

Original Article

Kerosene Up, Petrol Down: Demand and Distribution after Madagascar's Thirty-Month Fuel Price Freeze

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Abstract

Between July 2022 and December 2024, the administered retail prices of the three main petroleum products sold in Madagascar were held constant in ariary for 30 months. In January 2025, the government resumed monthly adjustment. The resulting price shocks went in opposite directions across products: by September 2025, petrol had fallen by 13.7 percent and diesel by 7.6 percent, while kerosene, the lighting fuel of poor households, had risen by 39.1 percent. We use this configuration to estimate the response of deliveries to relative prices on a monthly product panel that we construct from successive issues of the national statistical office's economic bulletin, covering October 2021 to September 2025, and we are explicit about how far the design can carry a causal reading. The estimates are consistent in sign and order of magnitude across specifications: the own-price elasticity is -0.84 in first differences, -0.51 in levels with product-specific trends, and -1.52 for kerosene taken alone. We test each estimate against an exact randomization distribution built by displacing the price path in time, a device that never treats the three products as interchangeable. Two of the five estimates clear that test at its attainable floor of $p = 0.021$; the pooled levels estimate does not, at $p = 0.38$. With two price events, the design has limited power, and we report failures alongside successes. A distributed-lag specification shows that part of the monthly response is undone within two months, so the monthly elasticity is an upper bound on the sustained response; on quarterly aggregates the elasticity falls to -0.45 and loses significance. Kerosene deliveries fell

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17.9 percent. Household survey data collected between May 2021 and April 2022, the *Enquête Permanente auprès des Ménages* of 2021-2022, published in March 2024, show that 42.6 percent of the poorest quintile and 45.3 percent of the second quintile light their homes with kerosene lamps, compared with 10.7 percent of the richest quintile. That gradient would have to close by three-quarters before the poorest quintile ceased to be the more exposed group, so the distributional conclusion does not depend on the elasticity estimates. The reform raised the price of fuel for the poor and lowered the price of fuel for the rich.

Keywords: administered prices, demand elasticity, energy poverty, fuel subsidy reform, Madagascar

1. Introduction

Fossil fuel subsidies remain one of the largest and least well-targeted fiscal instruments in the developing world. Coady et al. (2017) place global fossil fuel subsidies, measured relative to supply and environmental costs, on a scale that dwarfs public health spending in most low-income countries. The distributional case against them is equally established: reviewing incidence studies across developing countries, Arze del Granado et al. (2012) find that the top income quintile captures roughly six times more subsidy value in absolute terms than the bottom quintile, and that a reduction of USD 0.25 per liter costs households about five per cent of income, with more than half of that impact arriving indirectly through the prices of other goods.

Incidence is not uniform, and the literature knows it. Rentschler (2016) shows for Nigeria that uncompensated subsidy removal raises the national poverty rate by three to four per cent, and that a uniform national cash transfer fails to protect households against the price shock in sixteen of thirty-seven states, so that only regionally differentiated compensation restores pre-reform poverty rates. Gangopadhyay et al. (2005) pose the same question for household fuels in India, where kerosene has long been the subsidized product of record.

That literature has a blind spot, and it is a data blind spot rather than a conceptual one. Its incidence estimates are almost always simulations built on pre-reform household budget data and an assumed pass-through, because actual reforms are rarely observed with the price and quantity detail required to measure what households did. The reason they are not so observed is the counterfactual problem: fuel prices move for many reasons at once, world crude prices, exchange rates, tax changes and political cycles, and a single national time series offers no comparison group. What is needed is a setting in which several fuel prices move by different amounts at the same moment,

and in which quantities are recorded product by product. Madagascar between 2021 and 2025 is such a setting.

Madagascar between 2021 and 2025 offers an unusually clean case. The state sets retail prices for petrol, kerosene, and diesel administratively. From July 2022 to December 2024, it held all three at exactly 5,900, 2,430, and 4,900 ariary per liter for 30 months, through a period in which Brent crude moved by more than 30 dollars and the ariary depreciated steadily. Price variance over that window was not small; it was zero. In January 2025, monthly adjustment resumed, and the three products moved in different directions and by very different magnitudes.

This configuration supplies the two ingredients that incidence work usually lacks. The thirty-month freeze is a pre-period in which the treatment variable is constant by construction, making it a stronger placebo window than any parallel-trends assumption can normally provide. And because the post-reform price shocks differ in sign across products within the same country and month, aggregate shocks common to all fuels, income, seasonality, the exchange rate, and the world oil price are absorbed by month fixed effects, and the comparison rests only on relative price movements. It supplies them, however, in a setting with three products and two price events. Section 4 states what that buys and what it does not, and Section 5.3 reports the tests that decide the question rather than asserting an answer to it.

We assemble a monthly panel of prices and delivered volumes for the three products from October 2021 to September 2025 by digitizing successive issues of the *Tableau de Bord Économique* published by Madagascar's national statistical institute. These bulletins are not held in any international database; each issue covers 24 rolling months, so chaining two issues yields 47 matched product-month observations. To our knowledge, this is the first use of this source for econometric work.

Three findings follow. First, deliveries respond to relative prices, and by more than the international literature would predict. The own-price elasticity is -0.84 in first differences and -0.51 in levels with product-specific trends; the January 2025 episode taken alone gives -0.93 . By comparison, the meta-analysis of Havranek et al. (2012), which corrects the gasoline literature for publication selection bias, estimates the mean short-run price elasticity at -0.09 and the long-run elasticity at -0.31 . The estimates are mutually consistent, but their statistical support is uneven. Under an exact randomization test that displaces the price path in time, the first-difference estimate is the most extreme of forty-seven possible alignments. In contrast, the pooled levels estimate ranks eighteenth and cannot be distinguished from a placebo.

