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From Managed Adjustment to Market Reform? Weak-Form Efficiency in Uzbekistan's Stock Index and Official USD/UZS Rate

Evidence from the Uzbekistan Composite Index and Official USD/UZS Rate, 2013-2026

Working Paper

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Abstract

This study tests several implications of weak-form market efficiency for the Uzbekistan Composite Index (UCI) and the official USD/UZS exchange rate. Corrected daily samples contain 2,389 UCI returns from 30 August 2016 to 4 May 2026 and 4,865 FX returns from 2 January 2013 to 5 May 2026. Weekly levels use the last available observation in Friday-ending weeks. The analysis combines descriptive and microstructure indicators, selected autocorrelations, Ljung-Box tests, sign runs tests, robust Lo-MacKinlay variance-ratio tests, unit-root checks, a 5 September 2017 reform split, a precisely defined no-jump robustness sample, and rolling windows. UCI evidence is mixed: robust variance-ratio tests fail to reject a random-walk null at all reported daily and weekly horizons, while the daily runs test and longer-lag Ljung-Box tests reject their distinct randomness nulls; weekly aggregation removes the runs-test rejection but not all serial dependence. Full-sample FX autocorrelations are small, yet runs and selected variance-ratio tests reject non-random adjustment. Pre-reform official-rate changes show pronounced dependence. Post-reform linear autocorrelation is weak when the liberalisation jump is included, but short-horizon dependence remains after that single return is excluded. The results support a partial, test-dependent, frequency-dependent, reform-dependent, and time-varying interpretation. They do not establish profitable trading strategies, and the FX findings describe official exchange-rate adjustment rather than a continuously traded, freely clearing FX market.

Keywords: efficient market hypothesis; random walk; variance ratio test; runs test; thin trading; frontier markets; Uzbekistan; exchange rate liberalization

JEL classifications: C22, G14, G15, F31

1. Introduction

Weak-form market efficiency concerns whether information in historical prices and returns has systematic predictive content. In Fama's (1970) framework, current prices should incorporate the information contained in past price histories; later work emphasizes that tests of efficiency are joint tests of a return-generating model and an information set (Fama, 1991). Perfect informational efficiency is also difficult to reconcile with costly information acquisition (Grossman & Stiglitz, 1980). Lo's (2004) Adaptive Markets Hypothesis therefore provides a useful interpretation for institutions in transition: efficiency can vary as participation, liquidity, rules, and competitive conditions change.

Uzbekistan is an informative setting because its securities market remains comparatively shallow and its exchange-rate system changed sharply in 2017. Studies of the domestic stock market identify low capitalization, limited infrastructure, weak participation, and continuing institutional development (Alimov & Mukhamedov, 2021; Ataniyazov & Sayfullokhon, 2022). These features make thin trading, stale index values, zero-return clustering, and non-synchronous trading central to interpretation. The USD/UZS observations used here are official rates published by the Central Bank of Uzbekistan, not transaction-level quotes from a continuously traded market.

The study asks whether UCI and official USD/UZS returns exhibit serial dependence, whether conclusions differ between daily and weekly frequencies, whether official-rate adjustment changed around the 2017 liberalization, and whether test outcomes vary over rolling windows. No single statistic is treated as a definitive efficiency verdict. Different procedures test different implications of weak-form unpredictability, and statistical rejection need not imply economically exploitable predictability.

2. Literature Review

The random-walk implication is commonly assessed through serial-correlation and variance-ratio tests. Lo and MacKinlay (1988) developed a variance-ratio specification test that compares the variance of q -period changes with q times the one-period variance. Runs tests instead examine the ordering of return signs, while Ljung-Box tests assess whether a set of autocorrelations is jointly zero. Because these null hypotheses are not mathematically identical, disagreement across tests is informative rather than inherently contradictory.

Evidence from emerging and transitional markets often depends on frequency and microstructure. Antoniou, Ergul, and Holmes (1997) link thin trading and nonlinear behavior in an emerging market. Borges (2010) reports mixed daily and weekly evidence across European stock indexes, while Abrosimova, Dissanaik, and Linowski (2002) find frequency-dependent results for Russia and caution that short-run predictability need not survive transaction costs. Aumeboonsuke and Dryver (2014) further emphasize the value of tests that do not depend on preliminary data mining. This study follows that cautious, multi-test approach.

Evidence from other transition and foreign-exchange markets reinforces the importance of institutional context. Guidi, Gupta, and Maheshwari (2011) find departures from random-walk behavior across Central and Eastern European equity markets using autocorrelation, runs, and variance-ratio tests. For foreign exchange, Chiang, Lee, Su, and Tzou (2010) report that random-walk conclusions differ across four Asian economies. These findings support comparing Uzbekistan's stock index and official exchange rate as institutionally distinct series rather than assuming that both absorb historical information in the same way.

The Uzbekistan literature located for this study focuses primarily on stock-market infrastructure, capitalization, participation, and development constraints (Alimov & Mukhamedov, 2021; Ataniyazov & Sayfullokhon, 2022). It provides much less joint evidence on weak-form implications for domestic equity and official exchange-rate series. The present study addresses this gap geographically by examining Uzbekistan, empirically by comparing the UCI with official USD/UZS observations, and methodologically by combining daily and weekly frequencies, a reform split, microstructure indicators, several distinct randomness tests, and rolling-window evidence.

Building on this literature, the study evaluates weak-form implications in two institutionally different Uzbekistan price series and asks whether the evidence changes with frequency, regime, and time. Specifically, it examines whether UCI and official USD/UZS returns exhibit serial dependence or other departures from weak-form unpredictability; whether results differ between daily and weekly frequencies, potentially reflecting thin trading, stale prices, or short-term market microstructure; whether official USD/UZS behavior changed around the 5 September 2017 liberalization; and whether weak-form efficiency is stable over time or varies across rolling windows.

3. Methodology

3.1 Data and sample construction

The UCI workbook, supplied by the Republican Stock Exchange 'Toshkent,' contains index levels, trading volume, and trading value; the exchange also publishes UCI levels and activity on its official price-indices page (Republican Stock Exchange 'Toshkent,' n.d.). The FX workbook contains official UZS-per-USD observations published through the Central Bank of Uzbekistan's exchange-rate and archive pages (Central Bank of the Republic of Uzbekistan, n.d.-a, n.d.-b). Raw workbooks are preserved unchanged. The UCI source stores dates as DD.MM.YYYY strings; FX dates are 4,860 native Excel datetimes and seven trailing

ISO YYYY-MM-DD strings. The corrected parser applies explicit format rules and raises on unknown dates. In particular, 1 May and 4 May 2026 remain May dates rather than being swapped to January and April.

Three exact duplicate UCI rows and one exact duplicate FX row are resolved by retaining the first source row and removing the identical later row. No conflicting duplicate values remain. Every retained and removed duplicate is stored in a machine-readable audit. Levels must be positive, dates must be unique and increasing, and all missing or infinite returns are checked.

Market-specific tests use each series' full sample. The common-date matched sample is used only for direct comparison and joint figures. Weekly levels are the last available observation in each Friday-ending week; returns are then calculated from weekly levels. The actual source observation date is retained alongside the week-ending label.

Table 1. Corrected sample coverage

Sample	Freq.	Level start	Level end	Levels	Return start	Return end	Returns
UCI full	Daily	2016-08-29	2026-05-04	2,390	2016-08-30	2026-05-04	2,389
FX full	Daily	2013-01-01	2026-05-05	4,866	2013-01-02	2026-05-05	4,865
UCI full	Weekly	2016-09-02	2026-05-08	502	2016-09-09	2026-05-08	501
FX full	Weekly	2013-01-04	2026-05-08	697	2013-01-11	2026-05-08	696
UCI-FX matched	Daily	2016-08-29	2026-05-04	2,390	2016-08-30	2026-05-04	2,389
UCI-FX matched	Weekly	2016-09-02	2026-05-08	502	2016-09-09	2026-05-08	501

Note. Weekly dates are Friday-ending labels; the last available level within each week is used.

