

A Fuel Reform During a Moving Land-Price Gradient

Job access and registered property transactions in Kuwait

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Abstract

Kuwait raised regulated gasoline prices on 1 September 2016. We ask how the job-access gradient of registered property transaction prices moved around the reform. Fixed exposure is constructed from 2011 workplace employment and linked to 23,033 recorded-land and completed-home transfers from 2013Q3 through 2019Q3. The outcome is nominal transfer price per recorded plot square meter among properties that sell; the land category is administrative and does not verify vacancy or developability. In the transaction-weighted recorded-land sample, the lower-access gradient was already increasing by 0.0215 log points per quarter. Relative to its fitted linear pretrend, the first-full-quarter break is -0.0792 log points per standard deviation of lower access, and the fitted post-reform slope is -0.0010. This path is not a broad-market result. Sabah Al-Ahmad Al-Marine is 4.02 exposure standard deviations into the lower-access tail and supplies 39.3 percent of effective land weight. Omitting it reverses the estimated land break and post slope; equal neighborhood-quarter weights largely flatten the result. A comparison restricted to the 36 neighborhoods observed in both markets also changes sharply when that neighborhood is removed, so the data do not support a stable land-versus-home divergence. Alternative access measures, spatial standard errors, event windows, and a rolling pseudo-break scan do not repair this concentration problem. The reform remains a useful date around which to document an unusual spatial transaction cycle, but these data cannot distinguish gasoline-cost capitalization from neighborhood-specific development, sale composition, legal uncertainty, or other concurrent changes.

1. Introduction

When driving becomes more expensive, a location's access to work should matter more. A household farther from employment either pays a larger commuting bill or adjusts travel, fuel grade, vehicle, workplace, or residence. If buyers expect the change to persist, some of the adjustment can enter bids for land and housing. The mechanism is simple. The empirical object is not.

Kuwait changed its national gasoline-price schedule on 1 September 2016. Because every neighborhood received the same reform, identifying variation must come from spatial differences in exposure to driving costs. We use access to the pre-reform distribution of jobs as that exposure. Lower job access is

economically relevant, but it is also part of Kuwait's central-peripheral urban structure. It can therefore capture the commuting-cost mechanism and unrelated local development cycles at the same time.

The observed transaction path makes that concern concrete. In the years before the reform, registered land in lower-access neighborhoods was already gaining relative to land in higher-access neighborhoods. A transaction-weighted interrupted-gradient model says that this movement stopped after the reform. The fitted pre-reform differential slope is 0.0215 log points per quarter; the slope change is -0.0225; and the fitted post-reform slope is therefore -0.0010, with a 95 percent cluster- t interval from -0.0143 to 0.0122. Read literally, the outward land-price gradient had been steepening and then became approximately flat.

That average is carried by one unusual place. Sabah Al-Ahmad Al-Marine sits 4.0195 standard deviations into the lower-access tail, while the next-lowest-access land neighborhood is only 0.4583 standard deviations below the mean. It supplies 39.3 percent of effective land weight. Omitting it changes the pre-reform slope from 0.0215 to -0.0413, the first-full-quarter break from -0.0792 to 0.4799, and the fitted post slope from -0.0010 to 0.0619. Giving each neighborhood-quarter equal weight instead yields a land pre-slope of 0.0114, a break of -0.0114, and a slope change of -0.0024. These are different descriptions of the data, not minor standard-error choices.

The paper therefore asks a narrower question: what exactly is behind the apparent change in Kuwait's job-access price gradient around the reform? The answer is useful because it separates three facts that would otherwise be compressed into one coefficient. First, the transaction-weighted land gradient was moving before September 2016. Second, its later path is strongly associated with a very low-access, high-volume neighborhood. Third, the available administrative records cannot distinguish a gasoline-cost channel from place-specific development and transaction composition.

We construct a fixed job-access measure from 715,968 workplace workers across 677 positive-employment areas in Kuwait's 2011 census. We evaluate access at 95 reviewed neighborhood centroids and standardize minus log access, so a larger value means less access to the pre-reform job distribution. We link this surface to 23,033 Ministry of Justice records for two administrative market categories between 2013Q3 and 2019Q3. For recorded land, the dependent variable is the log nominal transfer price per recorded plot square meter. For completed homes, it is also the log nominal transfer price per recorded plot square meter--not price per unit of floor area. The transaction extract does not establish that a recorded-land parcel was vacant, developable, or otherwise comparable to a vacant-land concept in another country.

The analysis proceeds from the observed path to the leverage audit. We first estimate quarterly changes in the lower-access price gradient and an interrupted linear trend around 2016Q4, the first full post-reform quarter. We then report leave-one-neighborhood-out estimates, equal neighborhood-quarter

weights, and a shared-support comparison using the 36 neighborhoods observed in both administrative markets. Finally, we vary the access surface, inference method, event window, transition-quarter convention, trend order, and candidate break date.

The common-36 comparison is the appropriate starting point for discussing the two markets because it holds neighborhood support fixed. With transaction weights, land has a break of -0.0793 and a slope change of -0.0224, while completed homes have a break of -0.0954 and a slope change of 0.0200. A stacked model estimates a home-minus-land slope-change difference of 0.0423 ($p = 0.0002$). But after removing Sabah Al-Ahmad Al-Marine, that difference is -0.0418 ($p = 0.610$), and a joint equality test has $p = 0.789$. The sample therefore does not support a stable claim that land and completed homes reacted differently.

