مركز الملك عبدالله للدراسات والبحوث البترولية
King Abdullah Petroleum Studies and Research Center

Discussion Paper

The Determinants of Successful Energy Subsidy Reforms
A Logistic Regression Analysis

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December 2024 | Doi: 10.30573/KS--2024-DP74

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Abstract

Many factors influence whether an energy subsidy reform is successful, where success is defined as a reform that does not lead to social unrest and is not reversed. To better understand these factors, we apply logistic regression analysis to an original dataset capturing 400 distinct episodes of energy subsidy reform and their outcomes across 43 countries between 1995 and 2022. We find that larger energy price increases are more likely to trigger social unrest or lead to a reform reversal, pointing to the importance of gradual reform implementation. We also find that subsidy reforms are less likely to cause social unrest or a reversal when economies are growing, underscoring the importance of appropriate timing. Additionally, we quantify the contributions of factors such as the level of human development to the success of energy subsidy reform. Our analysis yields important insights that can help policymakers better understand the size of the barriers that they face, given their national circumstances, and design and implement reforms in a way that reduces the odds of an unsuccessful outcome.

1. Introduction

Energy subsidy reform has the potential to unlock fiscal, economic, health, and environmental benefits for a country (Black et al. 2023). However, its implementation is politically challenging. Despite an extensive number of past reform attempts, governments continue to face challenges in implementing reforms, with many resulting in social unrest or reversals – both unwelcome outcomes for policymakers. In 2022, energy subsidy reforms were implemented in countries as varied as Bangladesh, Haiti, Indonesia, Kazakhstan, Sri Lanka, and Tunisia, with most of these triggering social unrest (CE Notices Financieras English 2022; IANS-English 2022; Widianto 2022; Perera 2022). Moreover, in Kazakhstan, the reform also led to a reversal, the resignation of the government, and a two-week state of emergency (Sullivan 2022).

We define an energy subsidy reform as a subsidy reduction resulting from an increase in the regulated price of an energy product that brings it closer to the level it would be in a deregulated market.[1] As for success, we define it as an outcome where energy subsidy reform does not lead to social unrest and is not reversed. Such a two-pronged definition of success has been used previously in the literature (Chelminski 2018).

Different circumstances have moved energy subsidy reform up the policy agenda. Historically, fiscal pressures drove most countries' reform plans and continue to do so (Vagliasindi 2013; Rentschler and Bazilian 2017a). However, climate change is expected to drive further subsidy reforms moving forward. At COP26, which took place in Glasgow in the United Kingdom in 2021, countries convening under the United Nations Framework Convention on Climate Change (UNFCCC) agreed for the first time in 26 years to the "phase-out of inefficient fossil fuel subsidies, while providing targeted support to the poorest and most vulnerable in line with national circumstances" (UN Climate Change Conference 2021).

The current scope for energy subsidy reform globally is enormous. The International Monetary Fund (IMF) estimated explicit global fossil fuel subsidies to be US$ 1.3 trillion in 2022 (Black et al. 2023), equivalent to over 1% of global gross domestic product (GDP). The International Energy Agency (IEA) obtained a similar estimate for global fossil fuel subsidies in 2022, estimating the total to be over US$ 1.1 trillion, while noting that this was a record level for global fossil fuel subsidies since the IEA started tracking them (IEA 2024).

Given the political importance, enormous potential, and challenges associated with further energy subsidy reforms, research is needed to inform policymakers about the factors influencing reform outcomes. Despite the importance of the topic, there is surprisingly very little published research in this area, as McCulloch et al. (2022) noted. There are currently two strands in the literature on energy subsidy reform outcomes. The first – and much larger – strand consists of qualitative reviews that draw lessons from countries' successful and unsuccessful past attempts at energy subsidy reform (e.g., UNEP 2003;

Viktor 2009; Commander 2012; Beaton et al. [2013]; Clements et al. 2013; Vagliasindi 2013; Atansah et al. 2017; Rentschler and Bazilian 2017). The second strand, which relates more closely to this paper, includes studies that quantify the impact of different factors on the occurrence of social unrest following energy subsidy reform. To the best of our knowledge, there are currently only two such quantitative studies, both of which were published recently: Natalini, Bravo, and Newman (2020) and McCulloch et al. (2022). Both applied logistic regression analysis to explore how different factors influence the occurrence of fuel-related riots.

This paper builds on those two studies and contributes to a deeper understanding of the determinants of energy subsidy reform outcomes by applying logistic regression analysis on an original dataset that captures 400 energy subsidy reform episodes implemented across 43 different countries between 1995 and 2022. Through regression analysis, we quantify the effects of different possible explanatory variables on the odds of two key outcomes: the first outcome relates to whether social unrest occurs following an energy subsidy reform, while the second outcome relates to whether the reform gets reversed.

Our results indicate that gasoline and kerosene prices are two key determinants of social unrest after energy subsidy reforms. Other important determinants include annual GDP growth, population, civil freedom, and the level of human development in a country. Our results indicate that kerosene prices are the primary determinant of reversals, which is likely due to kerosene's importance to lower-income households. We also confirm that compensation plays an essential role in preventing the occurrence of a reform reversal.

The remainder of this paper is structured as follows. Section 2 looks at the literature and compares the few existing quantitative studies on this topic. Section 3 details and describes our newly constructed dataset on energy subsidy reforms and outcomes and shows our logistic regression method. Section 4 presents and discusses the results of our regressions, while Section 5 concludes.

2. Literature Review

There are very few published quantitative studies that explore the determinants of energy subsidy reform outcomes. This paper focuses on two key outcomes that can occur following energy subsidy reform. The first outcome relates to whether the reform is reversed or not, which is a measure of success used in some qualitative studies (e.g., Clements et al. 2013). The second outcome relates to whether or not the reform leads to social unrest.

To the best of our knowledge, no studies have quantified the determinants of energy subsidy reform reversal, and only two studies have quantified the determinants of energy subsidy reform-related social unrest: Natalini, Bravo, and Newman (2020) and McCulloch et al. (2022). Both studies quantified how different explanatory variables influence the odds of 'fuel riots,' which they define as incidents of significant social unrest that occur in response to the reduction or removal of energy subsidies. Natalini, Bravo, and Newman (2020) quantified the effects of one set of explanatory variables on fuel riots, covering the 2005-2016 period, while McCulloch et al. (2022) quantified the effects of a different set of explanatory variables after extending the same dataset to 2018. The same manual Google search with the same set of keywords was used to capture instances of fuel riots from news articles. Data from both Natalini, Bravo, and Newman (2020) and McCulloch et al. (2022) contain 59 instances of fuel riots. However, the explanatory variables in both studies' datasets, which were used to explain the occurrence of fuel riots in the regressions, differ completely. Natalini, Bravo, and Newman (2020) included the international crude oil price, political stability, net fuel exports, and regime type as explanatory variables, while McCulloch et al. (2022) included the change in international and domestic gasoline prices, GDP growth, government effectiveness, corruption, civil freedom, anti-government movements, GDP per capita, and population as explanatory variables in one of their main models. Natalini, Bravo, and Newman (2020) found that the international crude oil price has a statistically significant effect in increasing the odds of fuel riots, and that more politically stable or fuel- importing countries are less likely to have fuel riots (see Table 1). In contrast to the results of Natalini, Bravo, and Newman (2020), McCulloch et al. (2022) found that the international gasoline price has no statistically significant effect on the occurrence of fuel riots. Instead, McCulloch et al. (2022) found that it is the growth in the domestic gasoline price that increases the odds of fuel riots. Also, in contrast to Natalini, Bravo, and Newman (2020), who found statistically significant coefficients on the other explanatory variables, McCulloch et al. (2022) found that only anti-government movements have a statistically significant effect in increasing fuel riots, with no statistically significant effects observed for any of the other explanatory variables. Table 1 highlights some of the conflicting results between Natalini, Bravo, and Newman (2020) and McCulloch et al. (2022), which could stem from omitted variable bias, as each model contains a different set of explanatory variables. The conflicting results could also stem from the papers using different methods, or due to the extension of the dataset by two years by McCulloch et al. (2022), which increased the number of observations. Since the number of instances of fuel riots was 59 in both studies' datasets, this suggests that the larger dataset of McCulloch et al. (2022) included only a larger number of observations of no fuel riots.

Table 1. Estimated logistic regression models from the literature.

Included explanatory variables	Natalini, Bravo, and Newman (2020) model 1	McCulloch et al. (2022) model 1
International crude oil price	0.756***	
International gasoline price growth		4.022
Domestic gasoline price growth		2.109**
GDP growth		−0.0437
per capita GDP		0.145
population		−0.0733
Population growth		
Political stability	0.765***	
Net fuel exports	−0.405*	
Regime type	0.119	
Government effectiveness		−1.234
Extent of corruption		3.743
Civil society freedom		0.416
Anti-government movements		0.813
Method ***Number of observations***	Logistic mixed-effects model 1769	Fixed effects logit panel regression 3833

Given the conflicting results between Natalini, Bravo, and Newman (2020) and McCulloch et al. (2022), it is difficult to draw takeaways for policymakers. However, the conflicting results do point to an important discussion about the determinants of fuel riots. In theory, fuel riots as they have been defined are driven by consumers protesting higher domestic fuel prices. However, Natalini, Bravo, and Newman (2020) found that the international crude oil price is driving fuel riots. In countries with deregulated fuel prices, domestic fuel prices depend directly on the international oil price, as they are very closely correlated. However, this is not the case in many developing countries that regulate fuel prices (Kpodar and Imam 2020). In such countries, the effect of the international crude oil price on domestic fuel prices can depend on whether the country is a net fuel importer or exporter. In the case of fuel subsidizing oil exporters, domestic fuel prices are generally increased when international oil prices fall to compensate for decreased oil export revenues (Fattouh, Sen, and Moerenhout 2016). In contrast, fuel- subsidizing oil importers are more likely to raise domestic fuel prices when international oil prices are high, which tends to increase the fiscal burden from fuel subsidies (Vagliasindi 2013). Additionally, fuel-subsidizing countries generally face challenges in passing through changes in international oil prices to domestic fuel prices. For example, in Kuwait, gasoline prices had remained fixed for decades prior to the 2016 gasoline subsidy reform (Agence France Presse 2016). Therefore, the international crude oil price in many cases may not reflect domestic fuel prices, so domestic prices should ideally be used as explanatory variables in the regression.

McCulloch et al. (2022) included both the domestic and international gasoline price as potential determinants of fuel riots and found that only the domestic gasoline price has a statistically significant effect on the odds of a fuel riot occurring. However, McCulloch et al. (2022) looked at the relationship between fuel riots and only the domestic gasoline price. In many countries, it may be changes in the domestic prices of other fuels, such as diesel, kerosene, or liquefied petroleum gas (LPG), that drive the occurrence of fuel riots. For example, Kazakhstan's recent energy subsidy reform in 2022, which targeted LPG, led to widespread fuel riots (Sullivan 2022). Attributing such fuel riots to gasoline prices and omitting LPG prices from the model can potentially lead to misleading results. Furthermore, McCulloch et al. (2022) omitted explanatory variables that Natalini, Bravo, and Newman (2020) had found to be statistically significant, so there may be issues with omitted variable bias.