Second, the response is concentrated in kerosene, whose elasticity we estimate at -1.52 in levels and -0.87 in first differences. Kerosene deliveries fell by 17.9 percent between 2024 and the first nine months of 2025, while petrol and diesel volumes rose. Kerosene is elastic, whereas gasoline is not, because close substitutes exist and are diffusing: battery lamps and solar panels are already the main lighting sources for a third and a fifth of Malagasy households, respectively.

Third, the incidence is regressive. Household survey evidence shows that 42.6 percent of households in the poorest quintile and 45.3 percent in the second quintile

use kerosene lamps as their main lighting source, compared with 10.7 percent in the richest quintile, where grid electricity accounts for 48.2 percent. The products whose prices rose are the poor's; the products whose prices fell are the rich's. Since kerosene demand is elastic and substitution runs towards battery lamps and darkness rather than towards electricity for households without a connection, the welfare loss is understated by the expenditure change alone.

The paper contributes to three literatures. It adds a rare quasi-experimental estimate of fuel demand elasticities in a low-income African setting, where almost all existing estimates are simulated rather than measured. It documents the political economy of partial reform: in July 2022, the state raised petrol and diesel prices by roughly 44 percent while protecting kerosene by 14 percent, and in January 2025, it reversed that protection. It also introduced a national statistical source that supports monthly macroeconomic work in a country where the quarterly national accounts have not been published since the fourth quarter of 2022.

2. Methodology

2.1 Setting

Madagascar imports all of its refined petroleum products. Retail prices are set administratively by the state, with the *Office Malgache des Hydrocarbures* operating a price structure that combines an import-parity component, a specific excise, value-added tax, and regulated distribution margins. In principle, the structure is adjusted periodically; in practice, adjustment has been episodic and politically timed.

The country's macroeconomic context makes the stakes unusually high. Poverty affected 75 percent of the population in 2022, 95.2 percent of employment is informal, and the Gini coefficient is 0.368. Households devote 65.8 percent of consumption to food and beverages and 17.0 percent to housing, water, electricity, gas, and other fuels, making energy the second-largest budget item after food (INSTAT, 2024a). Only 15 percent of households have a grid connection.

Our sample covers two price events. In July 2022, under an International Monetary Fund program that made price adjustment a structural benchmark, the state raised petrol from 4,100 to 5,900 ariary per liter, a rise of 43.9 percent, and diesel from 3,400 to 4,900, a rise of 44.1 percent, while kerosene went from 2,130 to 2,430, a rise of only 14.1 percent. The asymmetry was deliberate and was justified publicly by the protection of poor households.

Prices then froze. From July 2022 to December 2024, the three figures remained unchanged. Over those thirty months the implicit subsidy accumulated silently, since world prices and the exchange rate continued to move while the domestic price did not.

In January 2025, adjustment resumed and has continued monthly since. The direction of movement differs sharply by product. Petrol fell from 5,900 to 5,090 ariary by September 2025, a decline of 13.7 percent; diesel fell from 4,900 to 4,530, a decline

of 7.6 percent; kerosene rose from 2,430 to 3,380, a rise of 39.1 percent. By October 2025, kerosene had reached 3,470, up 42.8 percent from the frozen level. What was presented as a technical resumption of the pricing formula was, in its effects, a reallocation of the cross-subsidy away from the lighting fuel and towards transport fuels.

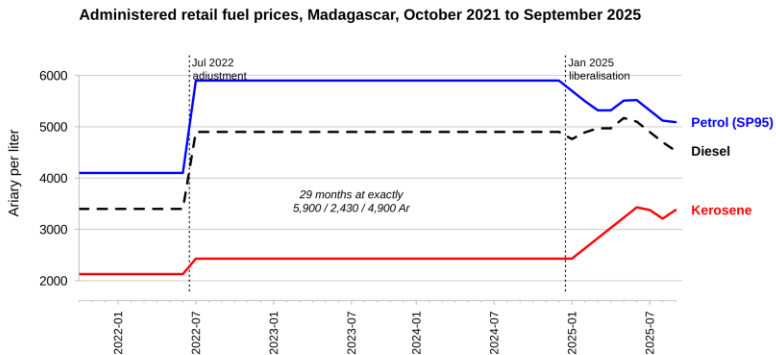


Figure 1. Administered retail prices of the three main petroleum products, Madagascar, October 2021 to September 2025.

Source: authors' compilation from INSTAT (2024b, 2025), *Tableau de Bord Économique*, issues 53 and 60, Table 20. Prices are monthly weighted-average retail prices in ariary per liter.

2.2 Data

Our source is the *Tableau de Bord Économique*, a quarterly bulletin published by the Institut National de la Statistique (INSTAT, 2024b, 2025). Each issue reports twenty-four rolling months of monthly indicators. We digitize Table 20, the monthly weighted-average retail prices of petroleum products, and Table 19, the monthly volumes delivered to the domestic market in cubic meters, from issue 53 of January 2024 (INSTAT, 2024b) and issue 60 of October 2025 (INSTAT, 2025).

Chaining the two issues yields prices for forty-eight months from October 2021 to October 2025, with one gap in October 2023, and volumes for forty-eight months from October 2021 to September 2025, with no gap. Matching prices to volumes yields a panel of forty-seven months by three products, balanced across products, or 141 product-month observations.

