5% level, whereas the nine-month estimate is significant at the 10% level. . These estimates are larger in magnitude than the pooled baseline estimates, but their interpretation is limited by

the overlap between the earlier shock's adjustment period and part of the available pre-period.

For the third gasoline-price shock, observations from month 63 and earlier are excluded to focus on the later part of the sample. Because the shock occurred in Aban 1398 ( $T=116$ ) and the sample ends at  $T=120$ , only the three-month adjustment specification can be estimated; its post-shock period consists of months 119 and 120. The estimated DID coefficient is approximately  $-2.9\%$  and statistically significant under sub-district-clustered inference. Given the very short post-shock period, this result should be interpreted cautiously and as supplementary evidence rather than a precise standalone estimate.

## **Robustness Checks**

### **Pseudo Panel Fixed-Effects Estimates**

As a robustness check, we construct a pseudo-panel at the sub-district-month level and estimate a fixed-effects specification. This exercise does not correct a deficiency of the transaction-level DID model; rather, it assesses whether the main findings remain similar after aggregating transaction outcomes and dwelling characteristics within geographic units over time.

For each sub-district-month cell, we calculate the mean logarithm of real housing prices, mean building age, and mean unit surface area. The resulting pseudo-panel contains 7,494 observed sub-district-month cells across 63 sub-districts and 119 usable transaction months. The panel is slightly unbalanced: most sub-districts are observed in all 119 usable months, while two have a small number of missing monthly cells. The fixed-effects specification includes sub-district fixed effects and month fixed effects, and standard errors are clustered at the sub-district level.

Table 6 reports the pseudo-panel fixed-effects estimates for the alternative lag specifications. The DID coefficients are negative across all lags, ranging from approximately  $-2.5\%$  to  $-2.9\%$ , and are statistically significant under sub-district-clustered inference. For example, the three-month estimate is  $-0.0246$  ( $SE=0.0121$ ,  $p=0.047$ ). Although aggregation changes the unit of analysis and discards within-sub-district-month variation, the similarity of these estimates to the transaction-level results provides supportive robustness evidence.

**Table 5.** Transaction-Level DID Estimates for the Second and Third Gasoline-Price Shocks

|                      | Second Shock         |                      |                      |                      | Third Shock          |
|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
|                      | Three-Month          | Six-Month            | Nine-Month           | Twelve-Month         | Three-Month          |
| Treatment=1          | 1.499***<br>(0.026)  | 1.499***<br>(0.026)  | 1.499***<br>(0.026)  | 1.501***<br>(0.026)  | 1.285***<br>(0.015)  |
| Post=1               | -0.273***<br>(0.017) | -0.274***<br>(0.018) | -0.278***<br>(0.018) | -0.276***<br>(0.017) | -0.697***<br>(0.013) |
| Treatment=1 * Post=1 | -0.031**<br>(0.012)  | -0.030**<br>(0.014)  | -0.025*<br>(0.015)   | -0.027**<br>(0.012)  | -0.029***<br>(0.011) |
| Age                  | -0.014***<br>(0.001) | -0.014***<br>(0.001) | -0.014***<br>(0.001) | -0.014***<br>(0.001) | -0.013***<br>(0.001) |
| Area                 | 0.006***<br>(0.000)  | 0.006***<br>(0.000)  | 0.006***<br>(0.000)  | 0.006***<br>(0.000)  | 0.010***<br>(0.000)  |
| Constant             | 19.101***<br>(0.070) | 19.101***<br>(0.070) | 19.101***<br>(0.070) | 19.100***<br>(0.070) | 18.871***<br>(0.079) |
| Sub-district FE      | Yes                  | Yes                  | Yes                  | Yes                  | Yes                  |
| Month FE             | Yes                  | Yes                  | Yes                  | Yes                  | Yes                  |
| Observations         | 891712               | 891712               | 891712               | 891712               | 642907               |
| R-squared            | 0.621                | 0.621                | 0.621                | 0.621                | 0.743                |

**Source:** Research findings.

**Note:** Standard errors in parentheses, \*  $p < 0.10$ , \*\*  $p < 0.05$ , and \*\*\*  $p < 0.01$ . SE are clustered at sub-district level. All specifications include age squared, surface-area squared, and building-frame controls.