In this paper, we seek to address several of the gaps identified in the literature. First, we run a logistic regression analysis on two reform outcomes: the occurrence of social unrest and the occurrence of a reversal – the latter having not been looked at previously. Second, we run our regressions on an original dataset that was constructed by leveraging multiple specialized news databases. Our dataset thus captures many instances of fuel riots that were missing in the datasets used by Natalini, Bravo, and Newman (2020) and McCulloch et al. (2022). For example, our dataset has 142 instances of social unrest driven by energy subsidy reform, compared to only 59 in both previous studies. Third, we include a comprehensive set of relevant explanatory variables in the regression models to minimize omitted variable bias. We do this by including both sets of explanatory variables used in Natalini, Bravo, and Newman (2020) and McCulloch et al. (2022). Our dataset also contains important explanatory variables that had not been looked at previously, including increases in the domestic prices of diesel, LPG, kerosene, and residential electricity, and whether compensation was given during the reform.

3. Data and Methods

3.1 Dataset Construction

The dataset used in this study captures information about the outcomes of energy subsidy reforms as well as their determinants. The first step in building this dataset involved collecting news articles that contain information related to energy subsidy reform attempts, obtained through precise search queries.

To maximize our coverage of energy subsidy reform episodes, we conducted our search for news articles by accessing three platforms concurrently: Nexis (2023), ProQuest (2023), and Google (2023). This is a substantial improvement on Natalini, Bravo, and Newman (2020) and McCulloch et al. (2022), who only used Google News, which has limitations for quantitative research, as pointed out in Buntain, Liebler, and Webster (2023). Given the very high number of news articles across the three platforms (e.g., Nexis alone hosting around 144 billion documents), we utilized search command operators to narrow our search results.[2] One of our most widely used search queries in Nexis combined the various operators, as shown in Figure 1. This query instructs Nexis to search for news articles that mention the country in question using seven words (e.g., "diesel," "fuel," and "hike") to create different terms. It also instructs Nexis to search for documents that meet these requirements in the title or lead paragraph. This search query also ensures that all five energy products that may be important to households – gasoline, diesel, kerosene, LPG, and electricity – are covered in the search results. This query on Nexis, and identical queries on ProQuest and Google, were repeated on a country-by-country and year-by-year basis. Our final database contains over 3,000 news articles, stretching from 1995 to 2022, covering 43 countries, and providing details on 400 distinct episodes of energy subsidy reform and their outcomes. These numbers exclude episodes and countries (specifically Venezuela) that had to be dropped due to the unavailability of many of the explanatory variables, which are discussed in the subsequent section, for those episodes or countries.

Figure 1. Example of query used in Nexis to search for relevant news articles for one of the countries.

HLEAD (
((India) **W/7**
(
((fuel) *OR* (gasoline) *OR* (petrol) *OR* (diesel) *OR* (kerosene) *OR* (LPG) OR ("cooking gas") OR (electricity) OR (power))
AND
((price) OR (tariff) OR (subsid*) OR (rate))
)
)
AND
((raise) *OR* (increase) *OR* (lift) *OR* (hike) *OR* (cut))
)

Note: Precision operators are in bold, while Boolean operators are in italics. The asterisk after subsidy allows the search to find both the terms "subsidy" and "subsidies."

The news articles obtained were reviewed manually for any content related to the absolute or percentage price increases implemented during a subsidy reform, the prices before and after the reform, the occurrence of social unrest, the occurrence of a reversal, and whether cash compensation was used to mitigate the negative impacts of reform. Quantitative data on the price changes across all five energy products were pulled into our dataset. The occurrences of unrest or reversal were coded into two separate binary variables, with these two variables used as the dependent variables in our analysis. Finally, a binary explanatory variable was also constructed from news content reflecting whether cash compensation schemes were used to compensate households for the higher energy prices.

We also collected data on additional explanatory variables that may influence the outcomes of energy subsidy reform. These variables include those related to the type of regime in a country, people's freedom to protest, economic performance, human development, governance, and institutional quality.

- **Political regime and civil liberties.** As noted by McCulloch et al. (2022), social unrest may be less likely to occur in countries with autocratic regimes and those with fewer civil liberties. The polity2 variable, obtained from the Center for Systemic Peace (CSP 2023), reflects a country's political regime. Its values range from -10, which reflects a hereditary monarchy, to +10, which reflects a consolidated democracy. A 21-unit change therefore represents a complete transition from a hereditary monarchy to a consolidated democracy. Since the polity2 variable only runs to 2018, we extend the values to 2022 for each country based on its last data value.[3] As for the degree of civil freedom, we used the Civil Liberty Dataset (CLD) (Skaaning 2020), which captures five aspects of civil freedom: 1) freedom of opinion and expression, 2) freedom of assembly and association, 3) freedom of thought, conscience and religion, 4) freedom of movement and residence, and 5) fair trial. For each of these five elements, the values range from 1 (the lowest) to 4 (the highest). We took an average of all five variables to obtain each country's average level of civil freedom. Our values range from a low of 1 (e.g., Afghanistan in 2021) to a high of 4 (e.g., the United States of America in 2019). The latest CLD dataset (v2.8) currently runs to 2023.
- **The population, level of development, and economic performance.** Explanatory variables that cover these national circumstances include population, annual GDP growth, per capita GDP (at constant prices), and inflation, all of which were obtained from the World Bank (2023). Furthermore, we obtained the Human Development Index (HDI) from the United Nations Development Programme (UNDP 2023), a composite variable that captures the health (e.g., life expectancy at birth), education (e.g., mean years of schooling), and standard of living (e.g., gross national income) of a country. The HDI varies from 0 (the lowest level of human development) to 1 (the highest), but we scaled it up to vary from 0 to 100 to simplify the interpretation of the regression results.
- **Governance and institutional quality.** The World Bank (2023) has developed the World Governance Indicators (WGIs), a dataset that captures six dimensions of governance and institutional quality. The six variables are 1) voice and accountability, 2) political stability and the absence of violence/terrorism, 3) government effectiveness, 4) regulatory quality, 5) rule of law, and 6) control of corruption, all of which were added to our dataset. All six variables are in units of a normal standard variable with zero mean and a standard deviation of one (Kaufmann, Kraay, and Mastruzzi 2010). Their values range from approximately -2.5 (weak governance) to +2.5 (strong governance), although the values for some countries in some years go beyond this range.[4] Any missing values in this dataset during the 1995-2022 period were interpolated using the estimates for the years before and after.
- **Status as a net energy importer or exporter.** As this factor may influence citizens' expectations of energy subsidies (Lockwood 2015; Chelminski 2018), we also developed a binary variable reflecting whether a country is a net energy importer or exporter in each year. A country was classified as a net exporter of energy (coded as one) if its primary energy production was higher than its primary energy consumption, based on data from the Energy Information Administration (EIA 2023), and classified as a net importer of energy (coded as a zero) in the opposite instance.

3.2 Dataset Description

Our final quantitative dataset contains exactly 400 episodes of energy subsidy reform, with two different dependent variables (social unrest and reform reversal) and 20 explanatory variables for each episode. The 400 episodes of energy subsidy reform can be disaggregated into 142 episodes that resulted in social unrest and 258 episodes that did not. They can also be disaggregated into 48 episodes that resulted in reversals and 352 episodes that did not. The frequency of energy subsidy reforms implemented by country and disaggregated by outcome are shown in Table 2, which reveals extensive variation in the number of reforms implemented across countries and their outcomes. The number of reforms a country implemented depended on a multitude of factors, such as whether it followed a gradual or an abrupt approach to reform or whether it decided to maintain fixed energy prices for long periods of time.[5,6] Table 2 also reveals that some countries have experienced both outcomes (e.g., Indonesia, Ecuador, India, and Nigeria), while some have only experienced one of the outcomes (e.g., Saudi Arabia, the United Arab Emirates, Qatar, and Morocco).

Table 2. Number of observations by outcome and country in our dataset.

Country	*Occurrence of social unrest*			*Occurrence of reversal*		
	No = 0	**Yes = 1**	**Total**	**No = 0**	**Yes = 1**	**Total**
Algeria	1	1	2	2	0	2
Angola	4	0	4	4	0	4
Azerbaijan	6	2	8	7	1	8
Bahrain	2	0	2	2	0	2
Bangladesh	9	9	18	17	1	18
Bolivia	0	3	3	1	2	3
Cameroon	2	3	5	4	1	5
Chad	1	0	1	1	0	1
Ivory Coast	0	1	1	0	1	1
Ecuador	4	7	11	6	5	11
Egypt	13	1	14	14	0	14
El Salvador	1	0	1	1	0	1
Gabon	1	0	1	1	0	1
Ghana	5	5	10	9	1	10
Haiti	1	5	6	5	1	6
India	7	17	24	19	5	24
Indonesia	4	13	17	14	3	17
Iran	12	3	15	15	0	15
Iraq	6	1	7	7	0	7
Jordan	11	4	15	14	1	15
Kazakhstan	1	1	2	1	1	2
Kuwait	4	0	4	3	1	4

Table 2. (continued)

Country	*Occurrence of social unrest*			*Occurrence of reversal*		
	No = 0	**Yes = 1**	**Total**	**No = 0**	**Yes = 1**	**Total**
Libya	1	0	1	1	0	1
Malaysia	17	3	20	19	1	20
Mexico	2	2	4	4	0	4
Morocco	3	0	3	3	0	3
Myanmar	4	4	8	7	1	8
Namibia	17	0	17	17	0	17
Nepal	9	16	25	17	8	25
Nigeria	2	13	15	9	6	15
Oman	3	0	3	3	0	3
Pakistan	0	1	1	0	1	1
Qatar	5	0	5	5	0	5
Saudi Arabia	6	0	6	6	0	6
Sierra Leone	2	3	5	4	1	5
Sri Lanka	23	5	28	27	1	28
Sudan	10	11	21	19	2	21
Thailand	4	1	5	5	0	5
Tunisia	8	2	10	10	0	10
Turkmenistan	3	0	3	3	0	3
UAE	10	0	10	10	0	10
Uzbekistan	29	0	29	29	0	29
Yemen	5	5	10	7	3	10
Total	**258**	**142**	**400**	**352**	**48**	**400**

Table 3 provides summary statistics on the energy price increases in our dataset, demonstrating that countries on average (across all 400 episodes) implemented price increases smaller than 30%, with increases in gasoline and diesel prices being higher on average than kerosene, LPG, and electricity. Nevertheless, the range of price increases is wide, and, in a few rare cases, governments decreased the price of one fuel to compensate for increases in the prices of other fuels during a reform.

