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Renewable Portfolio Standards and Economic Performance in New Mexico

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Abstract: Objectives: This article examines whether changes in New Mexico's renewable electricity requirement were associated with statewide real output from 1997 to 2024. **Prior Work:** Studies of renewable portfolio standards emphasize generation, prices, and differences across states; New Mexico research has also modeled prospective regional effects. **Approach:** We analyze 28 annual observations using a dynamic regression with national output and state controls, small-sample bounds tests, and a lag-augmented predictive test. **Results:** The preferred model gives a negative adjustment coefficient (−0.4187; p = 0.018), but the bounds statistic (F = 3.601) is inconclusive at 10 percent and does not establish a level relationship at 5 percent. Neither the predictive nor the noncausality null is rejected. **Implications:** Statewide output alone cannot determine whether a renewable mandate improves energy affordability, employment, or environmental performance. **Value:** The analysis separates a legal policy path from actual compliance and distinguishes conditional model coefficients from evidence of a durable economic relationship.

Keywords: Policy stringency; Bounds testing; Business cycles; Predictive causality; Electricity prices

JEL Classification: C22; O40; Q42; Q48

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1. Introduction

Renewable portfolio standards require electricity suppliers to include a certain percentage of eligible renewable energy. These standards can promote renewable generation and drive investment, but they also impact procurement costs, grid infrastructure, and retail prices. Whether these effects lead to a measurable change in the statewide economy is another matter. This article explores that issue in New Mexico, where the policy started at 5 percent in 2006 and increased to 20 percent by 2020 (New Mexico Legislature, 2004, 2007).

The state presents a complex yet informative case. Its economy is influenced by renewable resources, oil and gas activities, federal spending, and the overall US business cycle. Consequently, an increase in legal requirements and gross state product may coincide without direct policy influence. We evaluate the legal requirement as an indicator of policy strictness and control for factors such as US real output, employment, electricity prices, and inflation. The study investigates whether a conditional dynamic relationship exists, whether long-term relationships are supported by appropriate bounds tests, and whether historical policy levels can forecast output in a small vector autoregression.

The contribution is a focused, historical assessment of a state policy path rather than a claim to identify a causal treatment effect. We prominently report the inconclusive parts of the evidence. In particular, a negative estimated adjustment coefficient is not sufficient to establish a stable long-run relationship. These distinctions matter for economic policy: total state output is informative, but it does not capture electricity affordability, emissions, investment quality, or how benefits and costs are distributed across communities.

2. Related Evidence and Economic Channels

The design of a renewable portfolio standard remains important after adoption. Factors like stringency, eligible resources, and implementation rules affect deployment, with results varying by state and technology (Carley, 2009; Carley et al., 2018; Deschênes et al., 2023). While mandates can boost local construction and operations, electricity procurement and grid integration influence prices. Upton and Snyder (2017) analyze these effects, and Barbose et al. (2015) emphasize considering costs alongside benefits. These insights do not show a consistent effect on statewide output.

Cross-border electricity trade and renewable credits can influence economic impacts beyond the state where a target is set (Hollingsworth & Rudik, 2019). New Mexico-specific modeling shows that economic and environmental results can differ significantly between counties, even when overall state-level effects are similar (Mamkhezri et al., 2021). This study builds on those findings by analyzing actual output and the historical policy timeline. Because investment benefits and compliance costs may counterbalance each other, the direction of their relationship isn't predetermined. Implementing a national-cycle control is crucial to avoid confusing general growth with policy-specific effects.

3. Data and Policy Measurement

The sample contains 28 annual observations (1997–2024); the selected dynamic regression uses 27 observations after lagging. Real gross state product comes from the Bureau of Economic Analysis via FRED (Federal Reserve Bank of St. Louis, n.d.-d); employment comes from the Bureau of Labor Statistics via FRED (Federal Reserve Bank of St. Louis, n.d.-c); population comes from the U.S. Census Bureau (n.d.); electricity prices and renewable generation come from the U.S. Energy Information Administration (n.d.); and inflation and U.S. real output come from FRED (Federal Reserve Bank of St. Louis, n.d.-a, n.d.-b). Nominal electricity prices are deflated using the Consumer Price Index. Monetary output series are expressed in chained 2017 dollars. Table 1 defines the variables and sources.