This paper sits between urban economics and measurement work. Studies using locally varying gasoline prices find shifts toward locations with cheaper commutes, and other work shows that construction and housing-supply elasticity affect how transport costs enter property markets (Molloy and Shan, 2013; Blake, 2019; Morris et al., 2020). Kuwait adds a nationally administered reform in an oil-exporting, car-oriented setting, where energy-price changes also have fiscal, income, and general-equilibrium channels (Shehabi, 2017; Larson and Zhao, 2020). The contribution here is not a new causal estimate. It is a transparent account of what the available transaction records reveal, which neighborhood generates the apparent spatial break, and what additional data would be required to interpret that break.

2. Policy setting and mechanism

2.1 The September 2016 price regime

The new schedule took effect on 1 September 2016. It set 91-octane gasoline at 85 fils per liter, 95-octane gasoline at 105 fils, and ultra-premium gasoline at 165 fils (Kuwait News Agency, 2016b; Kuwait News Agency, 2016a). The previous prices for the first two grades were 60 and 65 fils per liter, so the nominal increases were 25 and 40 fils. The IMF described the change as an average increase of about 70 percent across grades (International Monetary Fund, 2017). These sources establish the policy date and schedule, not a property-market effect.

The post indicator marks this price regime; it is not a quarterly gasoline-price series. The empirical model does not observe grade-specific purchases, household fuel expenditure, expected future prices, or a real fuel-price path. This matters because motorists could switch grades instead of reducing driving. KUNA's one-year account reports that total motor-fuel sales declined by only 0.8 percent, while sales

shifted sharply from 95- to 91-octane fuel (Sultan and Al-Mejren, 2017). Those national before-and-after figures are descriptive and do not validate a neighborhood-level treatment intensity.

The timing also contains legal uncertainty. Contemporary reporting states that a lower court ruled the increase unlawful on 28 September 2016 but declined to suspend it; the Court of Appeals reversed that ruling in April 2017, after which the challengers said they would seek further review (Kuwait Times, 2017). We have not obtained the formal judgments and therefore do not characterize their precise legal holdings beyond that reporting. Economically, the point is that 2016Q4 was the first full quarter of the new schedule and also a period in which its legal durability could reasonably have been uncertain.

2.2 The commuting-cost channel is modest in levels

The mechanism begins with generalized commuting cost. If a household makes more or longer vehicle trips from a lower-access location, a higher fuel price raises the cost of occupying that location. In a spatial-equilibrium model, land and property prices help clear jointly determined housing, employment, and travel markets (Ahlfeldt et al., 2015). A persistent fuel-price increase can rotate the urban price gradient, but it can also change vehicle choice, construction, sorting, and workplace location.

Simple arithmetic helps place the reform in scale. Suppose a vehicle travels 10 kilometers per liter, makes 22 round-trip work journeys per month, and a household is an extra 10 kilometers from work in each direction. The monthly increase is about KD 1.10 on 91-octane fuel and KD 1.76 on 95-octane fuel. At 20 extra one-way kilometers, the figures are KD 2.20 and KD 3.52; at 30 kilometers, KD 3.30 and KD 5.28. This is an illustration, not an estimate of Kuwait's average commute. It shows why a large percentage increase from a low regulated base need not create a large cash shock for every household.

Several channels can therefore compete with direct capitalization. Drivers may switch grade, change discretionary travel, alter vehicle fuel economy, or absorb the expense (Busse et al., 2013; Li et al., 2014). A national reform can also affect government finances, expectations, wages, construction costs, and aggregate demand. In an oil-exporting economy, a change in energy prices need not resemble a locally varying retail-price shock in an importing city (Larson and Zhao, 2020; Shehabi, 2017). Our exposure isolates neither the household budget channel nor these broader channels.

3. Data and measurement

3.1 Registered property transfers

The transaction outcome comes from an internal research extract of Kuwait Ministry of Justice real-estate records (Kuwait Ministry of Justice, 2026). We retain two recorded property-status categories and estimate them separately. Exact duplicate records receive inverse-duplicate weights, so each transaction fingerprint contributes total weight one.

For recorded land, the outcome is the log nominal registered-land-status transfer price divided by recorded plot area in square meters. “Recorded land” is an administrative label in the source. We have not verified vacancy, servicing, development rights, intended use, or whether structures were present. For completed homes, the outcome is the log nominal completed-home-status transfer price divided by recorded plot area in square meters. The denominator is not internal floor area. Consequently, neither outcome is a conventional repeat-sales house-price index, and their levels should not be compared as if they priced the same physical quantity.

Transactions are linked to reviewed PACI neighborhoods through a hand-audited MOJ-to-PACI crosswalk. The canonical neighborhood is the exposure and clustering unit. MOJ block labels enter a sensitivity specification but are not treated as independently geocoded treatment locations. PACI 2023 boundaries are used to locate the neighborhoods, which imposes a current-boundary assumption on a 2013-2019 analysis.

The event window covers 2013Q3 through 2019Q3, twelve quarters on either side of 2016Q3. There are 24,650 otherwise eligible recorded-land and completed-home rows. The reviewed geography bridge retains 23,205. We then require a neighborhood-market cell to have at least five inverse-duplicate effective sales before and after the reform, excluding 2016Q3. The final sample contains 23,033 rows and 20,054 effective observations.

Table 1. Construction of the registered-transaction sample

Sample stage	Recorded land	Completed homes	Total
Eligible rows in event window	--	--	24,650
Rows after reviewed-neighborhood join	--	--	23,205
Rows before market-specific support rule	12,319	10,886	23,205
Rows after support rule	12,177	10,856	23,033
Effective observations after inverse-duplicate weights	9,310	10,744	20,054

Note: The support rule requires five effective sales in both periods and therefore selects neighborhoods using post-reform transaction activity. The supported samples contain 38 recorded-land and 74 completed-home neighborhood clusters. Main regressions exclude the transition quarter, leaving 11,987 recorded-land rows and 10,549 completed-home rows. These are aggregate sample counts; no transaction rows, deed identifiers, or exact transaction locations are disclosed. Source: Kuwait Ministry of Justice; author's calculations.