3.2 Return construction

Continuously compounded percentage returns are computed only after sorting positive levels chronologically:

$$r_t = 100 \ln(P_t / P_{t-1})$$

$$r_t^{UCI} = 100 \ln(UCI_t / UCI_{t-1})$$

$$r_t^{FX} = 100 \ln(E_t / E_{t-1})$$

Here E_t is the official number of UZS per USD. A positive FX return is an increase in UZS per USD, or a depreciation of the soum in this quotation.

3.3 Statistical procedures

Descriptive statistics report the mean, median, minimum, maximum, sample standard deviation, bias-corrected skewness, excess kurtosis, and the Jarque-Bera test. Non-normality is treated as a distributional feature and a motivation for robust inference, not as proof of inefficiency. Microstructure indicators include zero-return shares, zero UCI trading volume and value, missing values, maximum calendar gaps, and the longest unchanged-return run.

Selected daily return autocorrelations use lags 1, 5, 10, and 20; weekly tests use 1, 4, 8, and 12, testing $H_0: \rho_k = 0$ at each selected lag k . Two-sided large-sample coefficient tests use the standard error $1/\sqrt{n}$, with the lag-specific paired sample reported. Ljung-Box Q statistics test $H_0, LB: \rho_1 = \rho_2 = \dots = \rho_m = 0$ through each selected lag m .

The Wald-Wolfowitz runs test classifies returns relative to zero and removes zero returns, testing H_0, R that the remaining return signs occur in a random sequence. It reports positive and negative counts, observed and expected runs, variance, a two-sided z statistic, and p-value. All relevant samples exceed 50 observations, so no continuity correction is applied. The formula is independently coded and checked against statsmodels. If one sign count is zero, the asymptotic variance is zero and the test is reported as not estimable rather than assigned a fabricated p-value.

Variance-ratio tests use log levels, overlapping q-period changes, a drift, finite-sample debiasing, and heteroskedasticity-robust Lo-MacKinlay standard errors to test H_0, VR : $VR(q) = 1$ at each horizon q. Daily horizons are q = 2, 4, 8, and 16; weekly horizons are q = 2, 4, 8, and 16. Homoskedastic statistics are retained for comparison, but the robust statistic determines the main decision. Every custom result is cross-checked against arch.unitroot.VarianceRatio.

ADF and Phillips-Perron tests are applied to daily log levels and returns under a constant and a constant-plus-linear-trend specification. ADF lags are selected by AIC; Phillips-Perron uses the package's automatic Newey-West bandwidth. These tests are supplementary consistency checks. Failure to reject a unit root in levels is not, by itself, evidence that historical returns are unpredictable.

H_0, ρ , H_0, LB , H_0, R , and H_0, VR test different implications of weak-form unpredictability rather than a single common restriction. Rejection under one null therefore need not coincide with rejection under another, as discussed in Section 2.

3.4 Reform, no-jump, and rolling designs

The daily reform cutoff is 5 September 2017, the date of the large official rate adjustment associated with liberalization. Returns dated before the cutoff are pre-reform; the 5 September return is the first post-reform observation and includes the jump. The week ending 8 September 2017 is the first post-reform week. The no-jump sample excludes exactly that one daily return or that one weekly return. It does not remove a level and does not create a return spanning the omitted observation.

Rolling autocorrelation uses lag 1 and fixed windows of 500 daily or 104 weekly returns, testing H_0, ρ within each window, with approximate 95% bounds of $\pm 1.96/\sqrt{n}$. Rolling variance ratios use q = 4, the same fixed window lengths in levels, robust statistics, and a separate 5% rejection indicator for H_0, VR in each window. Each rolling estimate is dated at its window end. Zero returns are retained in autocorrelation and variance-ratio analysis.

All tests use a 5% significance level. The corrected implementation ran under Python 3.14.6 with pandas 3.0.5, NumPy 2.5.1, SciPy 1.18.0, statsmodels 0.14.6, arch 8.0.0, and Matplotlib 3.11.1. A fixed random seed is set although the reported procedures are deterministic.

4. Empirical Results

4.1 Sample cleaning and descriptive statistics

Correct parsing produces 2,390 UCI levels and 4,866 FX levels. The matched daily sample now reaches 4 May 2026, and the corrected matched weekly sample contains 501 returns through the week ending 8 May 2026. Figure 1 shows the log levels on their separate scales; the 2017 official-rate movement is visually dominant in USD/UZS.

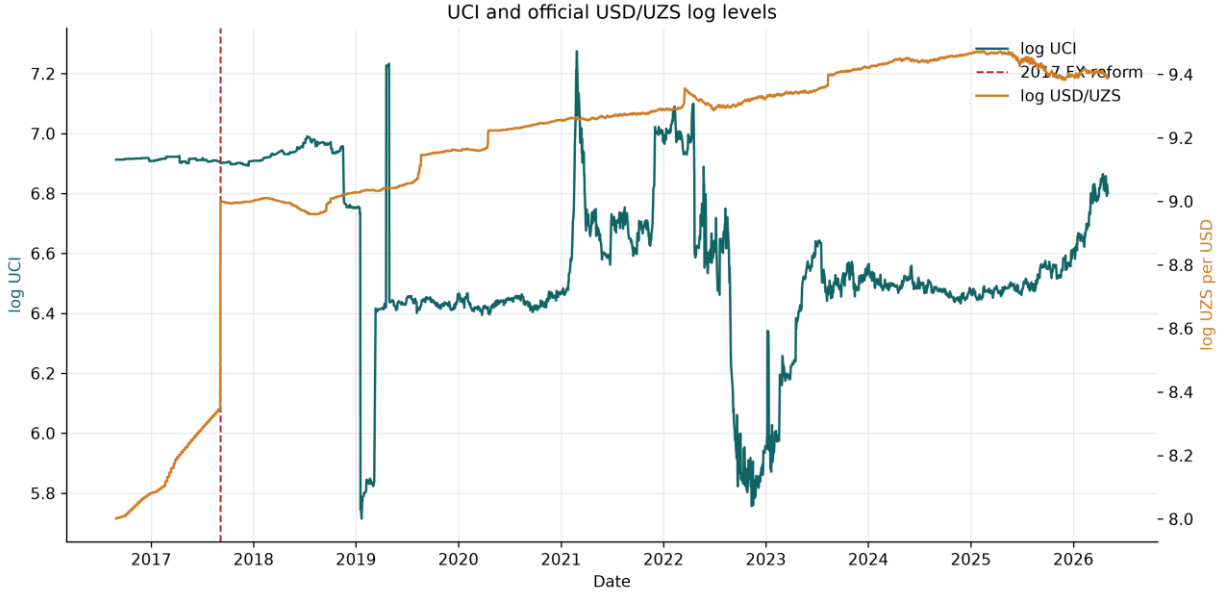


Figure 1. UCI and official USD/UZS log levels.

Table 2. Descriptive statistics for full-sample returns

Sample	Freq.	N	Mean	SD	Min.	Max.	Skew.	Excess kurt.	JB p
UCI full	Daily	2,389	-0.0047	4.1904	-98.9623	80.7015	-4.3957	262.3466	<0.0001
UCI full	Weekly	501	-0.0223	8.7583	-100.1329	79.7970	-2.6878	65.7763	<0.0001
FX full	Daily	4,865	0.0370	0.9560	-0.8581	65.4318	65.8857	4504.2692	<0.0001
FX full	Weekly	696	0.2584	2.5252	-1.3457	65.4318	24.8403	641.0195	<0.0001

Note. Returns are percentages. Kurtosis is excess kurtosis. Very small presentation p-values are shown as <0.0001; raw numeric values are retained in output files.

All four return series are strongly non-normal. UCI returns contain large positive and negative movements, while the 65.4318% reform-day FX return drives much of the full-sample positive skewness and kurtosis. Removing only that return reduces post-reform daily FX standard deviation from 1.1817% to 0.2042%, skewness from 53.7458 to 7.5627, and excess kurtosis from 2,973.24 to 130.07. Non-normality nevertheless remains, so it cannot be attributed solely to the reform movement.