Table 1. Variables Used In The Preferred Annual Model

Variable	Measurement	Data source
State output	ln(real gross state product), millions of chained 2017 dollars	BEA/FRED (NMRGSP)
RPS requirement	Statutory renewable share (%)	New Mexico legislation
Employment	ln(total nonfarm employment), thousands	BLS/FRED (NMNA)
Electricity price	ln(retail electricity price), cents/kWh	U.S. EIA state data
Inflation	Annual change in CPI-U (%)	BLS/FRED (CPIAUCSL)
National output	ln(U.S. real GDP), billions of chained 2017 dollars	BEA/FRED (GDPCA)

Note: ln denotes the natural logarithm; RPS, renewable portfolio standard; CPI-U, Consumer Price Index for All Urban Consumers; BEA, Bureau of Economic Analysis; BLS, Bureau of Labor Statistics; and EIA, Energy Information Administration.

The policy variable indicates the required share for public utilities, not compliance by all electricity providers. The 2004 Renewable Energy Act mandated 5% in 2006, increasing by 1% annually until 10% in 2011. The 2007 amendment set milestones of 10% in 2011, 15% in 2015, and 20% in 2020 (New Mexico Legislature, 2004, 2007). The historical series uses this ramp: 10% in 2011–2014, 15% in 2015–2019, and 20% in 2020–2024, with a mean of 8.929%. It serves as a policy proxy, not annual compliance. The 2019 Energy Transition Act sets future targets beyond the observed period (New Mexico Legislature, 2019).

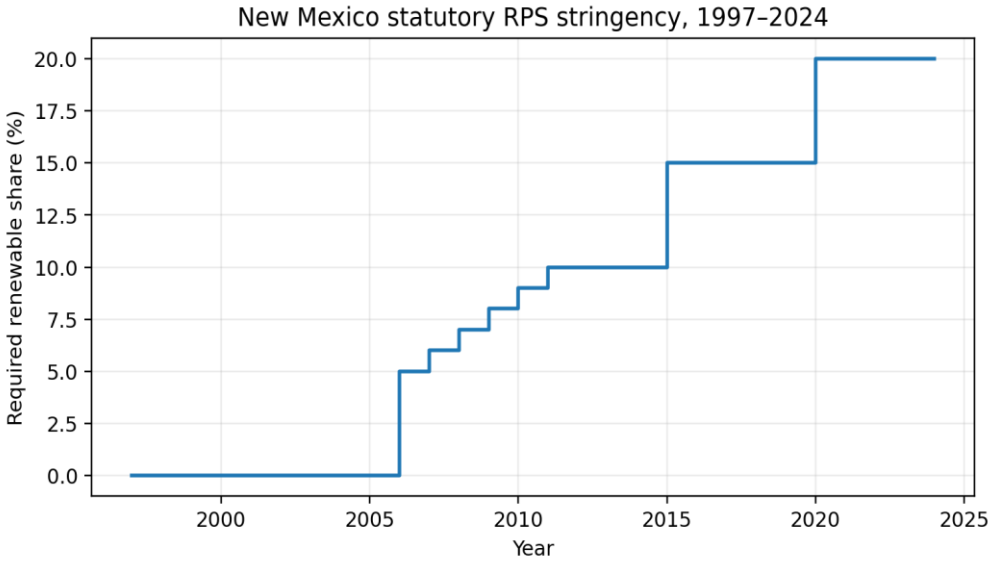


Figure 1. New Mexico Statutory RPS Stringency from 1997 to 2024

Source: New Mexico statutory schedule in the audited dataset

Table 2 summarizes the measured variables in their original units. Over these 28 years, the statutory target ranges from zero to 20 percent, and real state output increases across the sample. These descriptive trends do not identify a policy effect, since common macroeconomic conditions change at the same time.

Table 2. Descriptive Statistics, 1997–2024

Series and unit	Mean	Std. dev.	Minimum	Maximum
State output (USD million)	89,448.86	12,599.64	67,718.10	119,044.50
RPS requirement (%)	8.929	7.572	0.000	20.000
Employment (thousands)	806.014	44.813	708.500	889.100
Electricity price (cents/kWh)	8.315	1.169	6.570	10.020
Inflation (%)	2.511	1.550	−0.400	8.000
U.S. output (USD billion)	17,502.73	3,016.12	12,370.30	23,358.44

Note: N = 28 annual observations for each series. State and U.S. output are expressed in chained 2017 dollars. RPS denotes renewable portfolio standard.