Table 3 also shows the mean energy price increases disaggregated by outcome. We find that – for all five energy products – the mean increases implemented in episodes that triggered social unrest were higher than the mean increases implemented in episodes that did not trigger social unrest, suggesting that larger price increases may trigger unrest. The same applies to the mean increases implemented in episodes that culminated in a partial or complete reversal, as they were higher – across all five energy products – than the mean price increases implemented in episodes that were not reversed, again pointing to the possible influence of larger energy price increases on unsuccessful outcomes.

Figure 2 highlights the share of episodes and outcomes based on whether cash compensation was used. It shows that with compensation, social unrest occurred at a rate of around 30.8%. However, when compensation was not used, social unrest occurred in 36.0% of episodes. These values suggest that compensation may reduce the likelihood of unrest. In the case of reversal as an outcome, the effect of compensation is much more prominent. When compensation was used, none of the episodes resulted in a reversal (a reversal rate of 0%). In contrast, when compensation was not used, 13.4% of episodes culminated in a reversal.

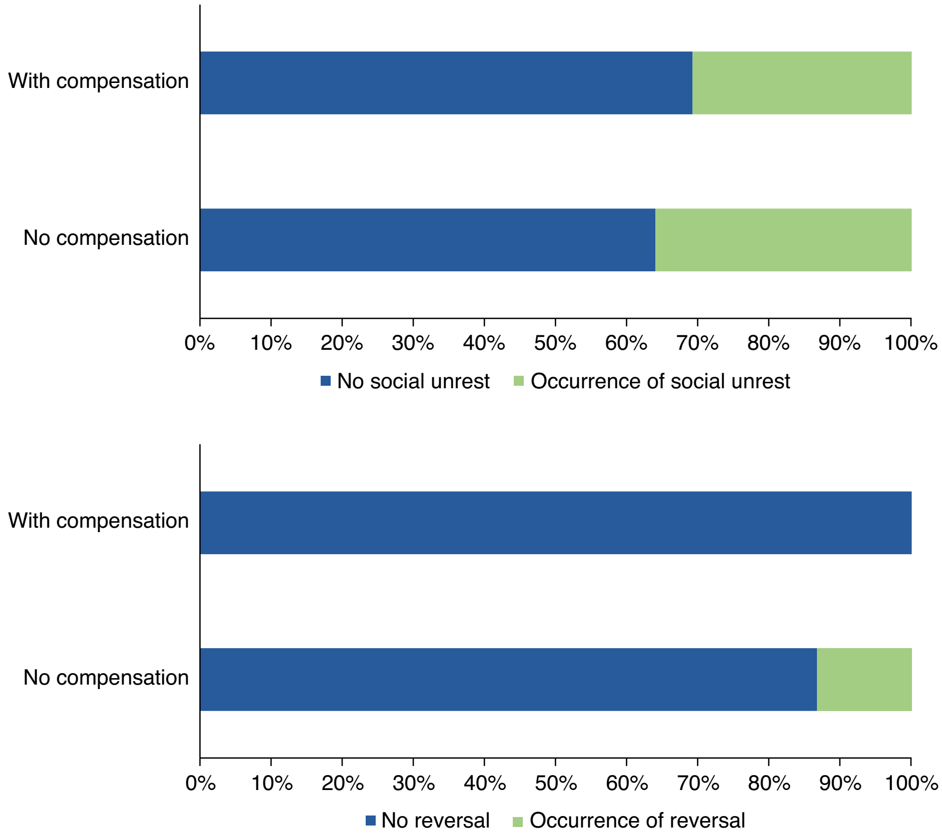
Table 4 provides summary statistics for all the remaining economic, political, institutional, and governance variables, disaggregated based on whether social unrest occurred or not. It shows that social unrest is correlated to slower economic growth, lower GDP per capita, higher inflation, a larger population, a lower HDI, and weaker governance across all WGI indicators, except for voice and accountability. It also shows social unrest was correlated with countries being more democratic, having more civil freedoms, and being importers of energy.[7] The same patterns are observed with regards to reversal as an outcome, as shown in Table 5, with only two differences: Countries in which reversals occurred had, on average, lower levels of inflation and were more likely to be exporters of energy.

Table 3. Summary statistics, categorized by outcome, for the energy price increases implemented across all 400 episodes of energy subsidy reform.

Episodes		Percentage increase in price of energy product (%)				
		Gasoline	Diesel	Kerosene	LPG	Electricity
No social unrest (N = 258)	Mean	19.0%	21.8%	9.4%	11.7%	10.7%
	SD	68.8%	103.9%	39.4%	67.5%	67.3%
	Min	0.0%	−29.3%	0.0%	0.0%	0.0%
	Max	733.3%	1,415.2%	506.1%	900.0%	854.5%
Occurrence of social unrest (N = 142)	Mean	32.0%	35.2%	23.1%	14.4%	13.7%
	SD	57.1%	76.1%	51.0%	40.3%	58.6%
	Min	0.0%	0.0%	0.0%	−18.9%	0.0%
	Max	525.0%	800.0%	400.0%	300.0%	450.4%
No reversal (N = 352)	Mean	22.9%	25.2%	12.2%	11.1%	11.5%
	SD	68.0%	99.5%	42.0%	59.4%	64.2%
	Min	0.0%	29.3%	0.0%	18.9%	0.0%
	Max	733.3%	1,415.2%	506.1%	900.0%	854.5%
Occurrence of a reversal (N = 48)	Mean	29.0%	36.8%	29.1%	23.9%	13.7%
	SD	38.2%	51.9%	56.8%	57.4%	65.4%
	Min	0.0%	0.0%	0.0%	0.0%	0.0%
	Max	183.7%	209.1%	283.3%	300.0%	441.7%
Total (N = 400)	Mean	23.6%	26.6%	14.2%	12.7%	11.7%
	SD	65.1%	95.1%	44.3%	59.2%	64.3%
	Min	0.0%	29.3%	0.0%	18.9%	0.0%
	Max	733.3%	1,415.2%	506.1%	900.0%	854.5%

Note: SD = standard deviation; Min = minimum; Max = maximum; N = number of episodes.

Figure 2. Frequency of outcomes based on whether compensation was used or not.



Source: Authors.

Table 4. Summary statistics, categorized by unrest outcome, for the economic, political, institutional, and governance variables in countries across all 400 implementations.

	No social unrest (N = 258)				Occurrence of social unrest (N = 142)				Total (N = 400)			
	Mean	**SD**	**Min**	**Max**	**Mean**	**SD**	**Min**	**Max**	**Mean**	**SD**	**Min**	**Max**
Annual % GDP growth	5.2	3.6	−5.3	34.9	4.2	5.5	−17.0	34.5	4.8	4.4	−17.0	34.9
Annual GDP per capita (2015 USD)	7343	12916	286	73493	2355	1918	555	11291	5572	10699	286	73493
Population (millions)	66.9	191.0	1.4	1340.0	204.0	369.0	6.6	1290.0	116.0	276.0	1.4	1340.0
Government effectiveness	−0.28	0.69	−2.09	1.51	−0.65	0.58	−2.23	1.25	−0.41	0.68	−2.23	1.51
Regulatory quality	−0.49	0.79	−2.20	1.11	−0.67	0.51	−2.27	0.54	−0.56	0.71	−2.27	1.11
Voice and Accountability	−0.82	0.76	−2.26	0.55	−0.51	0.65	−2.21	0.49	−0.71	0.74	−2.26	0.55
Political Stability	−0.61	0.92	−2.83	1.20	−1.17	0.70	−2.69	0.29	−0.81	0.89	−2.83	1.20
Rule of Law	−0.41	0.70	−1.84	0.86	−0.69	0.56	−1.74	0.45	−0.51	0.66	−1.84	0.86

Table 4. (continued)

	No social unrest (N = 258)				Occurrence of social unrest (N = 142)				Total (N = 400)			
	Mean	**SD**	**Min**	**Max**	**Mean**	**SD**	**Min**	**Max**	**Mean**	**SD**	**Min**	**Max**
Control of corruption	−0.46	0.68	−1.66	1.07	−0.80	0.45	−1.63	0.28	−0.58	0.63	−1.66	1.07
Regime type	−0.88	6.44	−10.00	9.00	3.37	5.24	−8.00	9.00	0.63	6.37	−10.00	9.00
Civil Freedom	2.23	0.61	1.20	3.60	2.57	0.50	1.40	3.60	2.35	0.60	1.20	3.60
Human Development Index	0.66	0.11	0.37	0.88	0.58	0.10	0.38	0.80	0.63	0.12	0.37	0.88
Annual % inflation	15.8	42.4	−16.1	359.1	20.4	46.7	−0.2	359.1	17.4	44.0	−16.1	359.1
Exporter or importer	0.55	0.50	0.00	1.00	0.47	0.50	0.00	1.00	0.53	0.50	0.00	1.00

Notes: SD = standard deviation; Min = minimum; Max = maximum; N = number of episodes.

Table 5. Summary statistics, categorized by reversal outcome, for the economic, political, institutional, and governance variables in countries across all 400 implementations.

	No reversal (N = 352)				Occurrence of reversal (N = 48)				Total (N = 400)			
	Mean	**SD**	**Min**	**Max**	**Mean**	**SD**	**Min**	**Max**	**Mean**	**SD**	**Min**	**Max**
Annual % GDP growth	5.0	4.4	−17.0	34.9	4.0	3.8	−13.1	14.0	4.8	4.4	−17.0	34.9
Annual GDP per capita (2015 US$)	5939.2	11243.7	286.0	73493.3	2879.4	4378.3	554.5	29315.2	5572.0	10698.7	286.0	73493.3
Population (millions)	108.0	265.0	1.4	1340.0	171.0	343.0	3.9	1270.0	116.0	276.0	1.4	1340.0
Government effectiveness	−0.37	0.69	−2.23	1.51	−0.66	0.51	−1.93	0.99	−0.41	0.68	−2.23	1.51
Regulatory quality	−0.55	0.74	−2.27	1.11	−0.62	0.43	−1.50	0.54	−0.56	0.71	−2.27	1.11
Voice and Accountability	−0.73	0.75	−2.26	0.55	−0.56	0.60	−1.86	0.48	−0.71	0.74	−2.26	0.55
Political Stability	−0.76	0.89	−2.83	1.20	−1.17	0.75	−2.68	0.17	−0.81	0.89	−2.83	1.20
Rule of Law	−0.48	0.68	−1.84	0.86	−0.72	0.53	−1.71	0.35	−0.51	0.66	−1.84	0.86
Control of corruption	−0.56	0.65	−1.66	1.07	−0.79	0.40	−1.57	0.28	−0.58	0.63	−1.66	1.07
Regime type	0.36	6.43	−10.00	9.00	2.58	5.53	−7.00	9.00	0.63	6.37	−10.00	9.00

Table 5. (continued)

	No reversal (N = 352)				Occurrence of reversal (N = 48)				Total (N = 400)			
	Mean	SD	Min	Max	Mean	SD	Min	Max	Mean	SD	Min	Max
Civil Freedom	2.32	0.60	1.20	3.60	2.56	0.52	1.40	3.60	2.35	0.60	1.20	3.60
Human Development Index	0.64	0.12	0.37	0.88	0.57	0.12	0.38	0.83	0.63	0.12	0.37	0.88
Annual % inflation	17.4	45.6	−16.1	359.1	17.8	29.4	0.3	132.8	17.4	44.0	−16.1	359.1
Exporter or importer	0.52	0.50	0.00	1.00	0.56	0.50	0.00	1.00	0.53	0.50	0.00	1.00

Notes: SD = standard deviation; Min = minimum; Max = maximum; N = number of episodes.