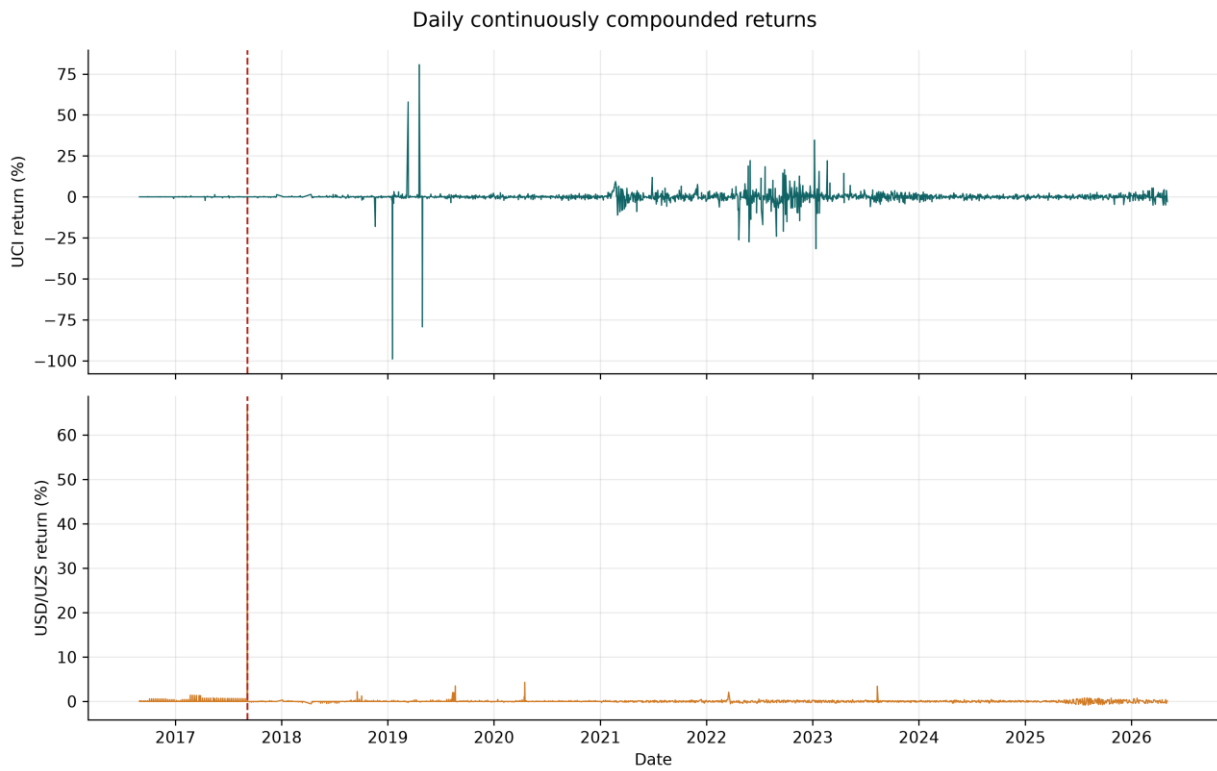


Figure 2. Daily UCI and official USD/UZS returns.

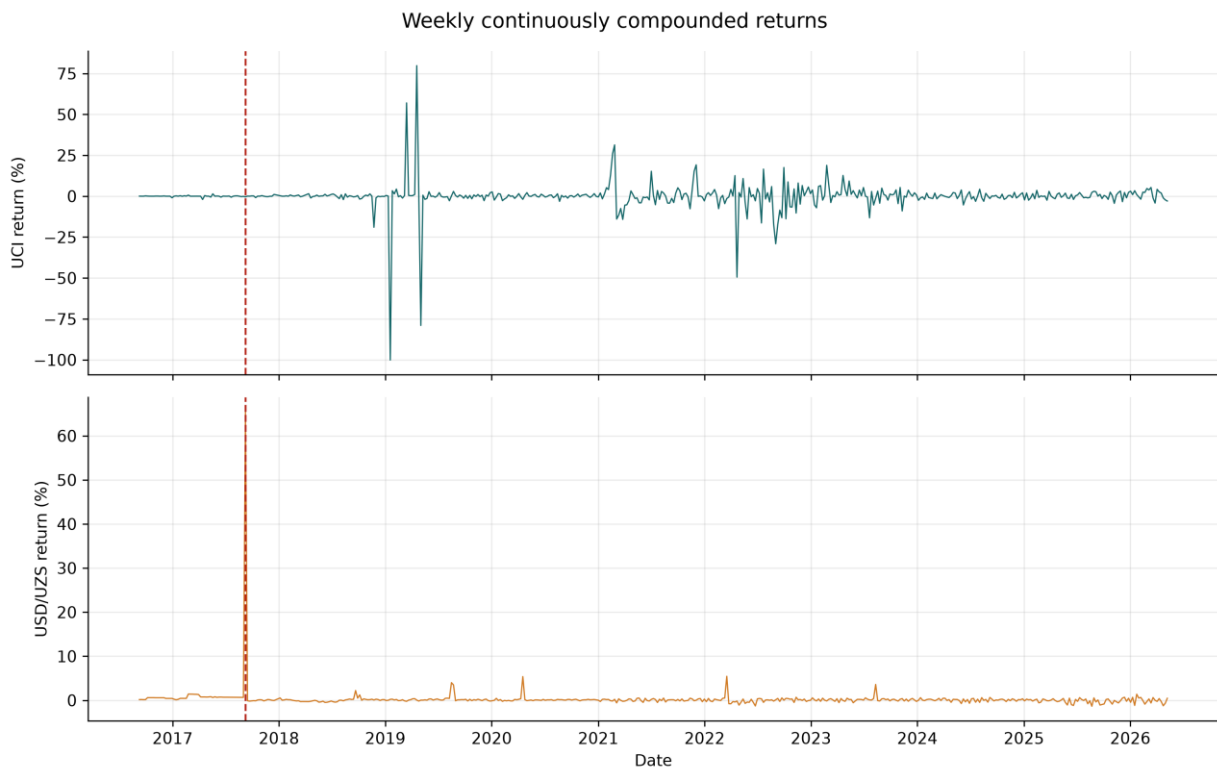


Figure 3. Weekly UCI and official USD/UZS returns.

Table 3. Market-microstructure indicators

Market	Freq.	Returns	Zero returns	Zero share	Longest run	Max (days) gap	Zero-volume share
UCI	Daily	2,389	253	10.59%	16	26	1.76%
FX	Daily	4,865	3,142	64.58%	9	3	n/a
UCI	Weekly	501	14	2.79%	2	21	n/a
FX	Weekly	696	0	0.00%	0	7	n/a

Note. The zero-volume measure is available only for daily UCI levels. Missing return counts after the initial differencing observation are zero.

Daily UCI returns are unchanged on 253 days, or 10.59% of the sample; the weekly share falls to 2.79%. These values are essentially the previously reported 10.6% and 2.8%. The daily FX zero-return share is much larger at 64.58%, consistent with stepwise official-rate publication and adjustment. These features caution against reading daily dependence as frictionless trading predictability.

4.2 Autocorrelation and Ljung-Box tests

Table 4. Selected return autocorrelations

Variable	Freq.	Lag	N	ρ	z	p	H ₀ : $\rho=0$
UCI	Daily	1	2,388	-0.0086	-0.4222	0.6729	Fail to reject
UCI	Daily	5	2,384	0.0368	1.7947	0.0727	Fail to reject
UCI	Daily	10	2,379	-0.0001	-0.0055	0.9956	Fail to reject
UCI	Daily	20	2,369	0.0057	0.2782	0.7809	Fail to reject
USD/UZS	Daily	1	4,864	0.0027	0.1895	0.8497	Fail to reject
USD/UZS	Daily	5	4,860	-0.0008	-0.0559	0.9555	Fail to reject
USD/UZS	Daily	10	4,855	-0.0014	-0.0989	0.9212	Fail to reject
USD/UZS	Daily	20	4,845	-0.0013	-0.0931	0.9258	Fail to reject
UCI	Weekly	1	500	0.0153	0.3411	0.7330	Fail to reject
UCI	Weekly	4	497	-0.0155	-0.3451	0.7300	Fail to reject
UCI	Weekly	8	493	-0.0933	-2.0714	0.0383	Reject
UCI	Weekly	12	489	0.0059	0.1299	0.8967	Fail to reject
USD/UZS	Weekly	1	695	0.0115	0.3037	0.7614	Fail to reject
USD/UZS	Weekly	4	692	0.0096	0.2535	0.7999	Fail to reject
USD/UZS	Weekly	8	688	0.0108	0.2821	0.7778	Fail to reject
USD/UZS	Weekly	12	684	0.0095	0.2483	0.8039	Fail to reject

Note. Tests are two-sided with the large-sample standard error $1/\sqrt{n}$. A non-significant selected coefficient is not proof of joint independence.