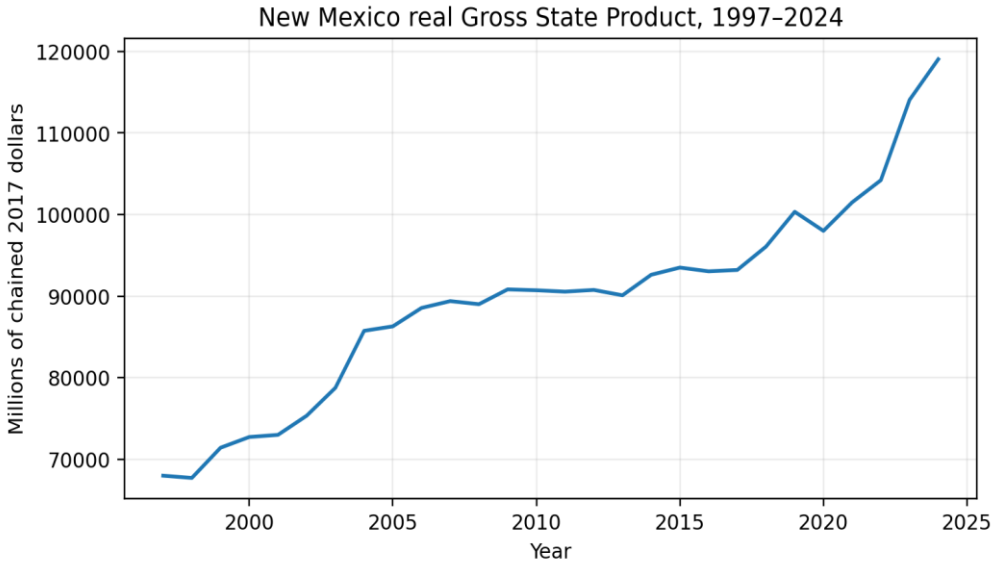


Figure 2. New Mexico Real Gross State Product from 1997 to 2024

Source: Bureau of Economic Analysis data retrieved through FRED

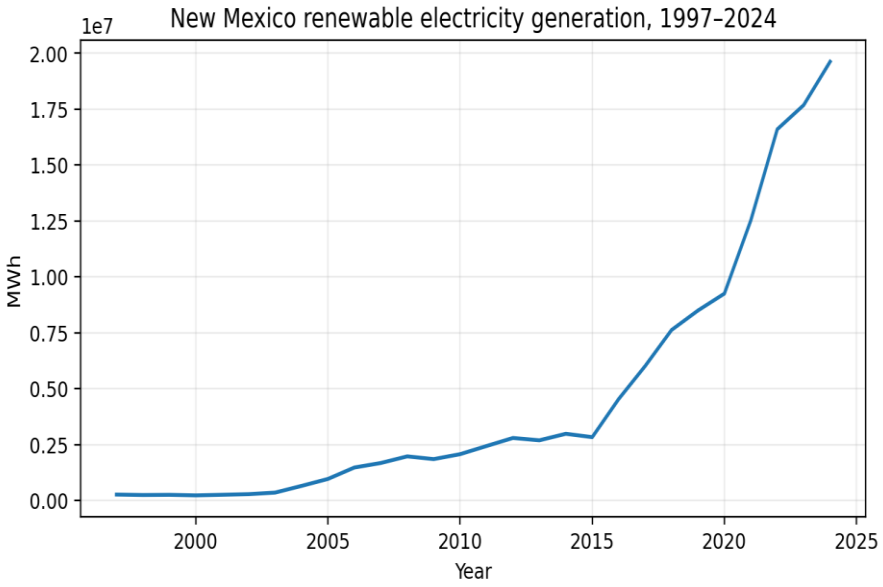


Figure 3. New Mexico Renewable Electricity Generation from 1997 to 2024

Source: U.S. Energy Information Administration state electricity data



Figure 4. New Mexico Real GDP as a Share of U.S. Real GDP from 1997 to 2024

Source: Authors' calculations using New Mexico and U.S. real GDP data from BEA/FRED

4. Econometric Strategy

4.1. Dynamic Specification and Bounds Testing

We estimate a single-equation autoregressive distributed lag model with logged state output as the dependent variable and five regressors from Table 1. The Akaike information criterion selects ARDL(1,0,1,1,1,1), using a short maximum lag to preserve degrees of freedom in a 27-observation regression. Its error-correction form reports a coefficient on lagged state output, long-run ratios, and short-run differences. We consider these ratios as descriptive unless a level relationship is independently supported.