Two features of the volume series deserve comment. The first is the one that matters most for interpretation. The series records *mises à la consommation*, volumes released to the domestic market by importers and distributors, not liters bought by households. Between the two lies a distribution chain that holds stocks, and stocks respond to expected price changes as well as to demand. A distributor who anticipates an increase has a reason to draw down inventory before it and rebuild afterward, which would displace the measured response in time and exaggerate its amplitude at the

monthly frequency. Three considerations bound the problem. Administered price changes in Madagascar are announced with little lead time, which limits the scope for anticipation; the January 2025 kerosene price was in fact unchanged, and the increases began only in February, so there was no pre-announced date around which to position stocks. Storage of illuminating kerosene is constrained because the product accounts for a small share of depot capacity and its turnover is rapid relative to the monthly frequency we observe. And the question is testable rather than rhetorical: if the monthly movements were stock adjustments, they would reverse within one or two months, and the cumulative response would be much smaller than the impact response. Section 5.4 runs that test, and reports a result that is partly unfavorable to a pure consumption reading. We therefore treat the monthly elasticity as an upper bound on the sustained response throughout, and report quarterly estimates alongside the monthly ones. And kerosene here is illuminating kerosene, a small and distinct category averaging about 5,750 cubic meters a month, separate from jet fuel; petrol and diesel average about 16,000 and 59,000 cubic meters, respectively.

For the incidence analysis, we use the *Enquête Permanente auprès des Ménages 2021-2022* (INSTAT, 2024a), the first nationally representative household survey conducted in Madagascar since 2010.

Its fieldwork ran from May 2021 to April 2022 and its results were published in March 2024, so the exposure rates it reports predate the January 2025 reform by roughly three years. We take the distribution of the main lighting source by welfare quintile, as reported in Table 10.21. Section 6 quantifies how much that gap would have to matter before it changed any conclusion

Table 1. Price regimes and mean monthly volumes, by product.

	Months	Petrol	Kerosene	Diesel
Price, Ar/litre, Oct 2021 to Jun 2022	9	4,100	2,130	3,400
Price, Ar/litre, Jul 2022 to Dec 2024	30	5,900	2,430	4,900
Price, Ar/litre, September 2025		5,090	3,380	4,530
Change at the July 2022 adjustment		+43.9 %	+14.1 %	+44.1 %
Change, December 2024 to September 2025		-13.7 %	+39.1 %	-7.6 %
Mean volume, m ³ per month, 2024	12	15,950	5,753	58,953
Mean volume, m ³ per month, Jan to Sep 2025	9	17,085	4,722	60,698
Change in volume		+7.1 %	-17.9 %	+3.0 %

Source: authors' calculations from INSTAT (2024b, 2025), *Tableau de Bord Économique*, issues 53 and 60, Tables 19 and 20.

2.3 Empirical strategy

We estimate a constant-elasticity demand equation on the product-month panel:

$$\ln Q_{it} = \alpha_i + \lambda_t + \gamma_i \cdot t + \beta \ln P_{it} + \varepsilon_{it}$$

where i indexes the three products and t the month. Product fixed effects α_i absorb permanent differences in market size. Month fixed effects λ_t absorb everything common to all three products in a given month: aggregate income, the exchange rate, the world crude price, seasonality and the general price level. Product-specific linear trends $\gamma_i \cdot t$ allow the three markets to grow at different rates.

The comparison, therefore, rests entirely on the differential movements of relative prices across products within a month. This is the differences-in-differences logic applied to a setting in which the treatment is continuous and its sign varies across units.

We state the identifying assumption plainly, because a referee of an earlier version was right to press on it. What β requires is not that prices be exogenous to households, which they are, but that the administration's choice of how much to move each product's price be unrelated to product-specific demand shocks occurring at the same time. That is a strong assumption and it is not implied by the administered character of the price. The July 2022 asymmetry was an explicit political decision to protect kerosene; a decision of that kind could in principle have been taken because kerosene demand was moving, in which case the correlation we measure would confound the response with its cause. Month fixed effects and product trends cannot remove a product-specific shock that is contemporaneous with a product-specific price decision.

Three considerations bear on the assumption, and only the third is a test. Prices are administered rather than market-clearing, so the price a household faces in month t is not set by that household's demand. The pre-period contains 29 months of zero price variation, so any spurious correlation must arise entirely from the two adjustment episodes rather than accumulating over the sample. And the pricing rule leaves a testable trace: an administration adjusting each product in response to how that product's deliveries had just moved would generate a relationship between a product's price change and its recent quantity changes. Regressing the monthly change in log price on two lags of the change in log quantity, with product and month fixed effects, gives coefficients of -0.021 ($t = -0.98$) and -0.039 ($t = -1.63$) over the full sample, and 0.050 ($t = 0.78$) and -0.063 ($t = -1.15$) over the months in which the price was actually being adjusted. None differs from zero at conventional levels. This is evidence, not proof: it rules out mechanical feedback from recent deliveries, and it does not rule out a decision taken on information we do not observe. Readers who find the assumption implausible should read what follows as a description of how deliveries and relative prices moved together, a weaker claim that the data support on their own terms.

Two features of the estimand deserve statement. Our treatment is continuous rather than binary, and the recent literature shows that two-way fixed effects estimators recover a weighted average of underlying effects whose weights need not be positive when treatment intensity and timing vary across units (de Chaisemartin and D'Haultfœuille, 2020; Callaway et al., 2024). With three units, two treatment dates,

and a treatment that changes sign across products, we cannot implement the group-time decompositions proposed in those papers. We therefore read β as an average of product-level responses over the observed range of price variation rather than as a structural parameter, and we report the product-specific coefficients precisely because the pooled estimate averages heterogeneous responses.