None of the selected daily UCI coefficients is individually significant. Weekly UCI autocorrelation is negative and significant at lag 8. Selected full-sample FX coefficients are small and insignificant at both frequencies. These coefficient tests concern only the reported lags.

Table 5. Ljung-Box joint serial-correlation tests

Variable	Freq.	Through lag	Q	p	Joint null
UCI	Daily	1	0.1785	0.6727	Fail to reject
UCI	Daily	5	5.0114	0.4145	Fail to reject
UCI	Daily	10	82.9450	<0.0001	Reject

Variable	Freq.	Through lag	Q	p	Joint null
UCI	Daily	20	85.4539	<0.0001	Reject
USD/UZS	Daily	1	0.0359	0.8497	Fail to reject
USD/UZS	Daily	5	0.1142	0.9998	Fail to reject
USD/UZS	Daily	10	1.4466	0.9991	Fail to reject
USD/UZS	Daily	20	2.2631	1.0000	Fail to reject
UCI	Weekly	1	0.1172	0.7320	Fail to reject
UCI	Weekly	4	10.1372	0.0382	Reject
UCI	Weekly	8	25.7371	0.0012	Reject
UCI	Weekly	12	26.1176	0.0103	Reject
USD/UZS	Weekly	1	0.0928	0.7607	Fail to reject
USD/UZS	Weekly	4	0.2290	0.9939	Fail to reject
USD/UZS	Weekly	8	0.4608	0.9999	Fail to reject
USD/UZS	Weekly	12	0.6279	1.0000	Fail to reject

Note. The null is that all return autocorrelations through the stated lag are zero.

The Ljung-Box results resolve the earlier contradiction. Selected short-lag evidence is weak: daily UCI does not reject through lags 1 or 5, but the joint null is rejected strongly through the longer lag sets of 10 and 20. This pattern indicates longer-horizon or cumulative dependence in UCI returns. Small selected individual coefficients can coexist with joint rejection because the Q statistic accumulates dependence across all intervening lags. Weekly UCI rejects through lag sets 4, 8, and 12. Full-sample FX fails to reject at every reported daily and weekly lag set.

4.3 Runs tests

Table 6. Wald-Wolfowitz sign runs tests

Variable	Freq.	Original N	Zeros removed	Used N	Positive	Negative	Runs	z	p	Decision
UCI	Daily	2,389	253	2,136	1,117	1,019	1,142	3.2639	0.0011	Reject random signs
USD/UZS	Daily	4,865	3,142	1,723	1,057	666	559	-13.1679	<0.0001	Reject random signs
UCI	Weekly	501	14	487	272	215	252	0.9967	0.3189	Fail to reject random signs
USD/UZS	Weekly	696	0	696	523	173	199	-6.2984	<0.0001	Reject random signs

Note. Zero returns are excluded because they cannot be classified as positive or negative runs. Used N therefore contains only nonzero returns and equals positive plus negative observations; zero returns are not missing observations. The large number of zero official FX returns reflects stepwise official-rate adjustment. The independent formula matches statsmodels, and no continuity correction applies because samples exceed 50 observations.

The corrected interpretation is the opposite of the reversed statement in the earlier draft: daily UCI rejects random sign ordering ($p = 0.0011$), whereas weekly UCI fails to reject ($p = 0.3189$). Aggregation therefore weakens this particular evidence of non-randomness, consistent with daily zero-return clustering, stale values, and thin-market microstructure. It is not direct evidence of a profitable rule. FX sign randomness is

rejected daily and weekly in the full sample, describing non-random official-rate adjustment rather than automatically demonstrating free-market FX inefficiency.

4.4 Variance-ratio tests

Table 7. Heteroskedasticity-robust variance-ratio tests

Variable	Freq.	q	VR(q)	Robust z	p	H ₀ : VR(q)=1
UCI	Daily	2	0.9921	-0.5162	0.6057	Fail to reject
UCI	Daily	4	0.9640	-0.9907	0.3219	Fail to reject
UCI	Daily	8	0.9290	-0.9441	0.3451	Fail to reject
UCI	Daily	16	0.7981	-1.0358	0.3003	Fail to reject
USD/UZS	Daily	2	1.0031	2.4885	0.0128	Reject
USD/UZS	Daily	4	1.0020	0.8256	0.4091	Fail to reject
USD/UZS	Daily	8	1.0019	0.4291	0.6678	Fail to reject
USD/UZS	Daily	16	1.0168	1.3615	0.1733	Fail to reject
UCI	Weekly	2	1.0192	0.4697	0.6386	Fail to reject
UCI	Weekly	4	0.8830	-0.6578	0.5107	Fail to reject
UCI	Weekly	8	0.8516	-0.5287	0.5970	Fail to reject
UCI	Weekly	16	0.6593	-0.8167	0.4141	Fail to reject
USD/UZS	Weekly	2	1.0144	1.6682	0.0953	Fail to reject
USD/UZS	Weekly	4	1.0367	2.2408	0.0250	Reject
USD/UZS	Weekly	8	1.0821	3.2271	0.0013	Reject
USD/UZS	Weekly	16	1.1747	4.8239	<0.0001	Reject

Note. Overlapping, finite-sample-debiased tests include a drift. Custom values match arch.unitroot.VarianceRatio to floating-point tolerance.

UCI fails to reject the random-walk null at every reported daily and weekly horizon under robust inference. This does not erase the runs and Ljung-Box rejections: variance-ratio tests emphasize cumulative variance restrictions and can have different sensitivity. Daily FX rejects only at $q = 2$, whereas weekly FX rejects at $q = 4, 8$, and 16 . The full-sample FX pattern therefore combines weak selected linear autocorrelations with non-random sign ordering and horizon-specific variance scaling.

4.5 Unit-root supporting evidence

Table 8. ADF and Phillips-Perron unit-root tests

Variable	Series	Test	Deterministic terms	Statistic	p	Unit-root null
UCI	log level	ADF	Constant	-3.3749	0.0118	Reject
UCI	log level	Phillips-Perron	Constant	-3.4407	0.0096	Reject
UCI	return	ADF	Constant	-9.2502	<0.0001	Reject
UCI	return	Phillips-Perron	Constant	-49.9163	<0.0001	Reject
UCI	log level	ADF	Constant + trend	-3.4962	0.0398	Reject
UCI	log level	Phillips-Perron	Constant + trend	-3.6080	0.0292	Reject
UCI	return	ADF	Constant + trend	-9.2640	<0.0001	Reject
UCI	return	Phillips-Perron	Constant + trend	-49.9217	<0.0001	Reject
USD/UZS	log level	ADF	Constant	-1.5849	0.4912	Fail to reject

Variable	Series	Test	Deterministic terms	Statistic	p	Unit-root null
USD/UZS	log level	Phillips-Perron	Constant	-1.5777	0.4948	Fail to reject
USD/UZS	return	ADF	Constant	-69.5382	<0.0001	Reject
USD/UZS	return	Phillips-Perron	Constant	-69.5461	<0.0001	Reject
USD/UZS	log level	ADF	Constant + trend	-0.6316	0.9773	Fail to reject
USD/UZS	log level	Phillips-Perron	Constant + trend	-0.6557	0.9759	Fail to reject
USD/UZS	return	ADF	Constant + trend	-69.5612	<0.0001	Reject
USD/UZS	return	Phillips-Perron	Constant + trend	-69.5636	<0.0001	Reject

Note. ADF lags are selected by AIC; Phillips-Perron uses an automatic Newey-West bandwidth. These are supporting checks, not standalone EMH tests.

Both ADF and Phillips-Perron reject a unit root in UCI log levels under both deterministic specifications, while they fail to reject for FX log levels. All return series are stationary by these tests. The UCI level result is not consistent with a simple unit-root model even though robust variance-ratio tests fail to reject at the selected horizons. This is further evidence that a binary conclusion based on one test would be misleading. The FX level failure to reject is only a consistency check and is not proof of weak-form efficiency.