3.3 Logistic Regression Methods

3.3.1 Pooled Logistic Regression

As both our outcome variables are binary, we run logistic regressions to quantify how each explanatory variable influences the occurrence of social unrest (the first outcome and dependent variable) and the reversal of the reform (the second outcome and dependent variable). In our pooled dataset, the unit of analysis is an episode of energy subsidy reform, denoted by the subscript *i*. In other words, each episode *i* is treated as an observation.

The first general logit model that we estimate is:

$$\begin{aligned} logit(social\ unrest_i) &= \alpha_0 + \alpha_1 GAS_i + \alpha_2 DSL_i \\ &+ \alpha_3 LPG_i + \alpha_4 KER_i + \alpha_5 ELC_i + \alpha_6 COMP_i + \alpha_7 ln(POP_i) \\ &+ \alpha_8 GDPG_i + \alpha_9 \ln(GDPPC_i) + \alpha_{10} GE_i + \alpha_{11} RQ_i \\ &+ \alpha_{12} VA_i + \alpha_{13} PSNV_i + \alpha_{14} \mathrm{RL}_i + \alpha_{15} CC_i + \alpha_{16} RT_i \\ &+ \alpha_{17} CF_i + \alpha_{18} HDI_i + \alpha_{19} INF_i + \alpha_{20} XM_i + \varepsilon_i \end{aligned} \quad [1]$$

where *social unrest$_i$* captures whether social unrest occurs following an episode *i* of energy subsidy reform. The explanatory variables are defined as follows:

- *GAS_i* is the percentage increase in the gasoline price during episode *i*
- *DSL_i* is the percentage increase in the diesel price during episode *i*
- *LPG_i* is the percentage increase in the LPG price during episode *i*
- *KER_i* is the percentage increase in the kerosene price during episode *i*
- *ELC_i* is the percentage increase in the residential electricity price during episode *i*
- *$COMP_i$* reflects whether a cash compensation scheme was launched alongside episode *i*
- *POP_i* is the annual population in the country when it implemented episode *i*
- *$GDPG_i$* is the annual GDP percentage growth for the country when it implemented episode *i*
- *$GDPPC_i$* is the annual GDP per capita for the country when it implemented episode *i*
- *GEi_i* is the annual level of government effectiveness when it implemented episode *i*
- *RQ_i* is the annual level of regulatory quality in the country when it implemented episode *i*
- *VA_i* is the annual level of voice and accountability in the country when it implemented episode *i*
- *PS_i* is the annual level of political stability in the country when it implemented episode *i*
- *RL_i* is the annual level of rule of law in the country when it implemented episode *i*
- *CC_i* is the annual level of corruption control in the country when it implemented episode *i*
- *RT_i* reflects the annual regime type (i.e., democracy level) in the country when it implemented episode *i*
- *HDI_i* is the annual Human Development Index score of the country when it implemented episode *i*
- *INF_i* reflects the annual percentage of inflation in the country when it implemented episode *i*
- *XM_i* captures whether the country was a net exporter or net importer of energy in the year when it implemented episode *i*
- ε_i represents the random error term

The second general logit model that we estimate is:

$$\begin{aligned} logit(reversal_i) &= \beta_0 + \beta_1 GAS_i + \beta_2 DSL_i + \beta_3 LPG_i + \beta_4 KER_i \\ &+ \beta_5 ELC_i + \beta_6 COMP_i + \alpha_7 ln(POP_i) + \beta_8 GDPG_i + \beta_9 \ln(GDPPC_i) \\ &+ \beta_{10} GE_i + \beta_{11} RQ_i + \beta_{12} VA_i + \beta_{13} PSNV_i + \beta_{14} \mathrm{RL}_i + \beta_{15} CC_i \\ &+ \beta_{16} RT_i + \beta_{17} CF_i + \beta_{18} HDI_i + \beta_{19} INF_i + \beta_{20} XM_i + \varepsilon_i \end{aligned} \quad [2]$$

where *reversal$_i$* captures whether or not an episode i of energy subsidy reform is reversed.

The α and β coefficients in Equation [1] and Equation [2] reflect the effects of each explanatory variable, *ceteris paribus*, on the occurrence of social unrest and reversal, respectively, with α_0 and β_0 being the intercept terms, and ε_i being the error term.

In statistical model building, the general-to-specific approach is conventionally used to find "the most parsimonious model that still accurately reflects the true outcome experience of the data" (Hosmer, Lemeshow, and Sturdivant 2013 p. 90). Hosmer, Lemeshow, and Sturdivant (2013) noted the rationale for minimizing the number of variables in a model to produce a model that is 1) more numerically stable, 2) more easily adopted for use, and 3) less likely to suffer from overfitting. Using the general-to-specific approach, we tested down to produce parsimonious models by dropping variables that were not statistically significant or problematic variables that were causing multicollinearity issues while monitoring an array of goodness-of-fit measures and diagnostic tests, including the likelihood ratio (LR) chi-square test, the pseudo R-squared, specification error tests, multicollinearity tests, and information criteria. A 10% level of statistical significance was used to determine whether any coefficient or test outcome was statistically significant. Throughout this study, the superscripts *, **, and *** are used to represent significance at the 10%, 5%, and 1% levels.

While starting from the most general model is one approach, Hosmer, Lemeshow, and Sturdivant (2013) also recommend a method they refer to as "purposeful selection." Purposeful selection begins with a univariable analysis of each explanatory variable in the general model, with a univariable logistic regression fitted for each explanatory variable. Any explanatory variables that have a p-value higher than 0.25 in the univariable regressions are then eliminated. Purposeful selection thus produces a model that is nested by the general model, assuming at least one explanatory variable gets eliminated in the screening step. Hosmer, Lemeshow, and Sturdivant (2013) then recommend testing down from the purposeful selection model, following the standard general-to-specific approach described above.

Applying the purposeful selection method described by Hosmer, Lemeshow, and Sturdivant (2013) to Equation [1], the variables LPG_i, ELC_i, $COMP_i$, and INF_i fail the screening test, leading to the following model for social unrest as a dependent variable:

$$logit(social\,unrest_i) = \alpha_0 + \alpha_1 GAS_i + \alpha_2 DSL_i + \alpha_4 KER_i + \alpha_7 ln(POP_i) + \alpha_8 GDPG_i + \alpha_9 \ln(GDPPC_i) + \alpha_{10} GE_i + \alpha_{11} RQ_i + \alpha_{12} VA_i + \alpha_{13} PSNV_i + \alpha_{14} \mathrm{RL}_i + \alpha_{15} CC_i + \alpha_{16} RT_i + \alpha_{17} CF_i + \alpha_{18} HDI_i + \alpha_{20} XM_i + \varepsilon_i \quad [3]$$

Similarly, after applying purposeful selection to Equation [2], the variables GAS_i, DSL_i, ELC_i, RQ_i, INF_i, and XM_i fail the screening test. Furthermore, $COMP_i$ is found to predict the occurrence of a reversal perfectly, as shown in Figure 2, so the variable had to be omitted from the general model. Purposeful selection led to the following model for reversal as a dependent variable:

$$logit(reversal_i) = \beta_0 + \beta_3 LPG_i + \beta_4 KER_i + \alpha_7 ln(POP_i) + \beta_8 GDPG_i + \beta_9 \ln(GDPPC_i) + \beta_{10} GE_i + \beta_{12} VA_i + \beta_{13} PSNV_i + \beta_{14} \mathrm{RL}_i + \beta_{15} CC_i + \beta_{16} RT_i + \beta_{17} CF_i + \beta_{18} HDI_i + \varepsilon_i \quad [4]$$

The *logit* command in Stata 16.0 (StataCorp 2019) was used to run all pooled logistic regression estimations.

We also included different sets of dummy variables in our models. First, we tested time dummies, including month-year and year time dummies. Month-year time dummies proved to be problematic given the small number of energy subsidy reform episodes implemented during a month-year, which often shared the same outcome, leading to most observations being dropped from the regressions as the month-year dummies perfectly predicted the outcomes. Therefore, we did not consider month-year time dummies any further. We then looked at year time dummies. The inclusion of year time dummies led to the loss of only several observations, so we proceeded to estimate and compare models with year time dummies. We also tested country dummies. Country dummies also proved to be problematic, but not as problematic as month-year time dummies. Many countries in our dataset experienced only one outcome when attempting energy subsidy reforms. In other words, these countries showed no variation in the dependent variable. For social unrest as a dependent variable, 18 of the 43 countries in our dataset implemented reforms that produced only one outcome. Since the dummies for these countries predicted the outcome perfectly, all the episodes implemented by these 18 countries were omitted from the regression. In the case of reversal as a dependent variable, 23 of the 43 countries in our dataset implemented reforms with no variation in the reversal outcome. Since the dummies for these countries also perfectly predicted the outcome, all the episodes implemented by these 23 countries were omitted from the regression. Therefore, the inclusion of country dummies leads to a significantly less-powered regression, which is a substantial cost given the relatively small dataset size. However, we continued to estimate and compare models with country dummies.

3.3.2 Panel Logistic Regression

Our regression analysis could also be conducted by treating the dataset as a panel. Since some countries in our dataset implemented multiple episodes of energy subsidy reform in the same year, we set month-year as the time variable and country as the panel variable. This produced a very unbalanced panel in which only a few countries implemented any energy subsidy reforms in each month-year of the study period. Our 28-year study period (1995–2022) includes a total of 336 month-years, and reforms were implemented in 211 of these month-years, with no reforms implemented in the remaining 125 month-years. Furthermore, the average number of energy subsidy reforms implemented by any country in each month-year (across the 211) was only 1.9. Nevertheless, we proceeded with running panel regressions, and the panel versions of Equation [1] and Equation [2], which include the unobserved effects for countries (Wooldridge 2010), are respectively shown as follows:

$$logit(social\,unrest_{jt}) = \alpha_0 + \alpha_1 GAS_{jt} + \alpha_2 DSL_{jt} + \alpha_3 LPG_{jt} + \alpha_4 KER_{jt} + \alpha_5 ELC_{jt} + \alpha_6 COMP_{jt} + \alpha_7 ln(POP_{jt}) + \alpha_8 GDPG_{jt} + \alpha_9 \ln(GDPPC_{jt}) + \alpha_{10} GE_{jt} + \alpha_{11} RQ_{jt} + \alpha_{12} VA_{jt} + \alpha_{13} PSNV_{jt} + \alpha_{14} \mathrm{RL}_{jt} + \alpha_{15} CC_{jt} + \alpha_{16} RT_{jt} + \alpha_{17} CF_{jt} + \alpha_{18} HDI_{jt} + \alpha_{19} INF_{jt} + \alpha_{20} XM_{jt} + c_j + \varepsilon_{jt} \quad [5]$$

$$logit(reversal_{jt}) = \beta_0 + \beta_1 GAS_{jt} + \beta_2 DSL_{jt} + \beta_3 LPG_{jt} + \beta_4 KER_{jt} + \beta_5 ELC_{jt} + \beta_6 COMP_{jt} + \alpha_7 ln(POP_{jt}) + \beta_8 GDPG_{jt} + \beta_9 \ln(GDPPC_{jt}) + \beta_{10} GE_{jt} + \beta_{11} RQ_{jt} + \beta_{12} VA_{jt} + \beta_{13} PSNV_{jt} + \beta_{14} \mathrm{RL}_{jt} + \beta_{15} CC_{jt} + \beta_{16} RT_{jt} + \beta_{17} CF_{jt} + \beta_{18} HDI_{jt} + \beta_{19} INF_{jt} + \beta_{20} XM_{jt} + c_j + \varepsilon_{jt} \quad [6]$$

The subscript *j* denotes the country, in contrast to the subscript *i* used previously to denote episodes of energy subsidy reform. The subscript *t* denotes the time interval, which is month-year. The unobserved effects (or unobserved heterogeneity) are denoted by c_j. The random effects and fixed effects estimators were both used, which we ran using the *xtlogit* command in Stata 16.0 (StataCorp 2019).