The product-specific trends are not a cosmetic addition. During the frozen window, twenty-nine of whose thirty months appear in our source, and with prices constant in the ariary, deliveries grew at 0.75 percent per month for petrol, 2.14 percent for kerosene, and 0.21 percent for diesel, and the first two coefficients are significant. A specification without trends would attribute part of the reversal of the kerosene trend to the price change. We therefore report both, and treat the trend-augmented specification as the baseline.

We complement the levels specification with a first-difference version, in which product fixed effects correspond to product-specific trends in levels:

$$\Delta \ln Q_{it} = \alpha_i + \lambda_t + \beta \Delta \ln P_{it} + \varepsilon_{it}$$

Inference requires care. With three products, cluster-robust standard errors are not reliable; the asymptotics that justify them require many clusters, and the small-cluster problem is well documented (Cameron et al., 2008; Conley and Taber, 2011; Ferman and Pinto, 2019). We report heteroskedasticity- and autocorrelation-consistent standard errors with a Newey-West kernel of six lags, and we treat them as descriptive rather than as the basis for any claim.

The first version of this paper based its inferential claim on a randomization test that permuted the three observed prices across the three products within each month. That test was not valid and we withdraw it. Its null requires the three products to be exchangeable, and they are not: petrol, illuminating kerosene and diesel have different users, different market sizes, different policy treatment and, as Section 2 records, a deliberately different price history. No assignment mechanism would have given petrol's price to kerosene. The permutation distribution such a test generates is also artificially narrow, with a standard deviation of 0.027 compared to 0.512 for the device we now use, so it understated the spread of the null by a factor of 19 and produced a p-value below 0.0002 that meant nothing. We are grateful for the correction.

We replace it with a randomization device that never permutes product identity. Each product keeps its own price series; what is randomized is the alignment of that series with calendar time, via a circular shift of the entire price path. Under the sharp null that prices do not move quantities, every alignment of the price path with the quantity data is as likely as the observed one, and the shift preserves each product's own price history exactly. All forty-seven alignments are enumerated, so the test is exact and its smallest attainable two-sided p-value is $1/47 = 0.021$. This is the in-time placebo logic of Bertrand et al. (2004) implemented as an exact test rather than a simulation, and it belongs to the family of randomization procedures whose power to

overturn conventional significance Young (2019) documents across a large body of experimental work. It is conservative: because the price path is a step function and the quantity series have low-frequency movements of their own, a displaced step can fit those movements by chance, which widens the null distribution considerably.

Finally, we run a placebo. Taking only the frozen months, we assign each product the price shock it would receive in 2025, set a fake reform date to October 2023, and re-estimate. Since prices did not move during that window, a significant coefficient would indicate that the products destined for large price increases were on differential paths for reasons unrelated to price.

3. Results

3.1 Elasticities

Table 2 reports the estimates, each with the exact randomization p-value described in Section 4 alongside the conventional standard error, so that the two can be compared line by line. We lead with the first-difference specification, which the randomization test supports. First differencing yields a pooled elasticity of -0.84 , and the observed value is the most extreme among the 47 possible price-path alignments, yielding the smallest p-value the test can produce. The levels specification with product-specific trends yields -0.51 and is unchanged relative to the specification without trends, but it ranks eighteenth of forty-seven under the randomization test and cannot be distinguished from a displaced-price placebo. We report it because it is the natural levels counterpart, not because it carries evidential weight.

Table 2. Own-price elasticity of demand for petroleum products, Madagascar, October 2021 to September 2025.

Specification	Pooled	Petrol	Kerosene	Diesel
(1) Levels, product and month FE	-0.508^{**} (0.227)			
(2) Levels, plus product trends	-0.508^{**} (0.248)			
(3) Levels, product-specific, with trends		-0.120 (0.266)	-1.523^{***} (0.459)	-0.076 (0.277)
(4) First differences, product and month FE	-0.835^{***} (0.194)			
(5) First differences, product-specific		-1.042^{***} (0.107)	-0.872^{**} (0.372)	-0.623^{***} (0.110)
(6) January 2025 episode only, with trends	-0.930^{***} (0.322)			
Observations	141			
Observations, first differences	135			

Note: Dependent variable: log volume delivered. Newey-West standard errors with six lags in parentheses. The last column reports the two-sided p-value from the exact randomization test of Section 4, which enumerates all 47 circular displacements of the price path over time, along with the rank of the observed estimate among those 47. The smallest attainable p-value is $1/47 = 0.021$. Conventional stars

are deliberately omitted: with three products, the sampling distribution behind them is not trustworthy, and the exact column is the inferential statement. The p-value for the kerosene column of specification (5) refers to the kerosene coefficient. Specification (6) is estimated from January 2024 to September 2025, with 63 observations, and the quarterly specification (7) is based on 15 quarters.

Source: authors' estimates.

Product-specific estimates locate the response. In levels with trends, kerosene has an elasticity of -1.52 and is the only product distinguishable from zero; petrol and diesel are small and imprecise. The kerosene coefficient is also the most extreme of the forty-seven alignments, so it clears the exact test at its floor. In first differences, the three coefficients are -1.04 for petrol, -0.87 for kerosene, and -0.62 for diesel, and the kerosene coefficient ranks third out of forty-seven, giving $p = 0.064$, which is suggestive rather than conclusive. The two specifications bracket the kerosene elasticity between roughly -0.9 and -1.5 .

Restricting the sample to the January 2025 episode gives -0.93 . The July 2022 adjustment, on its own, yields an imprecise positive coefficient, as one should expect: all three prices rose together on that date, so the month fixed effect absorbs nearly all of the variation, and only the modest differential between the 14 percent kerosene rise and the 44 percent rise elsewhere remains. The 2025 episode, with its opposing signs, carries almost all the identifying content.