**Table 6.** Pseudo Panel Fixed-Effects Estimates under Alternative Adjustment Lags

|                 | Three-Month          | Six-Month            | Nine-Month           | Twelve-Month         |
|-----------------|----------------------|----------------------|----------------------|----------------------|
| Treatment*Post  | -0.025**<br>(0.012)  | -0.029**<br>(0.011)  | -0.028***<br>(0.010) | -0.029***<br>(0.009) |
| (mean) Age      | -0.008**<br>(0.003)  | -0.008**<br>(0.003)  | -0.008**<br>(0.003)  | -0.008**<br>(0.003)  |
| (mean) Aream    | 0.004***<br>(0.001)  | 0.004***<br>(0.001)  | 0.004***<br>(0.001)  | 0.004***<br>(0.001)  |
| Constant        | 26.987***<br>(3.503) | 26.915***<br>(3.480) | 26.918***<br>(3.483) | 26.921***<br>(3.483) |
| Month FE        | Yes                  | Yes                  | Yes                  | Yes                  |
| Sub-district FE | Yes                  | Yes                  | Yes                  | Yes                  |
| Observations    | 7494                 | 7494                 | 7494                 | 7494                 |
| R-squared       | 0.856                | 0.856                | 0.856                | 0.856                |

**Source:** Research findings.

**Note:** Standard errors in parentheses, \*  $p < 0.10$ , \*\*  $p < 0.05$ , and \*\*\*  $p < 0.01$ . SE are clustered at sub-district level. All specifications include age squared, surface-area squared, and building-frame controls.

## Conclusion

This study examines whether increases in real gasoline prices affect the spatial distribution of housing prices within Tehran by raising commuting costs more strongly for peripheral than for relatively employment-accessible central areas. Using approximately 1.4 million residential transaction records from Farvardin 1389 (March 2010) through Esfand 1398 (March 2020), the analysis estimates a transaction-level hedonic difference-in-differences model with sub-district and month fixed effects. The study period covers 120 calendar months, of which 119 are usable after the exclusion of October 2016 because postal-code information is unavailable. Standard errors are clustered at the sub-district level.

The preferred specification pools the first two gasoline-price shocks and allows for a six-month adjustment lag. It indicates that real housing prices in peripheral areas were approximately 2.1% lower relative to central areas during the defined post-shock months ( $\beta = -0.0208$ ,  $SE = 0.0121$ ,  $p = 0.091$ ). The estimates are negative across all alternative lag structures, ranging from approximately -1.5% to -2.1%. The six- and twelve-month estimates are statistically significant at the 10% level, whereas the three- and nine-month estimates are not statistically significant at conventional levels. A formal pre-trend test for the period before the

first gasoline-price shock fails to reject the null hypothesis of no differential trend between peripheral and central areas ( $p\text{-value}=0.699$ ). This result is consistent with the parallel-trends assumption, although it cannot establish that assumption conclusively.

The pseudo-panel fixed-effects estimates provide supportive robustness evidence. After aggregating transactions to the sub-district-month level, the estimated DID coefficients remain negative across the alternative lag specifications, ranging from approximately  $-2.5\%$  to  $-2.9\%$ , and are statistically significant at 5% using sub-district-clustered standard errors. The similarity between the transaction-level and pseudo-panel estimates suggests that the negative relationship is not solely driven by the particular transaction-level specification or by changes in the composition of sold housing units within sub-districts over time.

The shock-specific results should be interpreted more cautiously. The estimates for the first gasoline-price shock are statistically imprecise under clustered inference, although they become negative at longer adjustment lags. The substantial compensatory cash-transfer program introduced alongside the 2010 subsidy reform may have partly cushioned the increase in household transport expenditures and attenuated the immediate capitalization of commuting costs into peripheral housing prices. The estimates for the second and third shocks are negative and larger in magnitude, but their interpretation is limited by the short and partly contaminated pre-shock periods available after earlier shocks. In particular, the third-shock estimate relies on only two observed post-shock months and should be regarded as supplementary evidence.

Overall, the findings are consistent with the proposition that higher real gasoline prices may reduce the relative attractiveness of peripheral residential locations by increasing commuting costs. The estimated effects are economically modest relative to the substantial aggregate fluctuations typically observed in Tehran's housing market. The results should therefore be interpreted as suggestive evidence of a relative spatial adjustment in housing prices, rather than as evidence of a large or immediate response.

More broadly, these findings contribute to the urban economics literature on transportation costs, residential sorting, and housing-price gradients. Previous studies of North American and European metropolitan areas have shown that changes in commuting costs can influence residential-location choices and alter the spatial distribution of housing demand. The evidence from Tehran suggests that similar mechanisms may operate in large developing-country cities characterized

by automobile dependence, regulated fuel prices, and persistent housing-market constraints. Although the estimated effects are modest and should be interpreted cautiously, they are consistent with the possibility that transportation-cost shocks affect the relative spatial distribution of housing prices even in highly regulated urban environments.