The post-period support condition is a first-order limitation. The estimand conditions on neighborhoods that transact on both sides of the event and on properties that sell. If the reform or another shock changes sale incidence, the selected sample can move even when underlying property values do not. Area controls and block-label fixed effects do not recover zero-sale neighborhood-quarters, unobserved structure quality, seller urgency, or changing land readiness.

3.2 Fixed job access

We construct access from official CSB-2011 area geography and workplace employment (Kuwait Central Statistical Bureau, 2011):

$$\text{Access}_n(\lambda) = \sum_j \text{Jobs}_{j,2011} \exp(-d_{nj} / \lambda).$$

Here, $\text{Jobs}_{j,2011}$ is workplace employment in census area j , d_{nj} is straight-line distance in kilometers from neighborhood centroid n to area centroid j , and the main decay is $\lambda = 10$ kilometers. We standardize minus log access across the 95 reviewed neighborhoods:

$$\text{LowAccess}_n = -[\log \text{Access}_n(10) - \text{mean}(\log \text{Access}(10))] / \text{sd}[\log \text{Access}(10)].$$

One unit is one cross-neighborhood standard deviation of lower predicted access. The surface contains 715,968 workplace workers across 677 positive-employment census areas. Five- and twenty-kilometer decays test whether the ordering depends on the main decay. Straight-line distance is used because the available current road network is post-reform; using it would introduce a less transparent vintage problem.

The main workplace surface includes workers of all nationalities. Because the ownership and buyer composition of the two property categories may be more closely related to Kuwaiti employment, we rebuild access from 92,534 Kuwaiti workplace workers. Across the 95 neighborhoods, its correlation with the all-worker surface is 0.9986 in levels and 0.9870 in ranks. The land break changes from -0.0792 to -0.0768, and the slope change from -0.0225 to -0.0220. This validates the spatial ranking against one nationality restriction. It does not reveal buyer nationality, ownership eligibility, or actual commutes.

3.3 The estimand

Let $S_{int m} = 1$ if property i , in neighborhood n , quarter t , and administrative market m , is observed selling. The empirical object is

$$E[\log(P_{int m} / A_{in}) \mid S_{int m} = 1, \text{LowAccess}_n, t, m].$$

Here, P is nominal registered transfer price and A is recorded plot area. The estimand is a conditional mean among observed transfers in supported neighborhoods. It is not the value of the full property stock, the national level effect of the reform, or a household treatment effect.

4. Empirical approach

4.1 Interrupted spatial gradient

For each administrative market, the baseline model is

$$\log(P_{int} / A_i) = \alpha_n + \delta_t + \theta \log A_i + \beta_1(\text{LowAccess}_n \times \tau_t) + \beta_2(\text{LowAccess}_n \times \text{Post}_t) + \beta_3(\text{LowAccess}_n \times \text{PostTime}_t) + \epsilon_{int}.$$

Neighborhood fixed effects absorb time-invariant location attributes, and quarter fixed effects absorb common market shocks. Time is centered at 2016Q2. The main model excludes 2016Q3, sets $\text{Post}_t = 1$ from 2016Q4 onward, and centers post time at 2016Q4. The weights are inverse exact-duplicate frequency, and standard errors are clustered by neighborhood with a cluster- t reference distribution.

The parameters describe four quantities. β_1 is the pre-reform change per quarter in the conditional price gradient associated with one standard deviation lower job access. β_2 is the 2016Q4 deviation from the fitted linear pretrend. β_3 is the change in slope after the reform. The absolute fitted post slope is $\beta_1 + \beta_3$. Reporting the sum matters: a negative slope change can mean that a previously rising gradient becomes flat, not that it begins falling.

A causal reading would require the fitted pretrend to approximate the no-reform counterfactual and no concurrent neighborhood-specific shocks correlated with access. It would also require the composition of observed sales not to respond differentially. Those conditions are not established here. The model is an organized description of a path around a policy date.

4.2 Leverage, weights, and shared support

Three analyses are primary for judging what the baseline summarizes.

First, we report the distribution of exposure and effective transaction weight by neighborhood. An exposure tail is especially consequential when it coincides with a large share of regression weight. Second, we re-estimate all interrupted-gradient parameters after omitting each neighborhood in turn and compare transaction weighting with equal neighborhood-quarter weighting. Third, we restrict both administrative markets to the same 36 neighborhoods and estimate a stacked model with market interactions. This makes the land-versus-home comparison geographically coherent, but it does not make the resulting difference causal.

The stacked model tests the home-minus-land differences in pre-slope, first-post break, and slope change. We report those tests both with and without Sabah Al-Ahmad Al-Marine because it is simultaneously the lowest-access observation and the largest land-weight observation. This is an influence analysis, not an invitation to discard an inconvenient neighborhood. Its purpose is to state whether the target result is a broad spatial relationship or a case-driven one.

4.3 Sensitivity exercises

The remaining exercises vary one part of the description at a time: 5-, 10-, and 20-kilometer access decays; area controls; block-label fixed effects; governorate-specific linear trends; inclusion of 2016Q3 as the first post quarter; quadratic interrupted trends; and windows from four to twelve quarters on each side. A neighborhood-score spatial covariance calculation uses Bartlett kernels at 10, 20, and 30 kilometers. These covariance estimates address residual dependence; they cannot repair exposure support or influence.