4.6 Reform split and no-jump robustness

Table 9. FX reform and no-jump samples

Sample	Freq.	Start	End	Returns	Reform excluded return
FX pre-reform daily	Daily	2013-01-02	2017-09-04	1,706	No
FX post-reform daily	Daily	2017-09-05	2026-05-05	3,159	No
FX post-reform no-jump daily	Daily	2017-09-06	2026-05-05	3,158	Yes
FX pre-reform weekly	Weekly	2013-01-11	2017-09-01	243	No
FX post-reform weekly	Weekly	2017-09-08	2026-05-08	453	No
FX post-reform no-jump weekly	Weekly	2017-09-15	2026-05-08	452	Yes

Note. The daily reform return is dated 5 September 2017. The corresponding weekly return is dated 8 September 2017.

Table 10. Reform-sample dependence diagnostics

Regime	Freq.	Lag-1 ρ (p)	Ljung-Box Q at max lag (p)	Runs z (p)
pre-reform	Daily	-0.0940 (0.0001)	2126.8536 (<0.0001)	Not estimable
post-reform	Daily	0.0229 (0.1988)	0.1202 (1.0000)	-9.1719 (<0.0001)
post-reform no-jump	Daily	0.1386 (<0.0001)	131.6869 (<0.0001)	-9.2051 (<0.0001)
pre-reform	Weekly	0.9310 (<0.0001)	1152.5358 (<0.0001)	Not estimable
post-reform	Weekly	0.0089 (0.8496)	0.0161 (1.0000)	-1.5807 (0.1140)
post-reform no-jump	Weekly	0.1253 (0.0078)	19.2260 (0.0832)	-1.6516 (0.0986)

Note. Maximum Ljung-Box lag is 20 daily and 12 weekly. Pre-reform runs tests are not estimable because all 243 nonzero daily changes and all weekly changes are positive after zero returns are removed.

Pre-reform official-rate adjustment is strongly patterned. Daily lag-1 autocorrelation is -0.0940, and the weekly coefficient is 0.9310; Ljung-Box tests reject across the reported lag sets. The sign runs statistic is not

estimable because every nonzero pre-reform change is positive. This itself documents a one-directional official-rate path, but no asymptotic p-value is fabricated.

In the full post-reform sample, selected autocorrelations and Ljung-Box tests fail to reject at daily and weekly frequencies. The daily sign runs test still rejects, while the weekly runs test fails to reject. Removing exactly the reform return changes the scale of the series: daily lag-1 autocorrelation rises to 0.1386 and weekly lag-1 autocorrelation to 0.1253; daily no-jump Ljung-Box tests reject through all reported lag sets, while weekly tests reject through lag 8 but not 12. This result was checked against chronological ordering and direct return exclusion. The 65.4318% movement inflates the full post-reform variance and masks smaller serial covariance; exclusion does not create a return across the removed observation.

Table 11. Reform and no-jump robust variance-ratio tests

Sample	q	VR(q)	Robust z	p	Random-walk null
pre-reform daily	2	0.9071	-9.0743	<0.0001	Reject
pre-reform daily	4	0.7202	-14.6029	<0.0001	Reject
pre-reform daily	8	0.6263	-6.6379	<0.0001	Reject
pre-reform daily	16	1.0418	0.2544	0.7991	Fail to reject
post-reform daily	2	0.5193	-432.9742	<0.0001	Reject
post-reform daily	4	0.2783	-333.7626	<0.0001	Reject
post-reform daily	8	0.1574	-248.8633	<0.0001	Reject
post-reform daily	16	0.1036	-165.9558	<0.0001	Reject
post-reform no-jump daily	2	1.1389	4.1328	<0.0001	Reject
post-reform no-jump daily	4	1.1681	2.5354	0.0112	Reject
post-reform no-jump daily	8	1.1544	1.5085	0.1314	Fail to reject
post-reform no-jump daily	16	1.3090	2.0721	0.0383	Reject
pre-reform weekly	2	1.9328	5.1655	<0.0001	Reject
pre-reform weekly	4	3.6787	8.1913	<0.0001	Reject
pre-reform weekly	8	6.6659	11.8971	<0.0001	Reject
pre-reform weekly	16	11.2272	16.6477	<0.0001	Reject
post-reform weekly	2	0.5226	-81.6084	<0.0001	Reject
post-reform weekly	4	0.2884	-63.7453	<0.0001	Reject
post-reform weekly	8	0.1846	-48.5657	<0.0001	Reject
post-reform weekly	16	0.1488	-38.7448	<0.0001	Reject
post-reform no-jump weekly	2	1.1296	1.3284	0.1840	Fail to reject
post-reform no-jump weekly	4	1.2614	1.6963	0.0898	Fail to reject
post-reform no-jump weekly	8	1.4704	2.4343	0.0149	Reject
post-reform no-jump weekly	16	1.6807	2.9175	0.0035	Reject

Note. For each return-defined regime, a contiguous log-level path is reconstructed from the retained returns. The no-jump path excludes exactly the reform return.

Pre-reform variance ratios reject at most horizons, especially in weekly data. The post-reform sample including the jump also rejects strongly because a single movement at the beginning of the regime dominates one-period variance relative to subsequent cumulative changes. In the no-jump sample, daily tests reject at $q = 2, 4$, and 16 but not $q = 8$; weekly tests reject at $q = 8$ and 16 but not $q = 2$ or 4 . Short-horizon dependence therefore remains after excluding the reform-related jump, but the evidence is horizon-specific and does not show that the jump caused all later non-randomness.

4.7 Rolling-window evidence

Table 12. Rolling-window summary

Measure	Market	Freq.	Window	First endpoint	Last endpoint	Min.	Max.	Reject share
Lag-1 autocorrelation	FX	Daily	500	2014-05-17	2026-05-05	-0.1613	0.4984	52.45%
Lag-1 autocorrelation	FX	Weekly	104	2015-01-02	2026-05-08	-0.1537	0.9159	40.64%
Lag-1 autocorrelation	UCI	Daily	500	2018-10-03	2026-05-04	-0.2162	0.2176	37.14%
Lag-1 autocorrelation	UCI	Weekly	104	2018-09-28	2026-05-08	-0.3844	0.4724	21.86%
VR($q=4$)	FX	Daily	500	2014-05-16	2026-05-05	0.2583	1.9245	45.25%
VR($q=4$)	FX	Weekly	104	2014-12-26	2026-05-08	0.2953	3.7146	32.83%
VR($q=4$)	UCI	Daily	500	2018-10-02	2026-05-04	0.2972	1.5730	26.86%
VR($q=4$)	UCI	Weekly	104	2018-09-21	2026-05-08	0.3124	1.8706	9.27%

Note. Autocorrelation rejection uses approximate 95% bounds. Variance-ratio rejection uses the robust two-sided p-value. The share is descriptive and windows overlap heavily.

Rolling estimates vary substantially across time. The movement of a variance ratio above or below one is not treated as significant unless the robust rolling test rejects. Figures 4-11 therefore show confidence bounds or explicit rejection markers. The corrected series end on 4 May 2026 for daily UCI, 5 May 2026 for daily FX, and 8 May 2026 for weekly estimates. This time variation is compatible with an adaptive-market interpretation, but the overlapping windows do not identify causal effects of the reform.