These magnitudes are large compared with those reported in the international literature. Havranek et al. (2012) report a corrected mean short-run gasoline elasticity of -0.09 and a long-run elasticity of -0.31 , and argue that published estimates are inflated twofold by publication selection. Our estimates exceed even the uncorrected consensus.

Four features of the Malagasy economy, rather than of our method, account for the gap, and they compound rather than merely coexist.

The first is the budget share. Households in Madagascar devote 65.8 percent of their consumption to food and 17.0 percent to housing, water, electricity, gas, and other fuels (INSTAT, 2024a). In a budget with that shape, there is no discretionary margin from which to absorb a 39 percent increase in the price of a necessity; the adjustment has to fall on the quantity of the good itself. In a high-income country, motor fuel is a small share of the budget and a price rise is absorbed rather than accommodated, which is exactly why the measured gasoline elasticity is small.

The second is that lighting, unlike commuting, has an extensive margin that incurs no cost to use. A household that cannot afford kerosene does not relocate or change jobs; it lights fewer rooms for fewer hours, or stops lighting altogether. Demand for a service that can simply be foregone is more elastic than demand for one embedded in a fixed daily routine.

The third is the substitute set, which in Madagascar is unusually close. Battery lamps are already the main lighting source for 33.9 percent of households, and solar panels for 21.8 percent, so the alternative to kerosene is a device many neighbors already own rather than a technology to be discovered. The diffusion of low-cost solar

kits in rural sub-Saharan Africa, studied experimentally by Grimm et al. (2017) in Rwanda, widened exactly this margin over the period we observe. Gasoline in high-income countries has no comparable short-run margin, since the substitute for a car is a vehicle stock or a transport network that does not exist in response to demand.

The fourth is the size of the shock. The kerosene price rose by 39 percent over nine months. Elasticities estimated from large discrete changes need not equal those estimated from the marginal variation that characterizes most of the meta-analytic literature, and, where a threshold of affordability is crossed, the response can be discontinuous. This is a reason to read our estimate as specific to a large shock in a poor economy rather than as a revision of the global consensus.

3.2 Dynamics

Figure 2 shows the volume series indexed to each product's 2024 mean, and, in the lower panel, kerosene relative to the average of petrol and diesel, which is the difference-in-differences object.

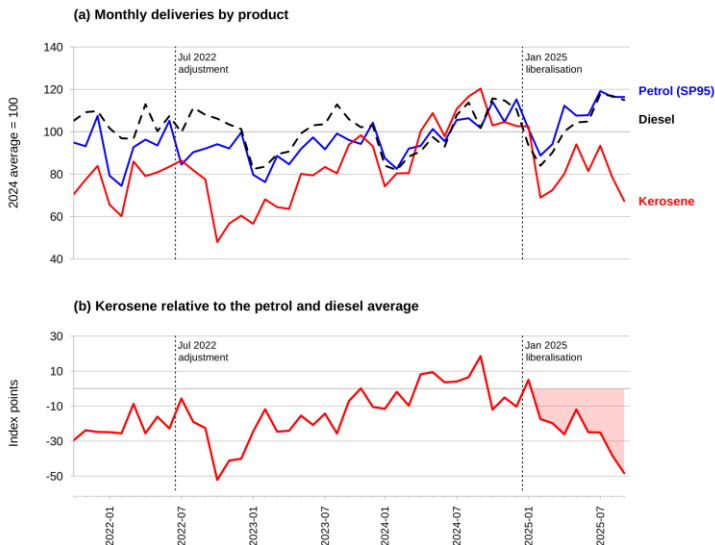


Figure 2. Monthly deliveries by product and the kerosene differential.

Note: monthly deliveries indexed to each product's own 2024 mean. Panel (b): kerosene index minus the mean of the petrol and diesel indices, in index points; shading marks the post-liberalization period.

Source: authors' calculations from INSTAT (2024b, 2025), Tableau de Bord Économique.

The lower panel makes both the pre-trend and the treatment effect visible. Relative kerosene consumption climbs steadily from late 2022, when the July adjustment left kerosene cheap relative to the other two, peaking at about 18 index

points above the comparison group in September 2024. It then falls back sharply from February 2025 and stands roughly 45 index points below the comparison group by September 2025. The reversal begins in the second month after liberalization, consistent with kerosene prices remaining unchanged in January and rising only from February.

The pattern is a mirror image. The state made kerosene relatively cheap in 2022 and consumption rose for two years; it removed that advantage in 2025 and consumption fell back within seven months. This is what an elastic demand with slow adjustment looks like, and it is also why the specification without product trends would misattribute part of the earlier climb.

3.3 Placebo and randomization inference

The placebo passes. Assigning the 2025 price shocks and a fake October 2023 reform date to the frozen window yields a coefficient of 0.303 with a standard error of 0.213 and a t-statistic of 1.43, which is not significant at conventional levels. The trend-augmented specification, therefore, does not manufacture treatment effects from the differential paths that exist during the freeze.

The exact randomization test gives a more mixed verdict, and we report it in full. Displacing the whole price path in time and enumerating all forty-seven alignments produces a null distribution centered on zero but wide: its standard deviation is 0.51 in levels and 0.37 in first differences, and the most extreme placebo reaches +0.85 in levels. That width is the honest measure of how much a design with two price events can establish.

Against that distribution, the first-difference pooled elasticity of -0.835 is the most extreme of the forty-seven values, giving an exact two-sided p of 0.021, the smallest the test can return. The kerosene elasticity in levels, -1.523 , is likewise the most extreme, at $p = 0.021$. The kerosene coefficient in first differences, -0.872 , ranks third, at $p = 0.064$. The pooled levels elasticity of -0.508 ranks eighteenth, at $p = 0.383$, and is therefore indistinguishable from what a displaced price path would produce by chance.