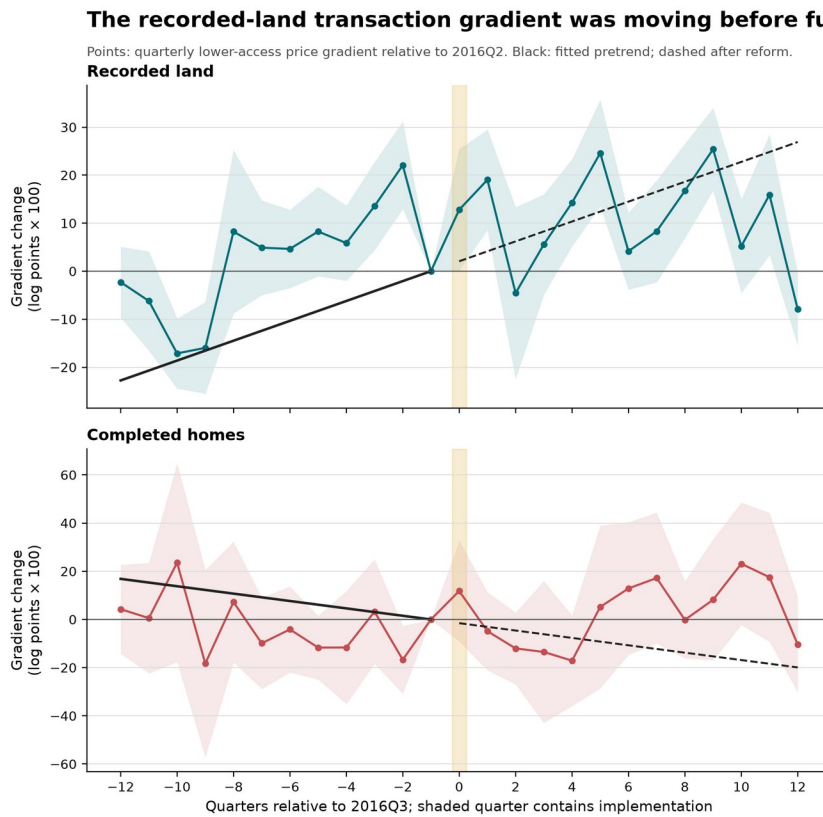
We also run a rolling pseudo-break scan. For each candidate from eight quarters before to eight quarters after 2016Q4, the model uses four quarters on each side and excludes the candidate quarter. Only candidates from 2014Q4 through 2015Q3 have windows entirely before the policy transition; later windows overlap the reform period. Adjacent estimates share most of their observations. The scan is therefore neither a placebo design nor randomization inference. It asks whether a local fitted break at 2016Q4 looks distinctive relative to mechanically similar nearby breaks.

5. Results

5.1 The transaction-weighted path

Figure 1 plots quarter-specific changes in the lower-access transaction-price gradient relative to 2016Q2. Recorded land was moving outward before the reform: lower-access transactions were becoming more expensive relative to higher-access transactions. The long linear fit projects that movement forward. The

post-2016 points no longer track that projection. Completed-home estimates are noisier and do not provide a clean parallel benchmark.



Neighborhood and quarter fixed effects, log-area control, inverse-duplicate weights, neighborhood-clustered 95% t intervals. Controlled descriptive evidence; not causal. Source: Kuwait Ministry of Justice; author's calculations.

Figure 1. Quarterly registered-transaction price gradients and fitted pretrends. Points are log nominal transfer price per recorded plot square meter per standard deviation of lower job access, relative to 2016Q2. Lines extrapolate fitted linear pretrends and are descriptive benchmarks, not identified no-reform counterfactuals.

Table 2 gives the corresponding interrupted-gradient estimates on each market's full supported sample. For recorded land, the pre-slope is 0.0215 log points per quarter. The fitted 2016Q4 break is -0.0792, with a 95 percent interval from -0.1759 to 0.0176. The slope change is -0.0225, and the absolute fitted post slope is -0.0010 [-0.0143, 0.0122]. For completed homes, the pre-slope is -0.0103, the break is 0.0240, the slope change is 0.0317, and the absolute post slope is 0.0214 [0.0114, 0.0315]. The samples cover different neighborhoods, so the last two rows should not be treated as a market contrast.

Table 2. Interrupted lower-access gradients on each market's supported sample

Administrative market	Pre-slope per quarter	First-full-quarter break	Post-slope change	Absolute post slope
Recorded land	0.0215 [0.0155, 0.0275]	-0.0792 [-0.1759, 0.0176]	-0.0225 [-0.0378, -0.0073]	-0.0010 [-0.0143, 0.0122]