4. Results

4.1 Logistic Regression Results: Social Unrest as the Dependent Variable

4.1.1 Selecting a Final Model for Social Unrest

We started by estimating the general model with and without time dummies, country dummies, and both sets of dummies using the pooled estimator. We then compared the pooled estimator to the panel fixed effects and random effects estimators. Various test statistics were monitored while testing down from the general model to the purposeful selection model to the final preferred parsimonious model. A complete description of this procedure, and all the estimated models, is available in Appendix A.

Table 6 shows the sensitivity and specificity of the final social unrest model. These two classification statistics reflect the shares of actual positives (i.e., the occurrence of social unrest) and actual negatives (i.e., no social unrest) that are correctly identified by the final model. The rate of correctly identifying the occurrence of social unrest is 57.0%, while the rate of correctly identifying the absence of social unrest is 84.9%. The superior detection of the absence of social unrest stems from its larger group size, as there were 142 episodes that led to social unrest and 258 episodes that did not. When the distribution of outcomes is unbalanced, studies have shown that the default approach of using a cut-off probability of 0.5 for assigning a successful or unsuccessful outcome does not work well (e.g., Freeman and Moisen 2008). Hosmer, Lemeshow, and Sturdivant (2013) recommended choosing a cut-off point where the sensitivity and specificity curves approximately cross (i.e., have similar values). By changing the cut-off to 0.378, the rate of correctly identifying the occurrence of unrest rises from 57.0% to 75.4%, at the cost of a smaller deterioration in the rate of correctly identifying the absence of unrest from 84.9% to 75.2%. Finally, we measured the area under the receiver operating characteristic (ROC) curve, which plots the sensitivity against 1 minus the specificity for a range of cut-off values. We measured the area under the ROC curve to be 0.82, indicating a final social unrest model that is strong at predicting outcomes correctly (Hosmer Lemeshow, and Sturdivant 2013).

Table 6. Classification statistics for our preferred final social unrest model.

Final model	Value (at default cut-off of 0.5)	Value (at cut-off of 0.378)
Sensitivity	57.04%	75.35%
Specificity	84.88%	75.19%
Correctly classified	75.00%	75.25%

4.1.2 Final Social Unrest Model: Results and Discussion

Table 7 presents the results from the final preferred model in terms of odd ratios for easier interpretation (Uberti 2022). It shows that for gasoline, a one-unit percentage increase in its price during an episode of reform raises the odds of social unrest by 0.57%.[8] In the case of kerosene, a one-unit percentage increase in its price raises the odds of social unrest by 1.26%. These results reveal that extensive increases in the prices of both fuels during a reform, particularly kerosene, can make social unrest much more likely to occur, so countries should reform energy prices gradually. Kerosene's importance to lower-income households, with it sometimes being referred to as the 'poor man's fuel' (MacRae 2008), may explain its relatively stronger effect on the occurrence of social unrest. (The coefficient on kerosene was also very robust, being consistently significant in every single model during the general-to-specific testing down procedure).

Table 7. Odds ratios for the final social unrest model.

Dependent variable (DV): social unrest	Final model
Gasoline % point increase (GAS)	1.0057*
Diesel % point increase (DSL)	0.9953*
Kerosene % point increase (KER)	1.0126***
Annual GDP growth in % points (GDPG)	0.9095***
Log population (POP)	1.5347***
Civil freedom (CF)	2.6849***
Human Development Index (HDI)	0.9451***
Constant	0.0012***

Surprisingly, for diesel, the preferred model shows that a one-unit percentage increase in its price reduces the odds of social unrest by 0.47%. Diesel is a fuel that tends to be consumed by specific groups, like truck operators, farmers, or fishermen – and it varies from country to country. We offer two hypotheses for this unexpected result. First, it is possible that governments directly or indirectly compensate these specific groups of diesel users when raising diesel prices. For example, the government may offer some form of indirect support for farmers to compensate them for higher diesel prices. These instances of group-specific compensation are not captured in our dataset, so the negative coefficient on the diesel price increase may be indirectly capturing the impact of group-specific compensation.[9] Our second hypothesis relates to diesel price increases being implemented as part of a wider economic transformation. Qualitative studies have suggested that comprehensive energy subsidy reforms that are positioned as part of broader economic transformations may be more likely to succeed (Beaton et al. 2013; Whitley and van der Burg 2018).

Looking beyond the energy-price-related variables, the regression analysis points to the importance of economic performance to the success of energy subsidy reform. Table 7 reveals that a one-unit percentage increase in real GDP reduces the odds of unrest by 9.05%. This result suggests that governments should take advantage of periods of fast economic growth to implement subsidy reforms, and it is in line with a recent study by Kollias and Tzeremes (2022), which demonstrated a relationship between economic downturns in Middle Eastern and Central Asian economies and general (not necessarily energy-related) social unrest.

The regression results also suggest that social unrest following energy subsidy reform is more likely to occur in countries with larger populations. Given that the population variable enters our equation in natural logs, our regression reveals that an *e*-fold (i.e., 2.78-fold) increase in the population leads to a 53.47% increase in the odds of unrest. We hypothesize that countries with larger populations might be more likely to carry the conditions needed to trigger unrest. This result suggests that countries with larger populations may need to design their energy subsidy reforms more carefully.

Unsurprisingly, we find that civil freedom has a very strong impact on whether social unrest occurs following reform. Our results show that a one-unit increase in the civil freedom index, which varies from a low of 1 to a high of 4, increases the odds of unrest by a factor of 2.68. This large increase stems from the fact that countries that provide their citizens with the freedom of expression and assembly to protest government actions are more likely to experience social unrest after reforms than countries where citizens are banned from doing so.

Our analysis also reveals that a one-unit increase in the HDI, which was scaled to vary from 0 to 100, reduces the odds of social unrest by 5.49%. This result shows that more developed countries, which have healthier, more educated populations with higher standards of living, are less likely to trigger social unrest when implementing energy subsidy reforms. On the other hand, less developed countries, particularly the least developed which exhibit the lowest HDI values, will likely have greater difficulties implementing successful reforms. To succeed, such countries will likely need to adopt a much more gradual approach, or take advantage of periods of rapid economic growth, to improve their chances of avoiding social unrest.

The absence of compensation in the final model, which only included statistically significant explanatory variables, was surprising. Multiple qualitative studies have discussed the importance of compensatory measures for mitigating the adverse impacts of energy subsidy reforms on households, and such measures have been listed as key enablers of success (e.g., Laan, Beaton, and Presta 2010; Commander 2012; Beaton et al. 2013; Vagliasindi 2013; Clements et al. 2013; Clements et al. 2014; Rentschler and Bazilian 2017a, 2017b). Our result likely stems from our analysis not capturing how effectively compensation was implemented. For example, in one country, effective compensation design might have prevented unrest, while in another country poor compensation design might not have done so. Two important aspects of compensation design relate to coverage and timing. In countries where compensation covered most lower-income households and was delivered before the reform, it likely prevented unrest. In countries where compensation coverage was limited and implemented after reform, it likely did not prevent unrest. Unfortunately, we were unable to obtain information related to the timing or coverage of compensation schemes for most episodes, so our compensation variable captures all instances of compensation, regardless of how well each one was implemented. We hypothesize that this contributed to the lack of a statistically significant coefficient for compensation in the final model.

4.2 Logistic Regression Results: Reform Reversal as the Dependent Variable

4.2.1 Selecting a Final Model for Reform Reversal

We started by estimating the general model with and without time dummies, country dummies, and both sets of dummies using the pooled estimator. We then compared the pooled estimator to the panel fixed effects and random effects estimators. Various test statistics were monitored while testing down from the general model to the purposeful selection model to the final preferred parsimonious model. A complete description of this procedure, and all the estimated models, is available in Appendix B.

Table 8 shows the sensitivity and specificity of the final reform reversal model. While the rate of correctly identifying the absence of a reversal is over 99%, the rate of correctly identifying its occurrence is only 2.1%. The much stronger results for predicting the absence of a reversal stem from its much larger group size, as there were only 48 energy subsidy reform episodes that produced a reversal, and 352 episodes that did not. Given that our observations on reform reversal are heavily unbalanced, we explored the use of different cut-off points for the classification tests. As discussed previously, Hosmer, Lemeshow, and Sturdivant (2013) recommended choosing a cut-off point where the sensitivity and specificity curves approximately cross (i.e., have similar values). By changing the cut-off to 0.14, the rate of correctly identifying the occurrence of a reversal rises from 2.1% to 66.7%, at the cost of a smaller deterioration in the rate of correctly identifying the absence of a reversal from 99.7% to 65.6%. Finally, we measured the area under the ROC curve to be 0.72, indicating a final model that is able to predict outcomes correctly (Hosmer, Lemeshow, and Sturdivant 2013).

Table 8. Classification statistics for the preferred reform reversal model.

Final model	Value (at default cut-off of 0.5)	Value (at cut-off of 0.144)
Sensitivity	2.08%	66.67%
Specificity	99.72%	65.63%
Correctly classified	88.00%	65.75%

4.1.2 Final Reform Reversal Model: Results and Discussion

For easier interpretation, Table 9 presents the results from the final preferred model in terms of odd ratios. It shows that a one-unit percentage increase in the price of kerosene during an episode of energy subsidy reform raises the odds of a reversal by 0.61%. As discussed previously, kerosene is well-known for its importance to lower-income households, so increases in its price have a strong impact on energy poverty, possibly leading to either political pushback or the widespread type of social unrest that forces a policy reversal.

Table 9. Odds ratios for the final reform reversal model.