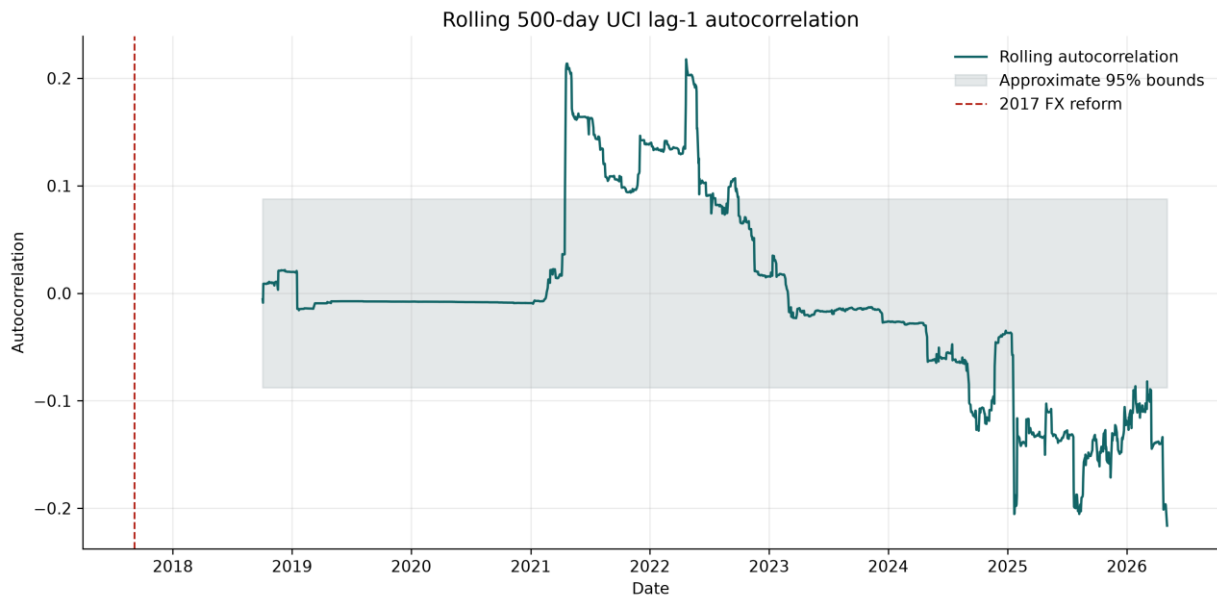


Figure 4. Rolling 500-day UCI lag-1 autocorrelation.

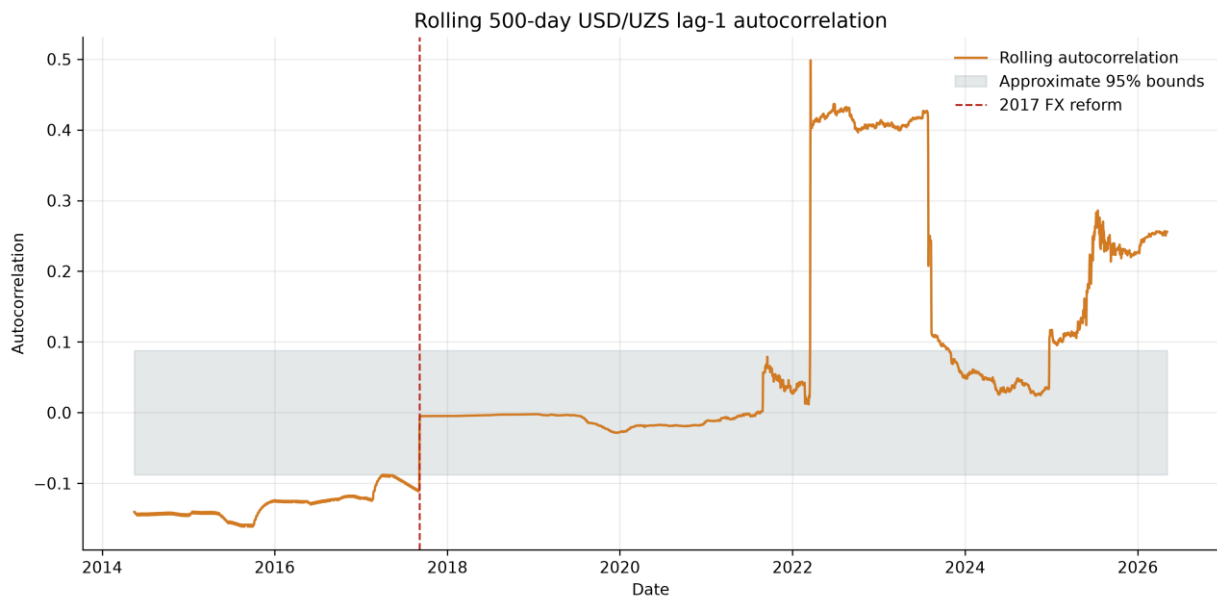


Figure 5. Rolling 500-day official USD/UZS lag-1 autocorrelation.

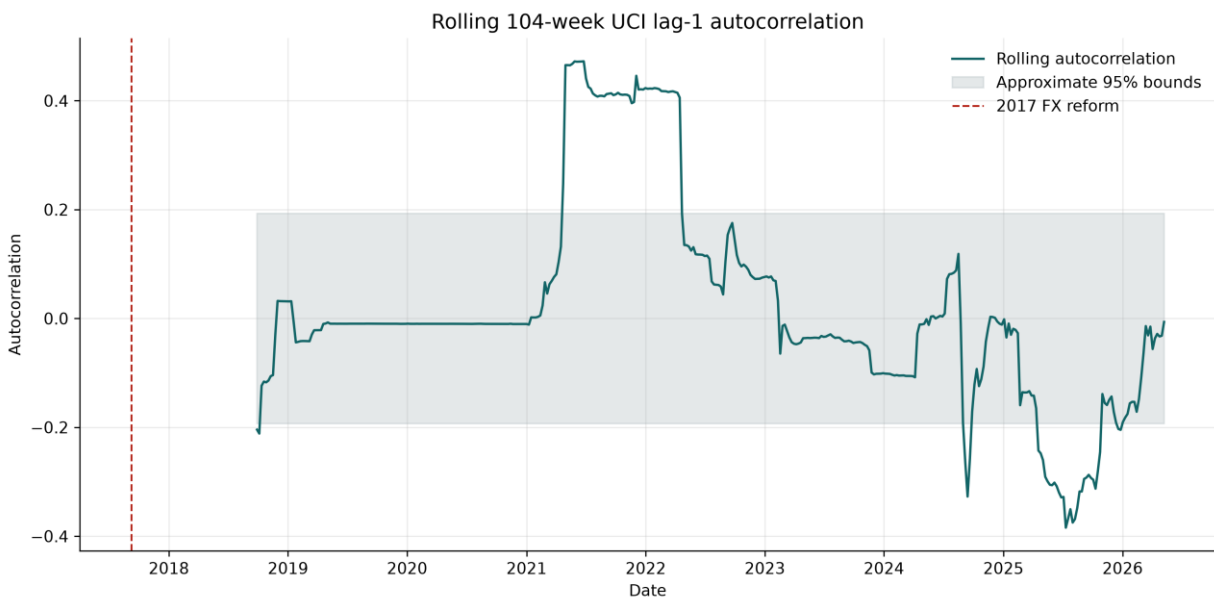


Figure 6. Rolling 104-week UCI lag-1 autocorrelation.

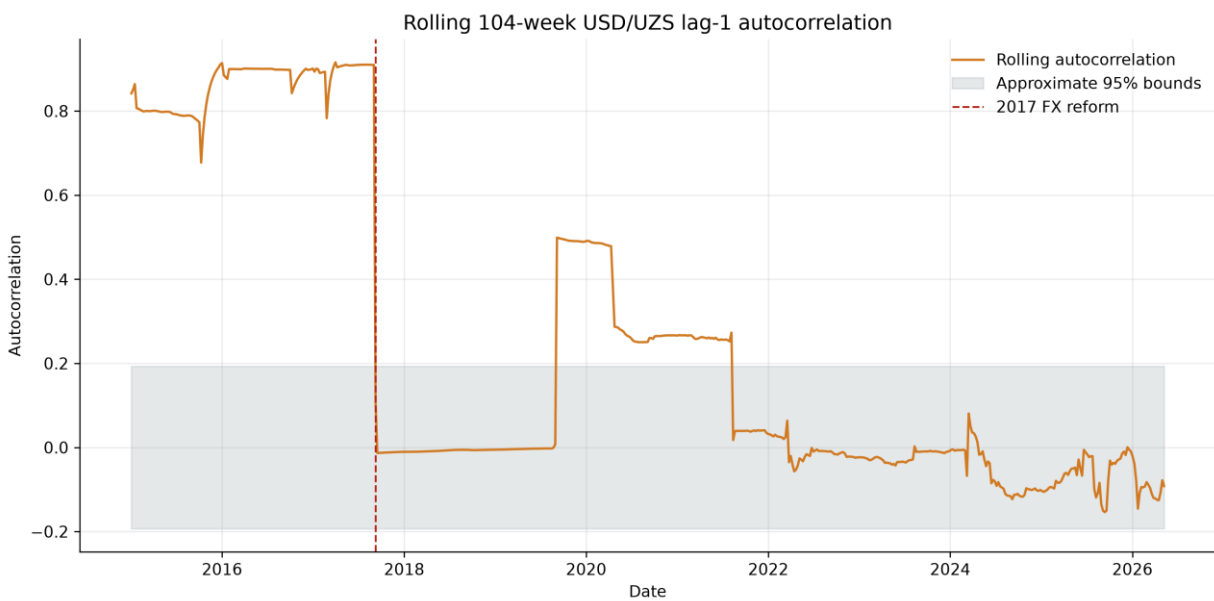


Figure 7. Rolling 104-week official USD/UZS lag-1 autocorrelation.