The preferred specification models New Mexico's real output as a function of the statutory RPS stringency, employment levels, electricity prices, inflation, and real U.S. GDP, and includes a national output control to reduce the likelihood that a common U.S. business-cycle trend is attributed to state renewable policy.

$$ln(RGSP_t) = \beta_0 + \beta_1 RPS_t + \beta_2 ln(EMP_t) + \beta_3 ln(EP_t) + \beta_4 INF_t + \beta_5 ln(USGDP_t) + \varepsilon_t \ldots\ldots\ldots\ldots\ldots (1)$$

The general ARDL representation is:

$$y_t = \alpha_0 + \Sigma_{i=1}{}^{p}\phi_i y_{t-i} + \Sigma_{j=0}^{q_1}\beta_{1j} RPS_{t-j} + \Sigma_{j=0}^{q_2}\beta_{2j} lnEMP_{t-j} + \Sigma_{j=0}^{q_3}\beta_{3j} lnEP_{t-j} + \Sigma_{j=0}^{q_4}\beta_{4j} INF_{t-j} + \Sigma_{j=0}^{q_5}\beta_{5j} lnUSGDP_{t-j} + u_t \ldots\ldots\ldots\ldots\ldots\ldots (2)$$

AIC is used to determine the lag length, with a deliberately modest maximum lag because the annual dataset is small. The model chosen is ARDL (1,0,1,1,1,1).

The null hypothesis in the Pesaran–Shin–Smith procedure is that the lagged levels do not form a long-run relationship (Pesaran et al., 2001). Given the small sample, inference uses the reported finite-sample response-surface critical values for an unrestricted intercept and no trend, with five long-run forcing variables (Kripfganz & Schneider, 2020, 2023). A conclusive rejection requires the relevant statistic to exceed the upper bound; a statistic between the bounds is inconclusive. Standard bounds inference also requires that no stochastic variable be integrated of order two.

Augmented Dickey–Fuller and Phillips–Perron tests mostly support levels or first differences, but they disagree on the first difference of logged state output: the former yields p = 0.1596, whereas the latter yields p = 0.0003. We therefore cannot

claim that these tests conclusively rule out integration of order two. The statutory policy step function is interpreted as a deterministic schedule rather than assigned a meaningful stochastic unit-root order. These qualifications limit confidence in the bounds exercise and in the integration-order choice for the supplementary vector autoregression.

4.2. Predictive Tests and Diagnostics

We use a three-variable Toda–Yamamoto lag-augmented vector autoregression of logged state output, RPS schedule, and logged U.S. output. One substantive plus one extra lag form a two-lag model; Wald restrictions apply only to the first lag (Toda & Yamamoto, 1995). P-values are based on chi-square inference, not bootstrap. With 28 observations and unclear maximum integration order, we treat the test as a limited check of predictive precedence, not causality. Diagnostics include serial-correlation, variance, normality, functional-form, and stability tests, but do not validate the policy variable or exogeneity.

5. Results

5.1. Conditional Dynamic Estimates

The preferred levels regression uses 27 observations with an adjusted R-squared of 0.9844. Its lagged-output coefficient is 0.5813 ($p = 0.002$), and the contemporaneous RPS coefficient is −0.00651 ($p = 0.028$). This level coefficient depends on other regressors and lag structure; it does not estimate a treatment effect or indicate a long-run equilibrium. Table 3 shows the error-correction transformation for interpreting long-run ratios and short-run differences.

Table 3. Error-Correction Estimates For The Preferred Annual Model

Term	Estimate	Std. error	*p*-value
Adjustment: lagged ln(state output)	−0.4187	0.1582	0.018
Conditional long run: RPS	−0.01554	0.00866	0.092
Conditional long run: ln(employment)	1.1468	0.4949	0.034
Conditional long run: ln(electricity price)	0.2222	0.2314	0.351
Conditional long run: inflation	0.03524	0.01481	0.030
Conditional long run: ln(U.S. output)	0.9379	0.4335	0.046
Short run: Δln(employment)	−1.4862	0.6157	0.028

Term	Estimate	Std. error	*p*-value
Short run: Δln(electricity price)	−0.2302	0.1295	0.094
Short run: Δinflation	−0.00941	0.00329	0.011
Short run: Δln(U.S. output)	1.3987	0.4979	0.013

Note: RPS denotes renewable portfolio standard; ln denotes the natural logarithm; and Δ denotes the first-difference operator. Reported p-values are two-sided.

The adjustment estimate is negative and statistically different from zero, consistent with conditional correction toward the model's implied path. The RPS long-run ratio is −0.01554 (p = 0.092). If a stable level relationship existed, that semi-log ratio would correspond to approximately 1.55 percent lower state output for a one-percentage-point increase in the requirement, holding the model's other variables fixed. The evidence below does not support presenting that calculation as a durable or causal policy effect.