Dependent variable (DV): reform reversal	Final model
Kerosene % increase (KER)	1.0061**
Annual GDP growth in % points (GDPG)	0.9350*
Civil freedom (CF)	1.9713**
Human Development Index (HDI)	0.9471***
Constant	0.8207

According to the final model, other fuels do not have statistically significant effects on the occurrence of a reversal, although there were instances of reversals in a few countries that appear to have been directly driven by gasoline or LPG price increases. In fact, when using fixed effects, the LPG price increase variable emerges as having a statistically significant effect in increasing the odds of a reversal, but this effect disappears in our final model when using the pooled estimator. The lack of a consistently significant effect for fuels other than kerosene may stem from the relatively small number of observations that led to a reversal. As such, there was not enough statistical power to identify statistically significant effects for price increases in those other fuels. Nevertheless, our results for kerosene suggest that large increases in the prices of fuels vital to lower-income households, which can vary from country to country, can lead to considerably higher odds of a reversal, and also violent unrest, which triggers such reversals.[10]

As was the case with the social unrest model, our results point to the importance of economic performance for the success of energy subsidy reform. Table 9 reveals that a one-unit percentage increase in real GDP reduces the odds of a reversal by 6.50%, a similar result to the 9.05% reduction in the odds of social unrest. These results demonstrate that governments should take advantage of periods of fast economic growth to implement energy subsidy reforms for better odds of success, whether from the perspective of avoiding social unrest or a policy reversal.

As was the case with the social unrest model, our results demonstrate that civil freedom has a very strong impact on whether a reversal occurs following reform. The results show that a one-unit increase in our civil freedom index, which varies from a low of 1 to a high of 4, increases the odds of a reversal twofold (compared to a 2.7-fold increase from the final social unrest model). This large increase likely stems from the fact that countries that provide their citizens with the freedom of assembly to protest government actions are more likely to experience protests that can escalate to the point of forcing governments into a reversal.

Our analysis also reveals that a one-unit increase in the HDI, which was scaled to vary from 0 to 100, reduces the odds of a reversal by 5.29% (compared to a reduction of 5.49% for the odds of unrest). These results suggest that more developed countries, which have healthier, more educated populations with higher standards of living, may have fewer people facing energy poverty (Halkos and Gkampoura 2021), and thus fewer people to trigger the violent type of unrest that often culminates in a policy reversal. These results underscore the difficulties that developing countries, particularly the least developed countries with the lowest HDI values, will likely have in implementing successful energy subsidy reforms compared to more developed countries with higher HDI values.

5. Conclusion

Despite extensive attempts to reform energy subsidies, many countries continue to face challenges in achieving successful outcomes, with many of those attempts leading to social unrest or a subsidy reversal, two unwelcome outcomes for policymakers. These challenges may partially explain the currently enormous potential for further reform, with existing subsidies estimated by the IEA and IMF to be over US $1 trillion (Black et al. 2023; IEA 2024). To overcome these challenges, policymakers need a better understanding of the national circumstances that influence energy subsidy reform outcomes, particularly through research that applies quantitative methods – an area where there is a big gap in the literature.

This paper contributes to a better understanding of the determinants of energy subsidy reform outcomes by applying logistic regression analysis on an original dataset that captures 400 distinct energy subsidy reform episodes implemented across 43 different countries between 1995 and 2022. Through our regression analysis, we quantified the effects of 20 explanatory variables on the odds of two key outcomes: whether social unrest occurs following an energy subsidy reform and whether it gets reversed. The 20 explanatory variables include increases in various energy prices alongside variables related to a country's economic performance, its level of human development, its governance, and its institutional quality, among other variables.

Our logistic regression results for social unrest as a dependent variable reveal that gasoline and kerosene price increases are two key determinants of social unrest. For example, we find that a one-unit percentage increase in gasoline and kerosene prices during an episode of energy subsidy reform raises the odds of social unrest by 0.57% and 1.26%, respectively. Therefore, a doubling of the gasoline or kerosene price, or of both fuels simultaneously, which appears to have happened many times during past reforms, can sharply increase the odds of social unrest. These results point to the importance of implementing reforms gradually, giving consumers time to adapt to price changes. It also points to the potential benefits of staggering energy subsidy reforms, such that each wave or step of reform focuses on increasing the price of an important fuel like gasoline while holding the price of another important fuel like kerosene fixed. Our quantitative results align with the qualitative literature, which suggests that gradual energy subsidy reforms are more likely to be accepted by the public (e.g., Beaton et al. 2013; Clements et al. 2013; Rentschler and Bazilian 2017a). Our logistic regression results for reform reversal as a dependent variable reveal that larger increases in kerosene prices in particular – a key fuel for lower-income households in many countries – increase the odds of a reversal. This points, again, to the importance of gradual reforms to minimize the occurrence of both social unrest and a policy reversal. These results also validate the hypothesis previously discussed in the literature review, which is that changes in domestic energy prices are key drivers of energy subsidy reform outcomes.

Another important determinant of both social unrest and reform reversal is annual GDP growth. We find that a one-unit percentage increase in real GDP reduces the odds of a reversal by 6.50% and the odds of social unrest by 9.05%. Therefore, for countries preparing to implement energy subsidy reforms, decision makers will likely have a better chance of achieving a successful outcome when launching those reforms during periods of rapid economic growth, which will likely vary among oil exporters and oil importers. This result aligns with the qualitative literature that discusses the importance of timing (e.g., Clements et al. 2013; El-Katiri and Fattouh 2017).

Our results reveal some of the other key national circumstances that can influence energy subsidy reform outcomes. Important determinants of social unrest include population size, civil freedom, and the level of human development. Regarding reform reversal, we find civil freedom and the level of human development to be important determinants.

While all countries have agreed to the phase-out of inefficient fossil fuel subsidies at COP26 to help meet the goals of the Paris Agreement, our results demonstrate that policymakers in developing countries with larger populations and lower levels of human development, for example, will face much greater difficulties in successfully reforming energy subsidies. These results link closely to the principle of “common but differentiated responsibilities and respective capabilities, in the light of different national circumstances” under the Paris Agreement (UNFCCC 2015). Perhaps increased support from developed countries to those developing countries that face the greatest challenges in reforming energy subsidies can help improve the odds of them doing so successfully. Alternatively, those developing countries may need to explore alternative policy options to achieve their climate goals or work to improve their national circumstances and capabilities before considering subsidy reform.

To summarize, we shed light on the factors and national circumstances that influence energy subsidy reform outcomes. Our analysis yields important insights that can help policymakers design and implement energy subsidy reforms in a way that minimizes the occurrence of negative outcomes, while helping countries better understand the size of the barriers they face, given their national circumstances.

Endnotes

[1] Energy subsidy reform is often referred to as energy price reform since energy subsidies capture the underpricing of energy (Clements et al. 2013; Coady et al. 2018).

[2] These operators include Boolean operators such as AND and OR, alongside specialized search operators. For instance, in Nexis, the HLEAD (gasoline AND price) command was used to search for news articles that contain the terms “gasoline” and “price” in either the title or lead paragraph. Nexis also provides the W/N connector, which is identical to the NEAR/N operator in ProQuest. This proximity operator ensures that terms appear within N number of words of each other.

[3] For most countries, the value of the polity2 variable does not change at all or changes very slowly over time. For example, the value for Qatar remains fixed between 1971 and 2018. However, in rare cases, the value can change rapidly due to political upheavals. In the case of Myanmar, we extended the last value of the polity2 variable to 2020. However, in 2021, the military overthrew the democratically elected government (U.S. Department of State 2024), “undoing a decade of progress,” so we set the values for 2021 and 2022 equal to the values that prevailed a decade earlier between 2011 and 2014 during the period of the military-backed government.

[4] The variables are defined as follows: 1) Voice and accountability captures perceptions of the extent to which a country’s citizens are able to participate in selecting their government, as well as freedom of expression, freedom of association, and a free media (World Bank 2023); 2) political stability and the absence of violence/terrorism measures perceptions of the likelihood of political instability and/or politically-motivated violence, including terrorism (World Bank 2023); 3) government effectiveness captures perceptions of the quality of public services, the quality of the civil service and the degree of its independence from political pressures, the quality of policy formulation and implementation, and the credibility of the government’s commitment to such policies (World Bank 2023); 4) regulatory quality captures perceptions of the government’s ability to formulate and implement sound policies and regulations that permit and promote private sector development (World Bank 2023); 5) rule of law captures perceptions of the extent to which agents have confidence in and abide by the rules of society, and, in particular, the quality of contract enforcement, property rights, the police, and the courts, as well as the likelihood of crime and violence (World Bank 2023); 6) control of corruption captures perceptions of the extent to which public power is exercised for private gain, including both petty and grand forms of corruption (World Bank 2023).

[5] The number of implemented reforms in our dataset also depends on the level of news coverage that each country receives. There may have been reform episodes that were not captured in our searches of Nexis, ProQuest, and Google due to coverage-related issues. For example, news on energy subsidy reforms in some countries may have only been published in languages other than English and would thus not be captured in our search.

[6] Our dataset on energy subsidy reform episodes only encompasses episodes of energy price changes in countries with subsidies (i.e., countries with regulated energy prices). Once a country fully deregulates its energy prices, any future energy price changes, which would occur in line with international market prices, would not be relevant to the analysis conducted in this paper.

[7] A mean value for this binary variable that is greater than 0.5 indicates a higher percentage of exporters, while a mean value less than 0.5 indicates a lower percentage of exporters.

[8] While this figure may appear small, it is important to note that during episodes of energy subsidy reform, increases in fuel prices tend to be much larger than 1%. For example, a 100% increase in the gasoline price would raise the odds of unrest by 77.2% (obtained by taking the exponential of 100 multiplied by the gasoline coefficient estimated in Table 9).

[9] Our compensation variable only captures instances of economy-wide cash transfer compensation schemes for lower- to middle-income households.

[10] We did not distinguish between different levels of social unrest in this paper. Future work could focus on building a social unrest variable with varying levels of intensity.

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Appendix

Appendix A

The logistic regression results for the general model are shown in Table A1, starting with the pooled general model with no dummy variables, given by Equation [1]. We then tested the inclusion of year dummies, finding none to be statistically significant but the set of them to be jointly significant. We then tested the inclusion of country dummies and ran into the previously discussed issue, where half of the countries were dropped due to their dummies perfectly predicting the outcome. With only half the countries remaining, we found that the country dummies were jointly significant. We also tested the inclusion of both sets of dummies. With half the countries being dropped again, the country dummies were found to be jointly significant, while the time dummies were not. While the p-values suggest that country dummies should be included, doing so comes at the cost of losing half of all countries and a quarter of all observations – a substantial cost given the relatively small sample size. Additionally, all the general models in Table A1 suffered from severe multicollinearity, with five explanatory variables exhibiting variance inflation factors (VIFs) greater than 10. Furthermore, some of the large estimated coefficients indicate the possibility of overfitting issues (Hosmer, Lemeshow, and Sturdivant 2013).