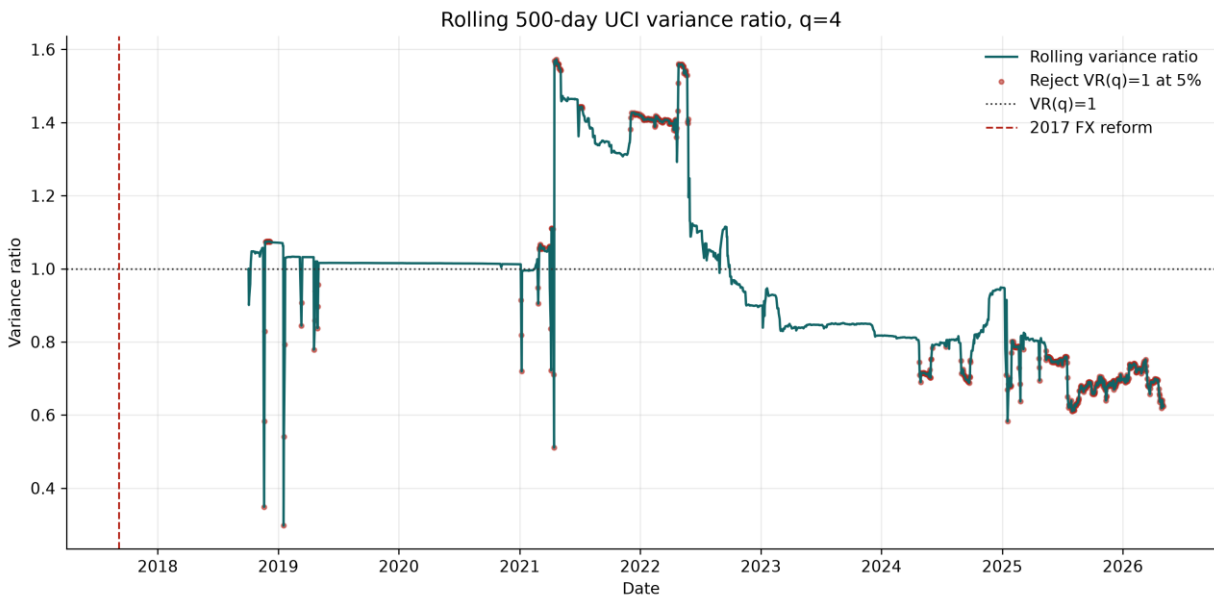


Figure 8. Rolling 500-day UCI variance ratio, $q = 4$.

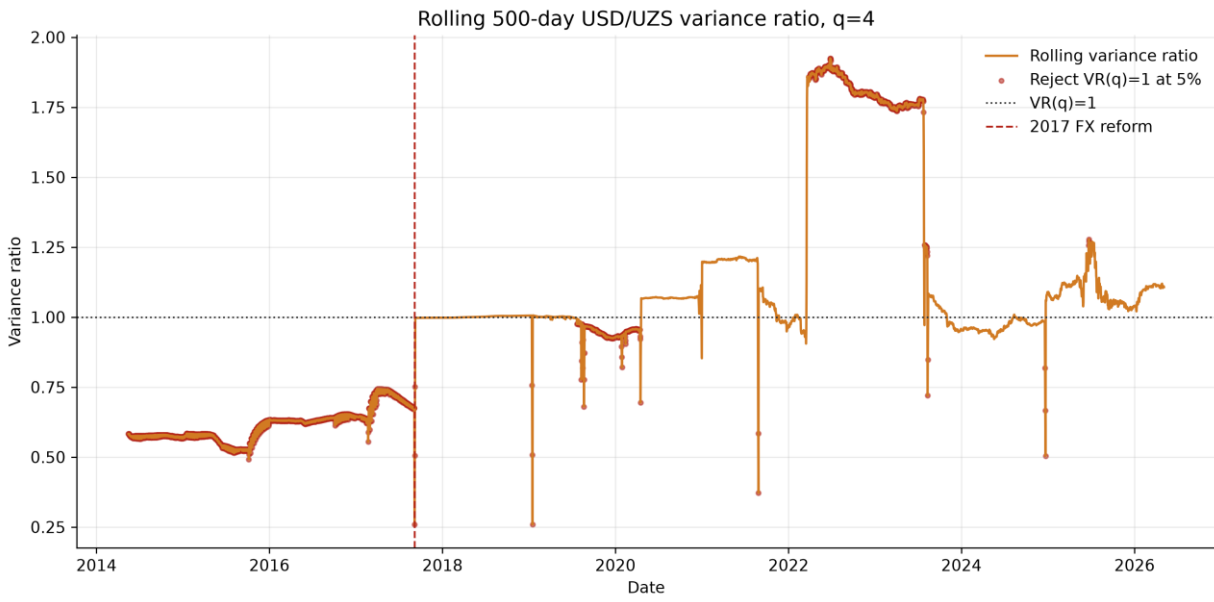


Figure 9. Rolling 500-day official USD/UZS variance ratio, $q = 4$.

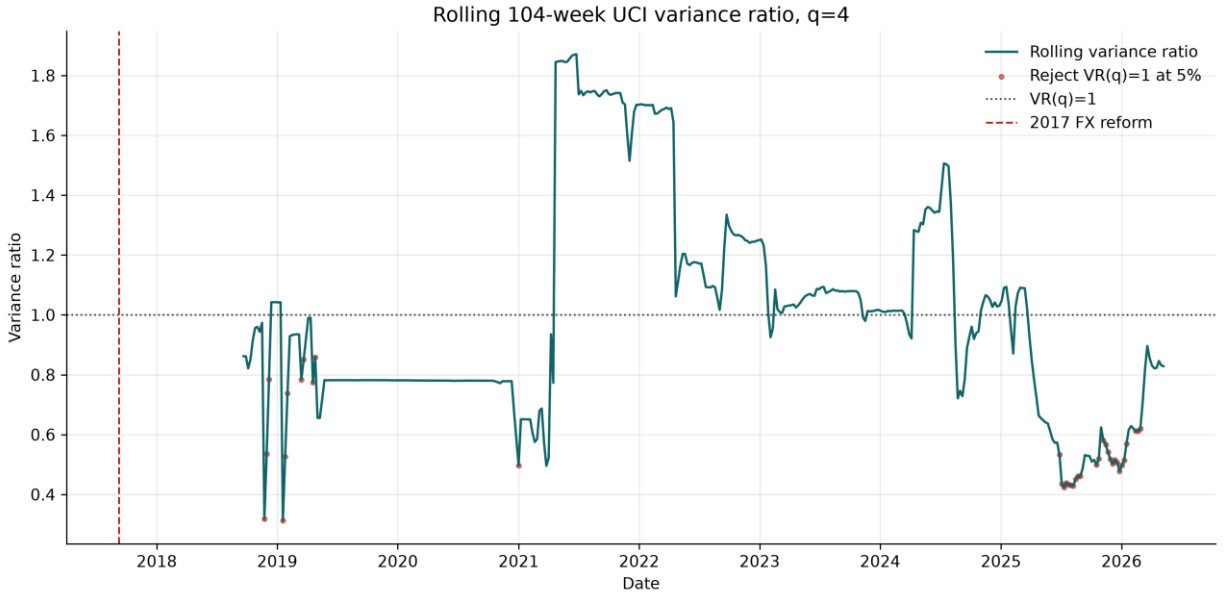


Figure 10. Rolling 104-week UCI variance ratio, $q = 4$.