We also applied the device to a reduced form that puts no price on the right-hand side at all, a binary post-January-2025 window interacted with the kerosene indicator. The point estimate is -0.299 with a Newey-West standard error of 0.107, implying a relative fall of 25.8 percent, and it is stable at -0.283 for quarterly totals. Randomizing the date of the break across the thirty-five candidate months, however, places the true date eighth, with $p = 0.229$. The magnitude is substantial and the statistical support is weak, and both statements belong in the paper.

The reading we take from this is neither that the effect is established nor that it is absent. Two of five estimands meet the exact test at its attainable floor, one is marginal, and two do not meet it. A design with three products and two price events has little power against a conservative null, and a test whose smallest possible p -value is 0.021

cannot deliver more. We therefore present the estimates as a coherent set of magnitudes with partial statistical support, and we place the weight of the paper's conclusion on the incidence result of Section 6, which does not depend on them.

3.4 Deliveries, stocks and the durability of the response

Section 3 flagged that our dependent variable is deliveries to the domestic market rather than household purchases. If the monthly movements were distributors adjusting stock levels in response to price changes, they would reverse quickly, and the cumulative response would be far smaller than the impact response. We test this directly with a distributed lag of the change in log quantity on the contemporaneous change in log price and its first two lags.

The result does not fully support a pure consumption reading, and we report it as we found it. The impact coefficient is -0.394 with a standard error of 0.143 . The coefficient at one month is $+0.304$ with a standard error of 0.113 , of opposite sign and of comparable size, and the coefficient at two months is -0.020 and indistinguishable from zero. The cumulative response of the level of deliveries to a permanent price change, the sum of the three coefficients, is -0.110 . Roughly three quarters of the impact movement is undone within a month.

Two readings are consistent with that pattern and we cannot separate them with these data. One is the inventory story: distributors overshoot in the month of a price change and correct afterward, so the monthly elasticity measures the timing of releases from depots rather than consumption. The other is household stockholding: a household that keeps a few liters of kerosene defers purchase when the price jumps and resumes when the shock is absorbed, which is a genuine consumption response displaced in time. Both imply the same correction, namely that the monthly elasticity overstates the sustained response.

Aggregating to quarters is the natural check, since a stock movement within a quarter nets out while a change in consumption does not. For quarterly totals, the level elasticity falls from -0.51 to -0.452 , with a standard error of 0.358 , and the reduced-form kerosene differential is -0.283 , compared with -0.299 monthly. The reduced form is essentially unchanged by aggregation, which is what a durable change in consumption would produce; the elasticity loses what precision it had, which is what a design with fifteen quarters would produce whether or not the effect is real.

We therefore state the bound rather than assert the interpretation. The monthly elasticities in Table 2 are an upper bound on the sustained response, in absolute value. The persistent component is better measured by the quarterly reduced form, a relative fall in kerosene deliveries of about 25 percent, and by the raw comparison of annual means, a fall of 17.9 percent between 2024 and the first nine months of 2025. Those magnitudes are large enough to carry the argument of Section 6 even if the monthly elasticity is halved.

3.5 Distributional incidence

Who consumes the product whose price rose? Figure 3 answers from the household survey.

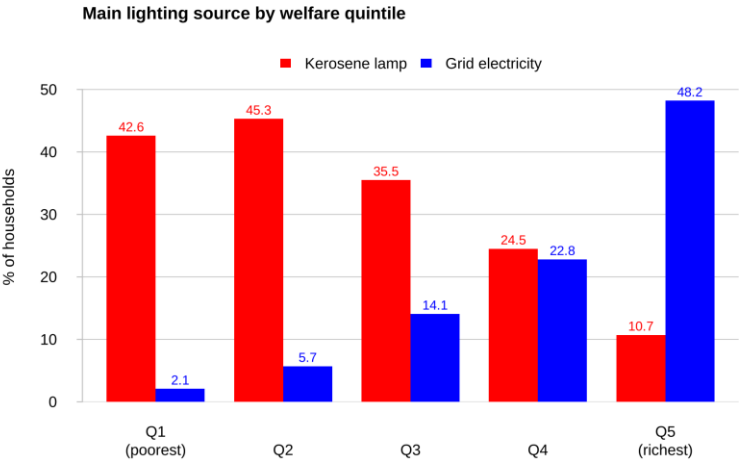


Figure 3. Main lighting source by welfare quintile, Madagascar, 2021-2022.

Source: INSTAT (2024a), *Enquête Permanente auprès des Ménages 2021-2022*, Table 10.21. Percentages of households by main lighting source.

The gradient is steep and it runs the wrong way for the reform. Kerosene lamps are the main lighting source for 42.6 percent of households in the poorest quintile and 45.3 percent in the second, falling to 24.5 percent in the fourth and 10.7 percent in the richest quintile. Grid electricity moves in the opposite direction, from 2.1 percent in the poorest quintile to 48.2 percent in the richest. Petrol, whose price has fallen, is consumed through private vehicle ownership, which, in this setting, is concentrated at the top of the distribution.

The reform thus raised the price of a good used by more than four in ten poor households and lowered the price of goods used disproportionately by the rich. The direction of the incidence is unambiguous even without household-level expenditure data.

The magnitude can be bounded. Kerosene deliveries averaged 5,753 cubic meters per month in 2024, equivalent to 5.75 million liters. At the peak increase of 1,040 ariary per liter, the transfer from kerosene users amounts to about 6.0 billion ariary a month, some 1.3 million United States dollars, or roughly 72 billion ariary a year, 15.5 million dollars, evaluated at pre-reform quantities. This is a Slutsky upper bound on the first-order welfare loss, since households reduced consumption.