We also estimated panel data models, finding that the panel-level variance component, denoted by rho, was zero. The LR test of whether rho equals zero was used to compare the pooled estimator to the panel estimator for the general model. The LR test produced a p-value of 0.262, failing to reject the null hypothesis that rho equals zero. This test result suggests that the pooled estimator should continue to be used in the regression analysis.

Having established that the pooled estimator is preferred and that there may be overfitting issues with the general model, we proceeded to estimate Equation [3], which was obtained by purposeful selection. The results are shown in Table A2 with different sets of dummy variables, starting with the purposeful selection model with no dummies. We then tested the inclusion of year time dummies, finding them to be jointly significant. We then tested the inclusion of country dummies and ran into the previously discussed issue of half the countries being dropped but found the country dummies to be significant. We also tested the inclusion of time and country dummies, finding that doing so led to each set of dummies no longer being jointly significant. Unlike the general model, the purposeful selection model results suggest that all dummies can be dropped from the regressions. However, the purposeful selection model, despite including fewer explanatory variables than the general model, continued to suffer from severe multicollinearity, with several variables exhibiting VIF values greater than 10.

We subsequently tested down from the purposeful selection model, following the general-to-specific approach, to look for a more parsimonious model. Through this procedure, three models were obtained, which are shown in Table A3. Model 1 includes eight statistically significant explanatory variables alongside the constant. Model 2 resembles model 1 but excludes political stability as an explanatory variable due to possible endogeneity issues (Natalini, Bravo, and Newman 2020). Unlike models 1 and 2, model 3 excludes the population and instead includes GDP per capita as a statistically significant explanatory variable. All explanatory variables across all three models were significant at the 10% level, with only the gasoline price increase in model 3 having a borderline p-value of 0.101. All three parsimonious models did not have any multicollinearity issues, in contrast to the general and purposeful selection models.

We compared the general, purposeful selection, and three parsimonious models presented in Table A1 through Table A3 using various goodness-of-fit statistics. According to Akaike's Information Criterion (AIC), the purposeful selection model that includes country and time dummies is the superior model, which had the lowest AIC of 352.9. However, if we exclude models with country dummies, which led to a significant loss in the number of observations, then model 1 emerges as the superior model, with an AIC of 415.1, followed closely by model 2 with an AIC of 417.1. However, when using the Bayesian Information Criterion (BIC), model 2 emerges as the superior model, with the lowest BIC of 449.0 among all models, including the ones with country dummies.

Proceeding with parsimonious model 2 for further analysis, we tested for time and country dummies. Time dummies were not jointly significant, but country dummies were. Including country and time dummies together revealed both sets to be jointly significant, but this again comes with the loss of many observations.

We also estimated model 2 using fixed effects and random effects panel estimators. Table A4 demonstrates that the results obtained using the pooled estimator and the random effects panel estimator are very similar. However, differences were observed in the results obtained with the fixed effects panel estimator, which caused the variables for annual GDP growth, population, and human development to no longer be statistically significant, while increasing the statistical significance of all the price increase variables. Furthermore, the use of fixed effects (i.e., country dummies) led to the same loss of observations discussed previously. Using the Hausman test to compare fixed and random effects, we found that the random effects estimator was preferred over fixed effects. As noted by Wooldridge (2010), in cases where the explanatory variables do not vary much over time, as is the case here for variables like civil freedom, fixed effects estimators can lead to imprecise estimates. Our analysis thus leaves us with a choice between the pooled and random effects estimators. We proceeded with the pooled estimator since it (1) is consistent, even when strict exogeneity does not hold (Söderbom et al. 2015), (2) was used throughout the general-to-specific testing down procedure, and (3) produces very similar results to random effects. To summarize, we proceeded with model 2 (using the pooled estimator) as our final model.

Table A1. Estimated general models (using pooled estimator) and goodness-of-fit tests with different sets of dummy variables.

Dependent variable: social unrest	**General model (no dummies)**	**General model (+ time dummies)**	**General model (+ country dummies)**	**General model (+ country and time dummies)**
Gasoline price % increase (GAS)	0.00786**	0.00920**	0.02488***	0.030165***
Diesel price % increase (DSL)	−0.00523**	−0.00582**	−0.01989***	−0.02552***
LPG price % increase (LPG)	−0.00502*	−0.00447	−0.00112	0.004562
Kerosene price % increase (KER)	0.01566***	0.02217***	0.04345***	0.064434***
Electricity price % increase (ELC)	0.00161	0.00103	0.00168	0.00086
Compensation (COMP)	0.42942	−0.71077	−0.29184	−2.09867**
Annual % GDP growth (GDPG)	−0.07430**	−0.04545	−0.04589	0.014797
Log annual GDP per capita (GDPPC)	−0.26213	−0.59165	9.1785***	6.729288*
Log population (POP)	0.41800***	0.21554	8.60380***	−10.0498
Government effectiveness (GE)	−0.75053	0.89214	−1.66506	−1.29672
Regulatory quality (RQ)	0.88861	1.16300*	0.16635	3.303677*
Voice and accountability (VA)	−0.58678	−0.06993	−0.79828	−1.15556
Political stability (PS)	−0.06997	−0.64974*	0.65252	0.155384
Rule of law (RL)	−0.43563	−0.80948	0.82590	0.417901
Control of corruption (CC)	−0.10665	−0.60761	−1.97255	−5.1005***
Regime type (RT)	0.06086	0.09234	−0.01839	0.061839

Table A1. (continued)

Dependent variable: social unrest	General model (no dummies)	General model (+ time dummies)	General model (+ country dummies)	General model (+ country and time dummies)
Civil freedom (CF)	1.33472*	1.05250	3.64974***	4.307967**
Human Development Index (HDI)	−0.01886	−0.05462*	−0.52560***	−1.11304***
Annual % inflation (INF)	−0.00028	0.00465	−0.00173	0.008189
Exporter or importer (XM)	0.45559	1.28739***	−1.26680	−0.07916
Constant	−8.72508*	0.07923	−196.16510***	180.6621
likelihood ratio (LR) chi-square test	chi2(20) = 140.08***	chi2(46) = 189.28***	chi2(44) = 144.06***	chi2(70) = 202.89***
Pseudo R-squared	0.2692	0.3656	0.3433	0.4876
AIC	422.3	422.5	365.6	419.66
BIC	506.1	609.7	533.0	503.06

Table A2. Estimated purposeful selection models (using pooled estimator) and goodness-of-fit tests with different sets of dummy variables.

Dependent variable: social unrest	**Purposeful selection model (no dummies)**	**Purposeful selection model (+ time dummies)**	**Purposeful selection model (+ country dummies)**	**Purposeful selection model (+ country and time dummies)**
Gasoline price % increase (GAS)	0.004558	0.005553*	0.023625***	0.028154***
Diesel price % increase (DSL)	−0.00386	−0.00415	−0.01932***	−0.02338***
LPG price % increase (LPG)				
Kerosene price % increase (KER)	0.011567**	0.015787***	0.041737***	0.05858***
Electricity price % increase (ELC)				
Compensation (COMP)				
Annual % GDP growth (GDPG)	−0.07696**	−0.04572	−0.0436	0.010954
Log annual GDP per capita (GDPPC)	−0.23871	−0.48642	9.378156***	7.384314**
Log population (POP)	0.431222***	0.249831	8.522376***	−7.77576
Government effectiveness (GE)	−0.78183	0.713868	−1.7099	−1.66092
Regulatory quality (RQ)	0.856772	1.11737*	0.16724	2.821102
Voice and accountability (VA)	−0.5233	−0.1056	−0.84084	−0.8951
Political stability (PS)	−0.01838	−0.55862*	0.649131	0.287619
Rule of law (RL)	−0.4933	−0.8846	0.733595	0.658714
Control of corruption (CC)	−0.02594	−0.55345	−2.00713	−4.83821*

Table A2. (continued)

Dependent variable: social unrest	Purposeful selection model (no dummies)	Purposeful selection model (+ time dummies)	Purposeful selection model (+ country dummies)	Purposeful selection model (+ country and time dummies)
Regime type (RT)	0.063412	0.085928	−0.02245	0.024307
Civil freedom (CF)	1.120961	1.026742	3.726872***	4.109005**
Human Development Index (HDI)	−0.02031	−0.06286**	−0.53795***	−1.00222***
Annual % inflation (INF)				
Exporter or importer (XM)	0.378288	1.079974**	−1.21084	−0.66305
Constant	−8.32662*	−0.09631	−196.027***	130.8016
likelihood ratio (LR) chi-square test	chi2(16) = 134.9***	chi2(42) = 184.6***	chi2(40) = 142.8***	chi2(66) = 197.1***
Pseudo R-squared	0.2593	0.3566	0.3404	0.4738
AIC	419.5	419.1	358.8	352.9
BIC	487.3	590.4	511.4	601.5

Table A3. Estimated parsimonious models (using pooled estimator) and goodness-of-fit tests.

Dependent variable: social unrest	**Model 1 (no dummies)**	**Model 2 (no dummies)**	**Model 3 (no dummies)**
Gasoline price % increase (GAS)	0.006144*	0.005725*	0.005700BL
Diesel price % increase (DSL)	−0.00479*	−0.00471*	−0.004505*
LPG price % increase (LPG)			
Kerosene price % increase (KER)	0.01198***	0.01253***	0.012250***
Electricity price % increase (ELC)			
Compensation (COMP)			
Annual % GDP growth (GDPG)	−0.08192**	−0.09489**	−0.09338***
Log annual GDP per capita (GDPPC)			−0.59564***
Log population (POP)	0.33289***	0.42833***	
Government effectiveness (GE)			
Regulatory quality (RQ)			
Voice and accountability (VA)			
Political stability (PS)	−0.39165**		
Rule of law (RL)			
Control of corruption (CC)			
Regime type (RT)			
Civil freedom (CF)	1.18197***	0.98764 ***	0.88609***
Human Development Index (HDI)	−0.04538***	−0.05647***	−0.03274**
Annual % inflation (INF)			
Exporter or importer (XM)			
Constant	−6.62457***	−6.69125***	4.17227***
likelihood ratio (LR) chi-square test	chi2(8) = 123.3***	chi2(7) = 119.32***	chi2(7) = 99.37***
Pseudo R-squared	0.2369	0.2293	0.1910
AIC	415.1	417.1	437.0
BIC	451.0	449.0	469.0

Note: BL = borderline statistically significant.

Table A4. Final model 2 logistic regression results using different estimators.