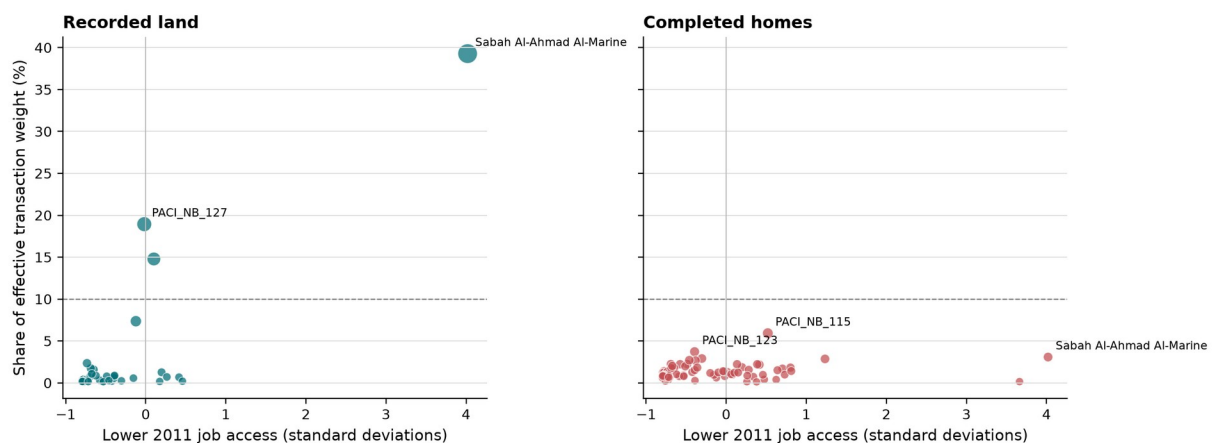
Administrative market	Pre-slope per quarter	First-full-quarter break	Post-slope change	Absolute post slope
Completed homes	-0.0103 [-0.0368, 0.0162]	0.0240 [-0.2405, 0.2886]	0.0317 [0.0058, 0.0577]	0.0214 [0.0114, 0.0315]

Note: Entries are log points per one standard deviation of lower access; brackets contain 95 percent neighborhood-cluster- t intervals. Outcomes are nominal transfer price per recorded plot square meter among observed sales. The land category does not verify vacancy or developability. Models include neighborhood and quarter fixed effects and log recorded plot area, exclude 2016Q3, and use inverse-duplicate transaction weights.

5.2 Sabah Al-Ahmad Al-Marine is the identifying tail

Figure 2 joins exposure and weight. Sabah Al-Ahmad Al-Marine is at 4.0195 standard deviations of lower access, far beyond the rest of the recorded-land support. It contributes 3,597 effective observations, or 39.3 percent of the land regression weight. Its median recorded plot area is 500 square meters, and one MOJ block label accounts for 96.5 percent of its effective weight. The three largest land neighborhoods together supply 73.0 percent of effective weight; the weight Herfindahl index implies only 4.56 equally weighted neighborhoods.

One neighborhood is isolated in both exposure and land-market weight



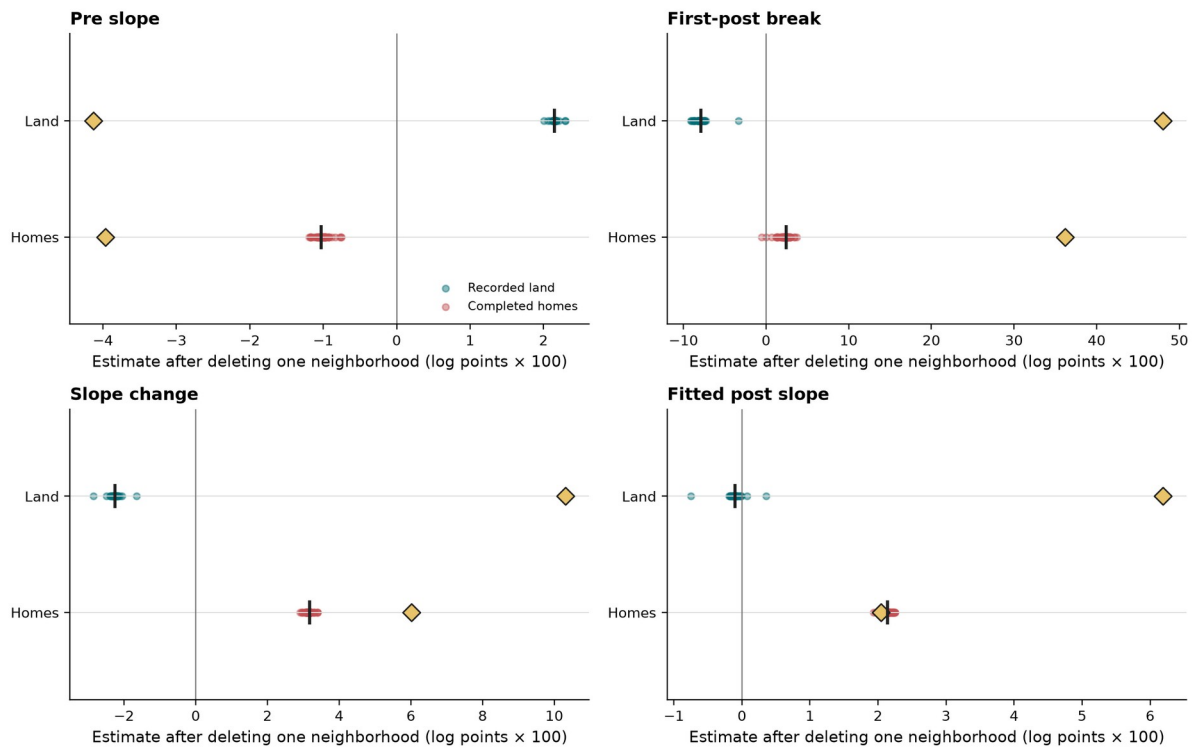
Main ± 12 -quarter estimation sample; 2016Q3 excluded. Dashed line marks 10% of effective transaction weight. Source: Kuwait Ministry of Justice; author's calculations.

Figure 2. Exposure and effective transaction weight in the recorded-land sample. Sabah Al-Ahmad Al-Marine is both the isolated lower-access tail and the largest source of land weight. Outcome and category limitations follow Table 2.

The leave-one-out exercise shows how much that geometry matters. Removing Sabah Al-Ahmad Al-Marine changes the land pre-slope from 0.0215 to -0.0413, the break from -0.0792 to 0.4799, the slope

change from -0.0225 to 0.1031, and the absolute post slope from -0.0010 to 0.0619. Every parameter changes sign. No other land omission produces a comparable shift.