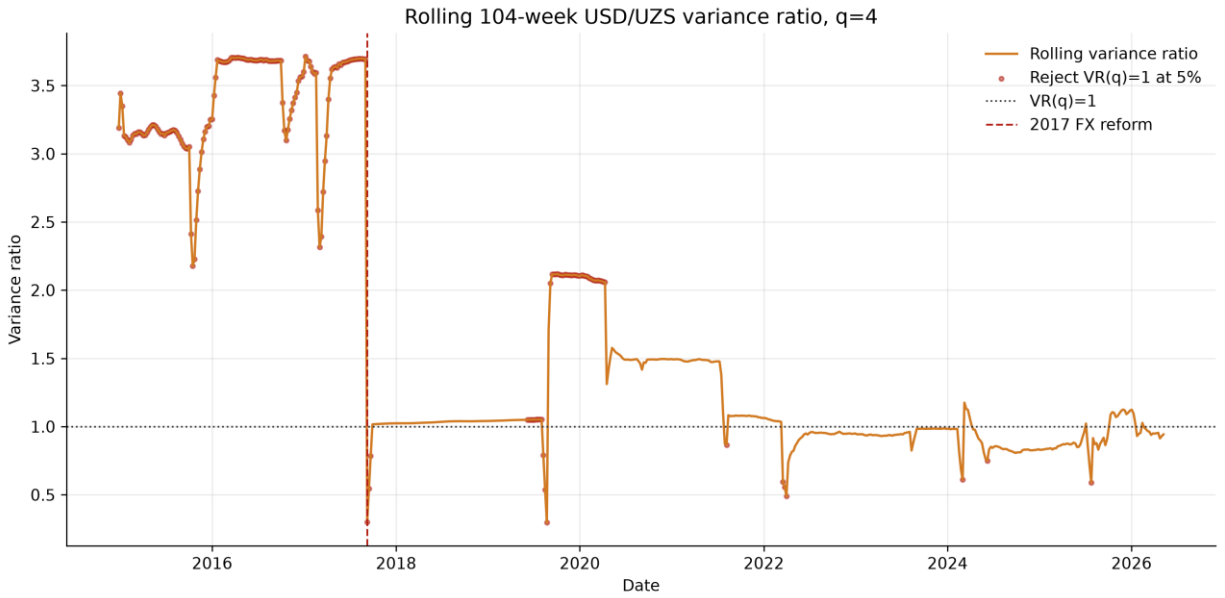


Figure 11. Rolling 104-week official USD/UZS variance ratio, $q = 4$.

4.8 Synthesis across tests

The UCI results are mixed because sign ordering, joint serial correlation, variance scaling, and unit-root persistence are different restrictions. Daily runs and longer-lag Ljung-Box tests detect departures, robust variance-ratio tests do not, and unit-root tests reject a unit root in log levels. Weekly aggregation removes the runs rejection but leaves selected negative autocorrelation and joint Ljung-Box rejection. Thin trading, stale index values, non-synchronous component prices, and zero-return clustering are plausible contributors.

For official USD/UZS, full-sample selected autocorrelations and Ljung-Box statistics are weak, but sign runs and selected variance-ratio horizons reject. The reform split reveals strongly managed pre-reform adjustment and much weaker post-reform linear dependence when the jump is retained. No-jump and rolling evidence

shows that adjustment remains time-varying. These results concern an official published series and should not be generalized automatically to a freely traded FX market.

5. Discussion

RQ1 receives a mixed answer. Historical-return diagnostics detect some dependence in both series, but conclusions differ across null hypotheses. RQ2 is also supported: daily and weekly UCI runs results differ, although weekly Ljung-Box evidence remains. RQ3 is supported as a change in official adjustment behavior: pre-reform dependence is substantially stronger than full post-reform linear dependence. This is an institutional break interpretation, not an unsupported claim that liberalization caused complete market efficiency. RQ4 is supported descriptively by rolling estimates and rejection indicators that vary over time. In the post-reform robustness analysis, removing exactly the 5 September 2017 return reveals short-horizon dependence in some tests because the very large jump otherwise inflates variance and can mask smaller serial covariance. This means the detected post-reform non-randomness is not solely an artifact of that one-time movement; it does not show that the reform caused all subsequent dependence.

The combined evidence is consistent with partial and adaptive efficiency. The word 'partial' does not mean averaging p-values or declaring a market half efficient. It means that different measurable implications hold in different samples, frequencies, and institutional regimes. Economic predictability would additionally require an implementable forecasting rule, out-of-sample validation, transaction costs, liquidity, and risk adjustment; those conditions are not tested here.

6. Limitations

Several limitations qualify these results. The UCI is an aggregate index rather than a panel of individual security returns, so index construction and changing constituents may influence the reported patterns. The official USD/UZS series presents a different measurement limitation: it is an official published rate rather than a continuous transaction-price record and may reflect administrative publication, smoothing, and institutional adjustment. The results for both series must therefore be interpreted in light of how each measure is constructed.

Market microstructure further complicates interpretation. Thin trading, stale prices, non-synchronous component trading, zero-return clustering, and low liquidity can generate measured statistical dependence without creating a feasible arbitrage opportunity. The available data contain no bid-ask spreads, order-book depth, individual trades, transaction costs, or investable trading strategies. Statistical dependence identified here therefore cannot be translated directly into economic profit.

The research design introduces additional constraints. The September 2017 devaluation is a large structural break; the pre/post comparison documents a change around that date but does not identify a causal reform effect. Results are also sensitive to the selected test, lag, horizon, frequency, and deterministic unit-root specification, and the tests are interpreted jointly without a formal family-wise error adjustment. In addition, the pre-reform weekly sample is smaller than the post-reform sample, and its runs test is degenerate because all nonzero changes have the same sign.

Finally, daily and weekly observations may miss intraday adjustment and may aggregate returns across market closures or publication timing. Conclusions at these frequencies should not be read as characterizing higher-frequency price behavior.

7. Conclusion

Uzbekistan's UCI does not support a single yes-or-no efficiency conclusion. Daily sign runs and longer-lag joint serial-correlation tests reject their respective unpredictability nulls, while robust variance-ratio tests fail to reject the random-walk null at the selected horizons. Weekly aggregation weakens sign dependence but does not remove all serial dependence. Given the index's zero returns and thin-market setting, these departures should be interpreted partly through microstructure rather than as proven profitable predictability.

The official USD/UZS series displays a clear regime difference. Strong pre-reform dependence is consistent with a more managed adjustment path. Post-reform selected autocorrelations weaken substantially when the reform jump is included. Removing exactly that return reveals short-horizon dependence in some tests, so later non-randomness is not solely a mechanical artifact of the one-time movement, although the analysis does not establish that the reform caused all subsequent dependence. Sign tests, selected variance ratios, and rolling windows still reveal non-random or time-varying adjustment. The defensible overall conclusion is partial, test-dependent, frequency-dependent, reform-dependent, and time-varying weak-form evidence, with the FX conclusions limited to official exchange-rate adjustment.

Data and Code Availability

The raw source workbooks, corrected Python pipeline, executable notebook, machine-readable tables, figures, duplicate audit, and validation report are available at <https://github.com/Foziljoniy/Research-EMP>. Raw workbooks are not overwritten. The complete analysis is executed from the repository root with `python run\_analysis.py`, followed by `python validate\_outputs.py`.

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Appendix A. Reproducibility and Validation

The validation suite checks raw-file presence, explicit ISO parsing, sorted unique dates, positive levels, finite returns, duplicate resolution, sample separation, rolling window sizes, CSV/Excel numerical equality, rejection labels, variance-ratio package equality, and the existence of all eleven figures. The final validation report records 68 of 68 checks passed.

Presentation tables display p-values below 0.0001 as '<0.0001'. Machine-readable CSV and Excel files retain raw numerical p-values, including floating-point underflow where a standard survival function returns zero.