That reduction is itself the concerning result. An elasticity between -0.9 and -1.5 means the response was more than proportional, and the observed volume fell by 17.9 percent. For a household in the poorest quintile with no grid connection, the margin of adjustment is not a cheaper fuel but less light: fewer lit hours, or a switch to battery lamps whose running cost per lumen-hour is not obviously lower. Substitution towards the grid is not a realistic short-run margin for these households. Lee et al. (2020), randomizing grid connection prices in rural Kenya, find that demand for connections falls sharply with price and that newly connected households consume very little electricity, so that even where the grid is physically present the welfare gain from connecting is small. In Madagascar, only 2.1 percent of the poorest quintile have a connection at all. Lam et al. (2012) document that kerosene lighting is itself hazardous, so the health consequences of substitution away from it are ambiguous in sign, but the consequences of substitution towards darkness for study time and household production are not.

Two caveats bound the incidence claim. Our volumes are national deliveries, not household purchases, so we cannot decompose the aggregate response into changes by quintile; the direction of incidence relies on combining an aggregate quantity response with a survey-based exposure gradient.

The second caveat is the three-year gap between the survey fieldwork, which ended in April 2022, and the January 2025 reform. Solar diffusion continued over those three years, so the exposure rates we use overstate 2025 exposure in levels. The question is whether they overstate it enough to matter, and that question has an arithmetic answer rather than a rhetorical one.

What the incidence argument needs is not the level of kerosene use in the poorest quintile but the ordering: that the poor are more exposed than the rich. For that ordering to fail, kerosene use in the bottom quintile would have to fall below kerosene use in the top quintile. Holding the richest quintile at its surveyed 10.7 percent, the poorest quintile's 42.6 percent would have to fall by 74.9 percent to 10.7 percent in three years. If the richest quintile also abandoned kerosene, say, by a fifth, the poorest would have to fall by 79.9 percent; if by two-fifths, by 84.9 percent.

A decline of three quarters in three years is not a plausible reading of the Malagasy energy transition. The only route out of kerosene at that scale is electrification or solar substitution, and grid access in the bottom quintile stood at 2.1 percent at the time of the survey, compared with 48.2 percent in the top quintile, so the grid cannot be the mechanism for the households in question. Solar kits are the plausible margin, and their diffusion among the poorest is bounded by the same budget constraint that makes the price increase bite. A fall of that magnitude would also have shown up as a collapse in national kerosene deliveries before 2025, and it did not: deliveries during the freeze were rising by 2.14 percent a month.

The mathematical point is therefore that the temporal gap erodes the margin of the conclusion without reversing it. Any plausible amount of substitution between 2022 and 2025 leaves the poorest quintile several times more exposed to a kerosene price increase than the richest, which is all the incidence claim requires. We note in

passing that if solar substitution did proceed rapidly among people with low incomes, part of the measured volume decline is that substitution rather than a loss of lighting, which would soften the welfare reading while leaving the distributional direction intact.

Against this, our findings sit uneasily with the standard result that fuel subsidies are regressive and their removal progressive. That result, as Arze del Granado et al. (2012) make clear, is driven by gasoline and diesel, which the rich consume disproportionately. Kerosene is the recognized exception, and the Malagasy reform is a case in which the exception was the instrument that was actually adjusted. A reform can be simultaneously fiscally sound in aggregate and regressive in composition, and the composition is a policy choice rather than a technical necessity.

4. Discussion

Our results fall within a particular place in the literature. On magnitudes, they are outliers: an elasticity between -0.5 and -0.9 pooled and between -0.9 and -1.5 for kerosene sits far above the corrected gasoline consensus of Havranek et al. (2012). We attribute the gap to product and setting rather than to method, since kerosene for lighting has substitutes that gasoline for transport does not, and since our shock is large and discrete. Readers who take the meta-analytic prior seriously should treat our kerosene estimate as an upper bound on the structural elasticity and our conclusions about the volume response as correspondingly cautious.

On incidence, our finding is a special case rather than a contradiction. The proposition that fuel subsidies are captured by the rich, established across developing countries by Arze del Granado et al. (2012) and Coady et al. (2017), is driven by gasoline and diesel subsidies. Kerosene is the recognized exception, and Madagascar has adjusted it. Our result is therefore consistent with that literature and inconsistent only with the policy inference sometimes drawn from it, namely that any fuel price increase is progressive.

On design, the lesson runs counter to Rentschler (2016), who argues from Nigerian data that what determines the poverty effect of a reform is not whether prices rise but which prices rise and what compensates the losers. Madagascar's reform contained no compensating instrument that we can identify. Calvo-González et al. (2015) document, in the context of an energy subsidy reform in Central America, that households' perception of a reform can diverge from its measured incidence, which matters for the political sustainability of any sequel.

The post-reform window contains nine months. Elasticities estimated over such a horizon capture short-run adjustment, and the long-run response, which would incorporate device replacement and grid connection, may be larger still. The direction of the bias is therefore towards understatement.

We observe deliveries to the domestic market rather than household consumption. If distributors ran down their stock in anticipation of price changes, the

measured response would be displaced in time relative to the true response. We see no evidence of an anticipatory movement in the month before liberalization, but we cannot rule it out.

We do not observe the price structure. Without the import parity price and the tax and margin components published by the hydrocarbons office, we cannot state whether the frozen prices were above or below cost recovery, nor quantify the fiscal saving from the reform. This is the single most valuable addition a future version of this paper could make, and it would convert an estimate of demand response into a full evaluation of the reform.