The transaction-weighted land coefficients depend on Sabah Al-Ahmad Al-Marine



Each point omits one neighborhood; black ticks are full-sample estimates and gold diamonds omit PACI_NB_146. Controlled descriptive influence audit. Source: Kuwait Ministry of Justice; author's calculations.

Figure 3. Leave-one-neighborhood-out interrupted-gradient estimates. Each point re-estimates the full transaction-weighted model after removing one neighborhood. The Sabah Al-Ahmad Al-Marine omission changes the sign of every recorded-land parameter.

Equal neighborhood-quarter weights supply a different diagnostic. They reduce the contribution of high-volume places without deleting them. Under those weights, the land pre-slope is 0.0114, the break is -0.0114, the slope change is -0.0024, and the absolute post slope is 0.0090; all intervals include zero. This does not prove that equal-cell weighting is the correct estimand. It establishes that the transaction-weighted result describes the price path of transactions, heavily concentrated in one place, rather than a typical-neighborhood path.

Sabah Al-Ahmad Al-Marine should not be treated as a nuisance observation. It may be precisely where an outward development cycle is economically important. But interpreting its path requires information not present in the transaction extract: subdivision and infrastructure timing, waterfront or amenity development, plot releases, tenure and development rights, and the composition of buyers and sellers. Without those data, "the fuel reform changed Kuwait's land gradient" is too broad. The defensible

statement is that a reform coincided with a sharp change in a transaction-weighted gradient whose effective lower-access comparison is largely Sabah Al-Ahmad Al-Marine versus the rest.

5.3 Comparing markets on the same 36 neighborhoods

Restricting both categories to 36 shared neighborhoods resolves the most immediate support mismatch. With transaction weights, recorded land has a pre-slope of 0.0215, a break of -0.0793, a slope change of -0.0224, and an absolute post slope of -0.0009. Completed homes have a pre-slope of 0.0064, a break of -0.0954, a slope change of 0.0200, and an absolute post slope of 0.0264.

Table 3. Interrupted gradients on the 36-neighborhood shared support

Weighting and sample	Market	Pre-slope	First-full-quarter break	Slope change	Absolute post slope
Transaction weighted, all 36	Recorded land	0.0215	-0.0793	-0.0224	-0.0009
Transaction weighted, all 36	Completed homes	0.0064	-0.0954	0.0200	0.0264
Equal neighborhood-quarter, all 36	Recorded land	0.0127	0.0033	-0.0068	0.0059
Equal neighborhood-quarter, all 36	Completed homes	-0.0074	0.0283	0.0378	0.0304

Note: Entries are log points per one standard deviation of lower access. Both administrative categories use the same neighborhoods, but transaction counts and recorded properties still differ. Full cluster- t intervals and stacked covariance tests are in the replication tables. The administrative categories and per-recorded-plot-area outcomes are defined in Section 3.1.

In the stacked transaction-weighted model, the home-minus-land slope-change difference is 0.0423 [0.0215, 0.0631], and the joint equality test for the pre-slope, break, and slope change has $p = 0.0012$. This is the strongest apparent evidence of different market paths. It is also leverage-sensitive. Excluding Sabah Al-Ahmad Al-Marine changes the slope-change difference to -0.0418 [-0.2068, 0.1232], and the joint equality test has $p = 0.7886$. Under equal neighborhood-quarter weights, the joint test on all 36 has $p = 0.1725$, and after the same omission it has $p = 0.9512$.

The market comparison therefore has no stable sign or joint rejection once the influential exposure tail is altered. This could reflect truly different local composition in Sabah Al-Ahmad Al-Marine, but it cannot support a general claim that registered land and completed homes capitalized the reform differently.

5.4 Measurement and inference sensitivity

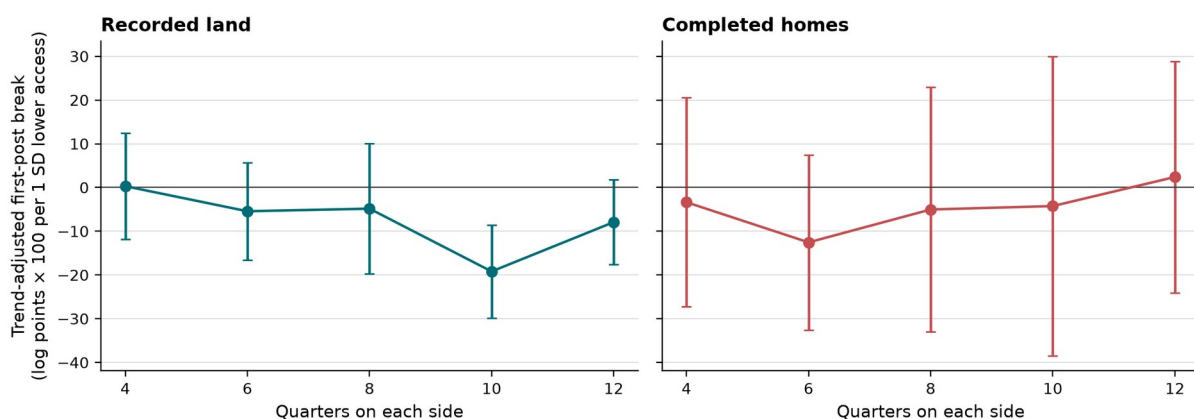
Changing the access decay from 10 kilometers to 5 or 20 leaves the transaction-weighted baseline estimates similar. The Kuwaiti-worker surface also produces nearly identical coefficients. These checks say that the result does not come from a particular smooth decay or from including non-Kuwaiti workplace employment. They do not add independent exposure support: each surface still ranks Sabah Al-Ahmad Al-Marine as an extreme low-access place.