Dependent variable: social unrest	**Model 2 (pooled estimator)**	**Model 2 (random effects)**	**Model 2 (fixed effects)**
Gasoline price % increase (GAS)	0.005725*	0.006979**	0.020079***
Diesel price % increase (DSL)	−0.00471*	−0.006105**	−0.016814***
LPG price % increase (LPG)			
Kerosene price % increase (KER)	0.01253***	0.01604***	0.03834***
Electricity price % increase (ELC)			
Compensation (COMP)			
Annual % GDP growth (GDPG)	−0.09489***	−0.07014*	−0.05952
Log annual GDP per capita (GDPPC)			
Log population (POP)	0.42833***	0.56890***	1.24134
Government effectiveness (GE)			
Regulatory quality (RQ)			
Voice and accountability (VA)			
Political stability (PS)			
Rule of law (RL)			
Control of corruption (CC)			
Regime type (RT)			
Civil freedom (CF)	0.98764***	1.33263***	2.15625***
Human Development Index (HDI)	−0.05647***	−0.03872**	0.03979
Annual % inflation (INF)			
Exporter or importer (XM)			
Constant	−6.69125***	−11.32963***	
LR / Wald chi-square test	chi2(7) = 119.32***	chi2(7) = 38.25***	chi2(7) = 38.05***
Pseudo R-squared	0.2293	N/A	0.1367
AIC	417.1	410.5	254.3
BIC	449.0	446.4	280.3

Appendix B

The logistic regression results for reform reversal as a dependent variable are shown in Table B1, starting with a general model with no dummy variables, given by Equation [2]. The regression revealed that compensation had to be omitted, as the variable perfectly predicted the occurrence of a reversal. In other words, all episodes of reform that culminated in a reversal did not include cash compensation, indicating that the absence of cash compensation is strongly associated with reversals. We then tested for the inclusion of year time dummies, which were not jointly significant. And, after losing half the countries from the sample, country dummies were also not jointly significant. Testing all dummy variables together, we confirmed that neither set was jointly significant. As was the case with the general models for social unrest, the general models for reform reversal may also be suffering from potential overfitting issues (Hosmer, Lemeshow, and Sturdivant 2013).

We also estimated panel data models. We found that the panel-level variance component, denoted by rho, was zero for the panel general model. The LR test of whether rho equals zero produced a p-value of 0.405, indicating that we should continue using the pooled estimator.

Having established that the pooled estimator is preferred and that there may be overfitting issues with the general models, we proceeded to estimate Equation [4], which was obtained by purposeful selection. The results are shown in Table B2 for different sets of dummy variables. We started with a model with no dummies and then tested the inclusion of time and country dummies, first separately and then together. In all cases, the dummies were not jointly significant. As was the case with the general model for reversal, our purposeful selection regression results suggest that the dummies can be dropped. However, the purposeful selection model, despite including fewer explanatory variables than the general model, continued to suffer from severe multicollinearity, with a few explanatory variables exhibiting VIF values greater than 10.

We subsequently tested down from the purposeful selection model, following the general-to-specific approach, to look for a more parsimonious model. Compared to social unrest as a dependent variable, it proved more difficult to find multiple parsimonious models for reversal as a dependent variable that included only statistically significant explanatory variables. Table B3 shows only one parsimonious model, which did not suffer from any multicollinearity issues.

We compared the general, purposeful selection, and parsimonious models (using the pooled estimator) presented in Table B1 through Table B3 using various goodness-of-fit statistics. According to the AIC, the purposeful selection model that includes country and time dummies is the superior model. However, if we exclude models with country dummies, model 1 emerges as the superior model, a result reinforced by examining the BIC.

Proceeding with parsimonious model 1 for further analysis, we tested for time and country dummies. Both sets of dummies were not significant, whether included separately or together. We also assessed the random effects and fixed effects panel estimators for model 1. Table B3 shows the regression results, highlighting differences across all three estimators. The use of fixed effects (i.e., country dummies) led to the same loss of observations discussed previously. The Hausman test to compare fixed and random effects showed that fixed effects were preferred, in contrast to the case for the final social unrest model. However, since the fixed effects estimator leads to the loss of many observations, and since we are interested in measuring the impacts of explanatory variables like civil freedom that vary very slowly over time (Wooldridge 2010), we proceeded with the pooled estimator for model 1 as our final model.

Table B1. Estimated general models (using pooled estimator) and goodness-of-fit tests with different sets of dummy variables.

Dependent variable: reform reversal	**General model (no dummies)**	**General model (+ time dummies)**	**General model (+ country dummies)**	**General model (+ country and time dummies)**
Gasoline price % increase (GAS)	−0.00596	−0.00900	−0.00153	0.02036
Diesel price % increase (DSL)	−0.00187	−0.00151	0.00457	−0.00994
LPG price % increase (LPG)	0.00493	0.00590	0.02023**	0.03017***
Kerosene price % increase (KER)	0.01071**	0.01342**	0.01404*	0.02776***
Electricity price % increase (ELC)	0.00110	−0.00080	0.00502	0.00721
Compensation (COMP)	Omitted	Omitted	Omitted	Omitted
Annual % GDP growth (GDPG)	−0.08573*	−0.09907*	−0.05256	0.00516
Log annual GDP per capita (GDPPC)	−0.74429	−0.89047	5.38800	2.59676
Log population (POP)	0.10487	0.09247	−0.49647	−7.03505
Government effectiveness (GE)	−0.78202	−0.85180	−1.98236	−5.55169*
Regulatory quality (RQ)	1.34444	1.59377*	0.52908	1.52811
Voice and accountability (VA)	0.30335	0.85044	−3.22766*	−7.72270
Political stability (PS)	−0.25654	−0.42853	0.32684	−0.09416
Rule of law (RL)	−0.63015	−1.06172	−0.01040	−0.52515
Control of corruption (CC)	0.53878	0.93175	1.73976	4.36665*
Regime type (RT)	−0.03881	−0.03852	−0.09607	−0.39608*
Civil freedom (CF)	0.53325	0.17957	1.63836	5.67764*

Table B1. (continued)

Dependent variable: reform reversal	General model (no dummies)	General model (+ time dummies)	General model (+ country dummies)	General model (+ country and time dummies)
Human Development Index (HDI)	−0.01944	−0.02326	−0.18877	−0.17945
Annual % inflation (INF)	0.00031	0.00682	0.00156	0.00175
Exporter or importer (XM)	1.15719**	1.57496**	0.11021	−0.80094
Constant	1.82451	5.46257	−0.80094	86.86301
likelihood ratio (LR) chi-square test	chi2(19) = 45.74	chi2(39) = 59.70	chi2(38) = 67.03	chi2(57) = 92.20
Pseudo R-squared	0.1558	0.2147	0.2785	0.4094
AIC	287.8	298.3	251.7	249.0
BIC	367.6	452.0	389.8	447.1

Table B2. Estimated purposeful selection models (using pooled estimator) and goodness-of-fit tests with different sets of dummy variables.

Dependent variable: reform reversal	**Purposeful selection model (no dummies)**	**Purposeful selection model (+ time dummies)**	**Purposeful selection model (+ country dummies)**	**Purposeful selection model (+ country and time dummies)**
Gasoline price % increase (GAS)				
Diesel price % increase (DSL)				
LPG price % increase (LPG)	0.00158	0.00121	0.02133**	0.02645***
Kerosene price % increase (KER)	0.00387	0.00482	0.01437**	0.02346**
Electricity price % increase (ELC)				
Compensation (COMP)	Omitted	Omitted	Omitted	Omitted
Annual % GDP growth (GDPG)	−0.06195	−0.04984	−0.04827	−0.02520
Log annual GDP per capita (GDPPC)	−0.09629	−0.05556	4.10238	3.13277
Log population (POP)	0.09061	0.05473	0.37036	−2.80128
Government effectiveness (GE)	0.00335	0.00467	−1.74014	−4.77108*
Regulatory quality (RQ)				
Voice and accountability (VA)	−0.07157	0.36181	−2.92667	−5.31482*
Political stability (PS)	−0.31536	−0.42990	0.56921	0.49186
Rule of law (RL)	−0.67868	−1.03986	−0.23356	−0.80601
Control of corruption (CC)	0.43726	0.59473	1.87642	3.82193
Regime type (RT)	−0.04164	−0.03465	−0.10996	−0.32030

Table B2. (continued)

Dependent variable: reform reversal	**Purposeful selection model (no dummies)**	**Purposeful selection model (+ time dummies)**	**Purposeful selection model (+ country dummies)**	**Purposeful selection model (+ country and time dummies)**
Civil freedom (CF)	1.45838*	1.28024	1.22613	3.15270
Human Development Index (HDI)	−0.03145	−0.03507	−0.16576	−0.22756
Annual % inflation (INF)				
Exporter or importer (XM)				
Constant	−4.74912	−2.76645	−36.35718	20.85207
likelihood ratio (LR) chi-square test	chi2(13) = 33.10***	chi2(33) = 44.63*	chi2(32) = 64.18***	chi2(51) = 86.76***
Pseudo R-squared	0.1128	0.0852	0.2666	0.3853
AIC	288.4	301.4	242.5	242.4
BIC	344.3	432.0	359.4	420.1

Table B3. Estimated parsimonious models (using different estimators) and goodness-of-fit tests.

Dependent variable: reform reversal	**Model 1 (pooled estimator)**	**Model 1 (random effects)**	**Model 1 (fixed effects)**
Gasoline price % increase (GAS)			
Diesel price % increase (DSL)			
LPG price % increase (LPG)			
Kerosene price % increase (KER)	0.00609**	0.00693**	0.01436**
Electricity price % increase (ELC)			
Compensation (COMP)			
Annual % GDP growth (GDPG)	−0.06725*	−0.06565	−0.05430
Log annual GDP per capita (GDPPC)			
Log population (POP)			
Government effectiveness (GE)			
Regulatory quality (RQ)			
Voice and accountability (VA)			
Political stability (PS)			
Rule of law (RL)			
Control of corruption (CC)			
Regime type (RT)			
Civil freedom (CF)	0.67871**	0.62132	−0.70446
Human Development Index (HDI)	−0.05433***	−0.05517***	−0.02102
Annual % inflation (INF)			
Exporter or importer (XM)			
Constant	−0.19760	−0.20926	
likelihood ratio (LR) chi-square test	chi2(4) = 27.90***	chi2(4) = 16.44***	chi2(4) = 9.88**
Pseudo R-squared	0.0950	N/A	0.0599
AIC	275.6	273.1	163.0
BIC	295.6	297.1	177.2

Notes

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Anwar is a Principal Fellow at KAPSARC in the Climate and Sustainability program. He is an energy and environmental economist with an engineering background and more than a decade of research and advisory experience, leading projects in energy demand, energy subsidy reform, greenhouse gas emission measurement, and carbon pricing. Anwar's research has been published in leading energy and environmental journals and has been cited by multiple media outlets, including the *Saudi Gazette, Asharq Al-Awsat, and Arab News*. Anwar holds an M.Sc. in Electrical Engineering from KAUST and a B.Eng. in the same field from the University of Liverpool. He is currently completing his Ph.D. in sustainable resources at UCL, supervised by Paolo Agnolucci and Paul Ekins.

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About the Project

This study is part of the project titled “Modeling Energy Demand, Greenhouse Gas Emissions, and the Impacts of Energy Price Reform.” The project aims to provide an in-depth understanding of the relationships between energy demand, GHG emissions, and energy prices, as well as how policymakers can effectively reform domestic energy prices. A key focus area of the project is to understand the factors that contribute to successful energy price reform outcomes.

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