Because that barrier is one other researchers working on administered fuel prices will meet, we set out how we would go about clearing it, in descending order of fidelity. The primary route is the *arrêté interministériel*, which sets each price revision; these instruments are published in the *Journal Officiel de la République de Madagascar* and include the structure de prix as an annex, line by line, so a series can be reconstructed from the gazette for any month in which a revision was decreed. The gazette is held in hard copy at the Imprimerie Nationale and at the law faculty libraries, which makes the work slow rather than impossible, and it is the only source that gives the administration's own accounting.

Where the gazette is incomplete, the structure can be rebuilt from its components. The import parity price for a cargo landed at Toamasina is the Arab Gulf or Mediterranean quotation for the relevant grade, published daily by the price reporting agencies and available as monthly averages in the commodity statistics of international financial institutions, plus freight, insurance, and port charges. Converted at the monthly average ariary rate published by the central bank and added to the statutory duty and value-added tax rates, which are in the tax code and rarely change, this reproduces the cost-recovery price within the distribution margin. The margin is the one component that must be assumed, and it can be bounded by the range implicit in the revisions that are documented.

A third route is indirect and cheaper. The gap between a reconstructed cost-recovery price and the frozen administered price, multiplied by volumes, is the implicit subsidy; the arrears accumulated by the state towards the distributors over the freeze are the same quantity seen from the other side, and those arrears appear in the fiscal reporting and in the program documents of the International Monetary Fund. Agreement between the two estimates would validate the reconstruction. We recommend attempting all three and reporting the spread, rather than treating any one of them as the price structure.

Three products yield three clusters, and this is the paper's binding limitation rather than a technical footnote. We address it with an exact randomization test on the timing of the price path rather than by asserting cluster-robust asymptotics, and the test yields a mixed verdict, which we report in full in Section 5.3. Its floor of $p = 0.021$ is the highest any test of this design can achieve, and two of our five estimands reach it, while two do not. The approaches developed for few treated groups by Conley and Taber (2011) and Ferman and Pinto (2019) require a larger set of untreated units than

three products provide. A convincing resolution requires more price events or more products, not a better estimator: the natural extension is to pool several African countries operating administered fuel price structures, which would turn three clusters into thirty and make the design testable in the ordinary way.

Finally, the two events are not independent draws. The July 2022 asymmetry created the relative price configuration whose reversal we study in 2025. A fuller treatment would model the pricing rule itself as a political-economy object, in the spirit of the literature on the timing of subsidy reform.

5. Conclusion

Madagascar froze administered fuel prices for 30 months and then released them, and the release raised the price of the poor's fuel by 39 percent while lowering the price of transport fuels. We use the resulting configuration to estimate fuel demand elasticities in a low-income African setting from observed rather than simulated variation, and find a pooled elasticity between -0.5 and -0.9 , with kerosene between -0.9 and -1.5 . The estimates pass a placebo on a window in which prices did not move, and they pass an exact randomization test over the timing of the price path in two of five specifications, at the smallest p-value that test can return; the pooled levels estimate does not pass it. We present them as a coherent set of magnitudes, with partial statistical support from a design that includes three products and two price events. The policy reading is not that the freeze should have continued. An untargeted price freeze is an expensive and opaque instrument, and the thirty months of accumulated implicit subsidy were financed by someone. The reading is that the unwinding's composition was a choice. Raising kerosene by 39 percent while lowering petrol by 14 percent shifts real income in a direction that the standard case for fuel subsidy reform does not support, in a country where three-quarters of the population is poor and only fifteen per cent have an electricity connection. A reform that had held kerosene and adjusted transport fuels further would have raised the same revenue with a different incidence.

The prospective question is how a government that has decided to unwind a subsidy can do so without this incidence, and the answer is arithmetic before it is political. Three design rules follow from our measurements. First, differentiate the increase by the exposure gradient rather than by the fiscal weight of each product. The exposure ratio between the bottom and top quintiles is roughly 4:1 for kerosene and the opposite for petrol. A revenue-neutral package that had raised petrol and diesel prices by the amount needed to fund the kerosene subsidy, rather than the reverse, would have collected the same sum from a base that the survey shows is concentrated in the upper quintiles. Second, if kerosene must rise, size the compensation relative to the measured loss rather than to a poverty headcount. The loss is the pre-reform quantity multiplied by the price increase, about 6.0 billion ariary a month at the peak, and it falls on households identifiable by an observable characteristic, the absence of a grid connection, which the utility records and which is far cheaper to verify than

income. A transfer conditioned on that characteristic and delivered through the mobile money networks that already reach rural Madagascar would cost a fraction of the sum at stake. Rentschler (2016) shows for Nigeria that a uniform national transfer fails precisely where exposure is uneven, which is the case here.

Third, sequence the increase so that the substitute arrives before the price does. Our estimate of an elastic response means households do switch; whether they switch to a solar lamp or to darkness depends on whether the lamp is affordable when the kerosene price rises. Pairing the price increase with a credit or subsidy for solar kits converts a welfare loss into a technology transition, and the elasticity we measure determines how large that pairing must be. The wider methodological point is that administered price regimes, common across low-income Africa and usually treated as a nuisance, are an underused source of variation. Where a state sets several prices, freezes them, and then changes them by different amounts, it generates relative price movements that no market would have produced. Whether that variation supports a causal reading depends on why the state chose the amounts it did, which is why we have stated the assumption and tested what is testable rather than calling the episode an experiment. The data needed to exploit it are available in national statistical bulletins that no international database holds.

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Conflict of Interest Statement

The author declares that he has no financial or non-financial interests that could be perceived to have influenced the conduct or the reporting of this study.

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