Adding block-label fixed effects, dropping the plot-area control, or adding governorate-specific linear trends also leaves the long-window land slope change negative. Spatial covariance estimates with 10-, 20-, and 30-kilometer Bartlett bandwidths preserve the same point estimates. At 20 and 30 kilometers, the interval for the land break excludes zero. That is an inference sensitivity, not a resolution of influence. Smaller standard errors cannot turn a one-neighborhood contrast into broad geographic evidence.

5.5 Event windows and pseudo-breaks

The immediate-break estimate depends on which part of the cycle defines the pretrend. For recorded land it is 0.0030 in a four-quarter window, -0.0543 at six quarters, -0.0484 at eight, -0.1919 at ten, and -0.0792 at twelve. The absolute post slope also moves across windows. Completed-home breaks remain imprecise.

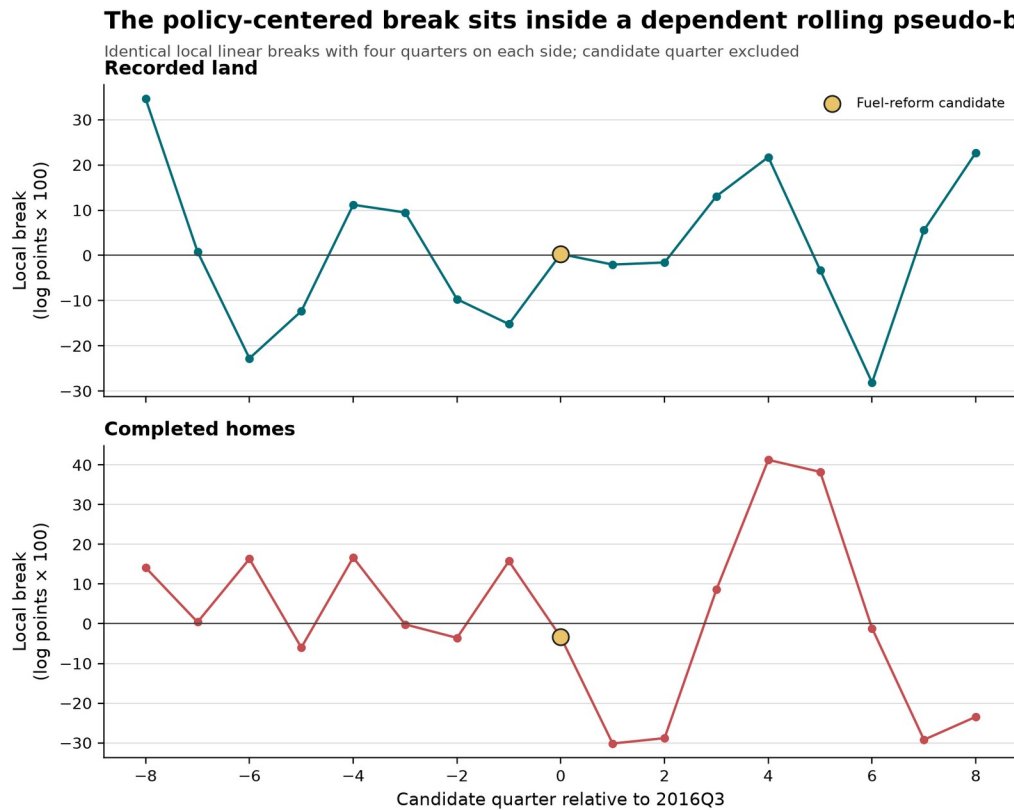
The estimated break depends on how much pre- and post-reform history is used



Each point re-estimates the same linear interrupted-gradient model; 2016Q3 excluded. Controlled descriptive evidence; not causal. Source: Kuwait Ministry of Justice; author's calculations.

Figure 4. Event-window sensitivity of the first-full-quarter break. Each estimate changes the number of quarters on either side while keeping the interrupted-gradient form. Units are log nominal transfer price per recorded plot square meter per standard deviation of lower access.

The rolling pseudo-break scan makes the local timing problem more visible. In the four-quarter model, the 2016Q4 land break is 0.0030 [-0.1184, 0.1244], and the completed-home break is -0.0337 [-0.2726, 0.2053]. Several nearby candidates have larger positive or negative estimates. Only four candidates have windows wholly before the reform, and adjacent windows overlap heavily, so the plot should not be read as a distribution of exchangeable placebo dates.



Adjacent windows overlap, and windows containing or following 2016Q3 include policy exposure. Descriptive pseudo-break scan. Source: Kuwait Ministry of Justice; author's calculations.

Figure 5. Rolling pseudo-break scan. Each candidate uses four quarters on either side and excludes the candidate quarter. Only the earliest candidates have windows wholly before the policy; adjacent windows share observations. This is not a placebo design or randomization inference.

Together, the window and timing exercises make the long-window result easier to interpret. The fitted land flattening is a feature of the multi-year transaction cycle. It is not a sharp, locally unique discontinuity at the first full quarter of the new price schedule.

6. Interpretation and limits

The observed land path is consistent with a commuting-cost story in one limited sense. A lower-access gradient that had been rising becomes roughly flat after driving costs increase. The sign of that slope change matches the idea that access becomes more valuable. The result cannot tell us whether that mechanism caused the change.

The main alternative is a neighborhood-specific development process. Sabah Al-Ahmad Al-Marine is not merely another point along a dense access continuum. It is an isolated exposure tail, a large share of land transfers, and overwhelmingly represented by one recorded block label. Infrastructure delivery, plot availability, expectations about a new district, waterfront amenities, or administrative recording could generate the observed cycle without any change in commuting-cost capitalization.