If future increases in gasoline prices become unavoidable, policymakers should consider these spatial consequences alongside the direct effects of fuel-price reform on household expenditure. In cities where employment opportunities, public transport access, and housing are unevenly distributed, higher private-vehicle commuting costs may place disproportionate pressure on peripheral residents and peripheral housing markets. Fuel-price reforms should therefore be accompanied by reliable and affordable public transport and, where necessary, compensatory support for households facing high commuting exposure.

The present analysis also has important data limitations. The transaction records do not contain information on household travel behavior, employment locations, residential relocation, rental prices, or the receipt and use of compensatory transfers. They also lack several housing characteristics that may affect prices, including the number of bedrooms and bathrooms, parking spaces, and floor level. Although sub-district and month fixed effects, observable dwelling characteristics, and clustered standard errors reduce concerns about several sources of confounding and dependence, unobserved factors may still affect central and peripheral housing markets differently.

Future research could strengthen the analysis by combining transaction data with household travel surveys, employment-location measures, public-transport fares, rental-market outcomes, and more detailed housing characteristics. In particular, constructing a broader commuting-cost index that incorporates gasoline prices, taxi fares, bus fares, metro fares, and travel time would allow researchers to examine more directly how changes in the full cost of commuting affect residential location and housing-market outcomes.

## **Appendix**

The raw dataset includes transactions for non-residential units. After excluding these non-residential transactions, 1,414 observations in Tehran city were removed. The variable representing the surface area of residential units displayed several outlier observations. These outliers may correspond to transactions involving multiple residential units recorded under a single contract. To address this issue, we eliminated the top and bottom 0.5% of observations based on the surface area

variable. The total number of observations removed for the city of Tehran was 14,374.

Tehran consists of 22 districts and over 400 neighborhoods. For this study, we required a variable to control for the fixed effects of the geographical location of residential units. Each district within Tehran Municipality varies significantly in size and population, leading to varying access to urban amenities. Given the high number of neighborhoods, a new classification system was needed to subdivide Tehran. So, each of Tehran's 22 municipal districts was divided into several sub-districts.

In the study by Yazdanifard et al. (2021), this subdivision was achieved using the first three digits of postal codes. However, this method has its drawbacks: postal code divisions do not always align with municipal district boundaries, resulting in cases where two residential units sharing similar postal codes are located at significant distances from one another. To overcome this limitation, we adapted the methodology of Ahmadi and Jahangard (2020), dividing Tehran's 22 districts into 63 sub-districts.

This research classified 333 neighborhoods in Tehran using 53 indicators measuring quality of life and access levels, organized into five clusters: "Highly Affluent", "Affluent", "Average", "Less Affluent", and "Needing Intervention". The sub-districts created for this study were designed to respect the boundaries of the 22 municipal districts while ensuring that each sub-district contained neighborhoods of similar socio-economic status. As a result, no sub-district includes neighborhoods that are both highly affluent and in need of Intervention. Instead, each sub-district is composed of neighborhoods within the same cluster or from adjacent clusters, such as affluent and average or less affluent and needing intervention.

We then used maps from Iran Post Company along with the reported postal codes in our dataset to determine the sub-district of each transacted residential unit. As noted in Section 4, the raw data from the Ministry of Roads and Urban Development did not report the postal codes for residential units transacted in October 2016. Consequently, we excluded observations from this month, removing a total of 11,678 entries from our dataset.

**Table A1.** Timing of Gasoline-Price Shocks and Definitions of Lag-Specific Post-Shock Periods

| Shock                              | Iranian month    | Month index | Three-month lag | Six-month lag | Nine-month lag | Twelve-month lag |
|------------------------------------|------------------|-------------|-----------------|---------------|----------------|------------------|
| <b>First gasoline-price shock</b>  | Dey 1389         | 10          | T=13-41         | T=16-44       | T=19-47        | T=22-50          |
| <b>Second gasoline-price shock</b> | Ordibehesht 1393 | 50          | T=53-66         | T=56-69       | T=59-72        | T=62-75          |
| <b>Third gasoline-price shock</b>  | Aban 1398        | 116         | T=119-120       | -             | -              | -                |

**Source:** Research findings.

**Note:** T=1 denotes Farvardin 1389 (March 2010). The sample ends at T=120; therefore, only the three-month lag can be examined for the third shock. October 2016 is excluded because postal-code information is unavailable.

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