5.2. Level Relationship and Predictive Evidence

Table 4 presents the critical test for the principal claim. The F statistic of 3.601 falls within the 10 percent bounds (2.664, 4.110) and the 5 percent bounds (3.318, 5.018); standing alone, it is inconclusive at both thresholds. The t statistic of −2.647 is not sufficiently negative to reject the upper bound at either threshold. Joint inference is inconclusive at 10 percent and does not reject the null of no level relationship at 5 percent or 1 percent. Thus, no robust long-run relationship is established; a statistically significant adjustment term does not alter that conclusion.

Table 4. Finite-Sample Bounds Test For A Level Relationship

Test	Value	10% *I*(0)	10% *I*(1)	5% *I*(0)	5% *I*(1)
F	3.601	2.664	4.110	3.318	5.018
t	−2.647	−2.501	−3.809	−2.907	−4.312

Note: Unrestricted intercept and no trend; five forcing variables; 27 effective observations. I(0) and I(1) denote the lower and upper bounds, respectively.

The lag-augmented vector autoregression does not reject the null hypothesis that past RPS values add predictive content for New Mexico output after U.S. output is included ($\chi^2(1) = 0.49$; p = 0.4836). It also does not reject the reverse direction, from state output to the RPS series ($\chi^2(1) = 1.06$; p = 0.3035). These failures to reject are

consistent with weak power and a stepwise policy path; they do not demonstrate that economic effects are exactly zero.

5.3. Model Checks

Breusch–Godfrey tests do not reject the null of no serial correlation up to lag two. The Breusch–Pagan test does not reject constant error variance, the skewness–kurtosis test does not reject normal residuals, and RESET does not reject the tested functional-form restriction. The recursive CUSUM statistic remains below its 5 percent critical value. These checks have low power in a short annual sample and cannot resolve uncertainty about the integration order, omitted shocks, or the inconclusive bounds result. The vector autoregression passes stability and lag-one/two serial-correlation checks, although its joint residual normality test rejects, making asymptotic Wald p-values especially tentative.

Table 5. Diagnostic Checks For The Dynamic Model

Assessment	Statistic	*p*-value or threshold
Serial correlation, lag 1	$\chi^2(1) = 0.216$	$p = 0.6423$
Serial correlation, through lag 2	$\chi^2(2) = 0.262$	$p = 0.8772$
Breusch–Pagan variance test	$\chi^2(1) = 1.47$	$p = 0.2249$
Skewness–kurtosis normality	Adjusted $\chi^2(2) = 0.04$	$p = 0.9824$
Ramsey RESET	$F(3, 13) = 0.66$	$p = 0.5909$
Recursive CUSUM	0.6700	5% critical = 0.9479
VAR joint normality	$\chi^2(6) = 19.185$	$p = 0.00386$

Note: VAR denotes vector autoregression. The null hypothesis of joint normality in the VAR is rejected.

6. Discussion and Policy Implications

The analysis finds limited evidence that the historical requirement created a stable relationship with aggregate output once dynamic variables and national output are included. This does not mean the renewable standard had no effect, but the ability to detect one is constrained by only 28 observations, a policy changed on few dates, and factors like federal spending, fossil-fuel cycles, and shocks. The electricity-price variable may also reflect the policy itself, and including it could partially account for transmission channels rather than isolating the total effect. Similarly, employment

can be both a determinant and an outcome of renewable investment, so coefficients should be seen as conditional associations, not causal effects.

Policy assessment should track impacts on renewable procurement, electricity bills, energy burden, grid reliability, jobs, income, emissions, and benefits across counties. Statewide output may stay stable while employment and costs shift locally. Comparing legal schedules with annual data and publishing consistent records improves measurement and credibility. Evidence can reflect clean energy, jobs, infrastructure, and emissions without relying on a single gross-output statistic.

A future causal evaluation needs a credible counterfactual, like comparison states or exposure changes among utilities and sectors. It should test different lag lengths, identify breaks, audit compliance series, address mixed unit-root evidence, and use small-sample inference. Current estimates are a transparent view of one state's history and caution against interpreting long-run ratios as policy multipliers.

7. Conclusion

New Mexico's renewable electricity quota increased from 1997 to 2024, but no strong link with gross state product is shown. The regression model's negative adjustment coefficient and marginal long-run ratio are inconclusive at the 10% and 5% levels. Predictive tests do not support a clear relationship. These findings do not confirm a zero economic impact. They highlight that overall economic output is insufficient for evaluating electricity policy; actual compliance and effects on prices, investment, jobs, and emissions are needed for better assessment.

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