Sale selection is a second alternative. The dependent variable conditions on transactions, and the supported sample requires sales in both periods. A fall in the sale of lower-quality plots can raise the conditional mean price per recorded plot square meter even if underlying values do not rise. Conversely, a burst of newly released plots can move the measured gradient through composition. The data need a balanced neighborhood-quarter incidence panel and richer property characteristics before price and quantity margins can be separated.

Legal uncertainty further complicates a sharp expectation story. The first full post-reform quarter follows a lower-court ruling against the increase, even though the schedule remained in force. A buyer deciding whether the new fuel cost would persist faced more than a clean permanent shock. The quarterly model cannot identify when expectations changed.

External validity is correspondingly narrow. Kuwait combines regulated low fuel prices, a national reform, extensive driving, specific ownership institutions, and rapid outward development. Estimates from US commuting-price variation cannot be transported as magnitudes, and the Kuwait coefficient should not be transported to other Gulf cities. Even within Kuwait, the result concerns supported neighborhoods and observed registered transfers, not renters, commercial property, informal transactions, or the full housing stock.

No welfare conclusion follows. A relative price-gradient change does not reveal whether households gained or lost once government revenue, congestion, fuel use, sorting, and distributional effects are included. It also omits the national level component absorbed by quarter fixed effects. The analysis is best read as a diagnostic spatial accounting exercise around an economically meaningful reform.

7. Relation to the closest literature

The closest empirical papers use gasoline-price variation to study residential location and property values. Molloy and Shan find that higher gasoline prices reduce construction in long-commute locations, with no average house-price response and larger price effects in more supply-constrained areas (Molloy and Shan, 2013). Blake uses locally exogenous gasoline-price movements and finds demand shifts toward housing with shorter commutes or easier driving alternatives (Blake, 2019). Morris, Neill, and Coulson show theoretically and in Las Vegas data that housing-supply elasticity shapes how gasoline-

price changes rotate property-value gradients (Morris et al., 2020). These papers motivate job access and the importance of separating a land-development margin from completed structures.

Larson and Zhao are especially relevant to the oil-exporting setting because their model separates the transport-cost rotation of an urban price gradient from the income and demand effects of oil-producing specialization (Larson and Zhao, 2020). Kuwait's regulated retail reform is not their oil-price shock, but the distinction explains why a national energy-price change can have level and spatial channels that move differently.

Shehabi studies Kuwaiti energy-pricing reform in a general-equilibrium model with fiscal, labor-market, industry, and external-asset channels (Shehabi, 2017). That work does not estimate neighborhood property responses. It does show why a purely partial-equilibrium commuting interpretation is incomplete in Kuwait.

Methodologically, the paper follows the broader warning that visible pretrends and researcher-selected trend restrictions should change the estimand and uncertainty statement (Roth, 2022; Rambachan and Roth, 2023; Bilinski and Hatfield, 2026). We do not implement a causal difference-in-differences design or partial-identification bounds. The interrupted trend, leverage audit, and pseudo-break scan are descriptive tools for showing exactly where the counterfactual is weak.

8. What would change the interpretation

Four additions would most improve the paper.

First, a neighborhood-quarter panel including zero-sale cells would show whether the reform date coincides with transaction incidence, sold area, or composition changes. A sample selected only on pre-reform activity would reveal how much the current both-period support rule matters.

Second, parcel or project histories for Sabah Al-Ahmad Al-Marine would allow the influential case to be studied on its own terms. Infrastructure completion, plot release dates, development permissions, shoreline attributes, and block-level geocoding are more valuable here than another conventional regression control.

Third, clearer property measurement is essential. Verified land vacancy and developability, structure floor area and age, repeat sales, buyer and seller type, and financing terms would distinguish asset values from changing transaction bundles. Until then, “recorded land” and “completed home” should remain source categories rather than economic primitives.

Fourth, realized commuting or pre-reform network access would connect the exposure to behavior. Buyer workplaces, household nationality and vehicle use, or origin-destination flows could test whether

lower job access actually predicts reform-induced fuel expenditure. The Kuwaiti-worker access exercise validates a map, not a household treatment.

9. Conclusion

Kuwait's September 2016 gasoline-price reform occurred during a pronounced movement in the spatial pattern of registered land transactions. A long-window, transaction-weighted model describes that movement as an outward pre-reform gradient that becomes approximately flat afterward. Its absolute post slope is -0.0010 log points per quarter per standard deviation of lower access, not a continuing decline.

The average is not geographically broad. Sabah Al-Ahmad Al-Marine is both an isolated lower-access observation and 39.3 percent of effective land weight. Removing it reverses the land estimates; equal neighborhood-quarter weighting largely erases them. A same-36-neighborhood market comparison also loses its apparent land-versus-home distinction when that place is excluded. The honest result is therefore a case-driven spatial transaction cycle around a national policy date, not a stable asset-market response to fuel reform.

The empirical signal is concentrated enough to define the next research step: reconstruct what changed in Sabah Al-Ahmad Al-Marine's development and transaction process around 2016. Joining that history to sale incidence and realized commute data would separate a neighborhood development cycle from a gasoline-cost channel.

Data, code, and disclosure

The analysis package contains deterministic scripts, derived result tables, figures, and automated tests. Aggregate Ministry of Justice estimates and figures may be published with explicit attribution, but the underlying transaction extract is not a public replication source. Transaction rows, deed identifiers, and exact transaction locations remain private. CSB and PACI reuse and attribution terms also require confirmation. Source: Kuwait Ministry of Justice; author's calculations.

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