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ARTICLE

Uncertain times: The causal effects of coups on national income

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Abstract

We use doubly robust difference-in-differences models to estimate the causal effect of successful coups on national incomes. We find that real per capita gross domestic product (GDP) decreases by 10%–12% 5 years after a coup and the effect has not begun to diminish at that point. When we investigate the economic and political mechanisms behind this outcome, we find that our result is mostly driven by a fall in investment and in the rule of law, along with an increase in repression. Given the size of the effect, preventing coups can be seen as a significant development issue, and though the international community has taken steps to discourage coups, further consideration of anticoup policies seems well-warranted.

Perhaps once thought to be a relic of the Cold War, coups are on the rise again. Sub-Saharan Africa has seen seven successful coups (and five unsuccessful ones) since 2021. There have also been recent coups attempts in Moldova, Kyrgyzstan, and Ukraine.[1] As we detail below, there is a voluminous literature on the causes of coups, but there is much less written about their economic effects. Coup-makers uniformly tell us that their actions were necessary to save the country from disaster, to protect or create democracy, and other high-minded reasons, but we have good reasons to doubt the veracity of these claims. We believe that, on average, coups increase uncertainty about future polices and that they reduce the quality of governance, both of which are likely bad for economic performance.

In this paper, we estimate the causal effect of coups on real per capita income, using data from 1950 to 2019. We find that per capita income falls by 10%–12% 5 years after a coup. This result is robust to sequentially dropping regions of the world, it is not driven by coups from the cold-war era, and it is not driven by the 20% of our cases that are nonmilitary coups.

We then study six potential economic and political mechanisms that may be driving this result. More specifically, we first examine whether coups cause significant decreases in economic variables important to income (such as domestic investment, foreign direct investment [FDI], and foreign aid flows). Later, we explore the effect of coups on governance and civil society measures (such as the rule of law, freedom of speech, and the protection of property rights).

On the economic side, the main mechanism is a decline in domestic investment. We believe that coups create uncertainty about policies and tax regimes, which delays or prevents new investment and lowers gross domestic product (hereafter GDP). On the governance side, we find that coups significantly reduce the rule of law and increase repression (lower freedom of speech). Less formally, we also examine the rate of constitutional "events" before and after coups for our treated cases, finding massive increases in constitutional suspensions, interim constitutions, and reinstatement of previous constitutions. This phenomenon likely also raises uncertainty about the rules that will be in place.

Our approach to causal inference is three-pronged. First, we address endogeneity/parallel trends issues by studying cases where there is a substantial pretreatment period (10 years) that is coup-free. In this way, we can test whether economic performance in the treated countries begins declining before the coup occurs,

[1] And of course, there are the events of January 6, 2021 in the United States as well.

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which would be a strong indication of reverse causality.[2] This pretreatment period also helps ensure that the effects we find from the coup under study are not contaminated by lingering effects of recent previous coups. Second, we deal with the possibility that countries that suffer a coup may be quite different from countries that do not experience a coup. We address the problem of extreme counterfactuals (King & Zeng, 2006) by weighting the data so that the treated and untreated units are as similar as possible for a range of chosen covariates.

Third, we take care not to use early treated units as controls for later treated ones. It is this type of "forbidden" comparison that creates the bias issues that are becoming well-known in the use of traditional two-way fixed effects models to estimate treatment effects. Specifically, we use a newer difference-in-differences technique (Cengiz et al., 2019) that accounts for staggered treatment and produces an unbiased estimate of the treatment effects. As a bonus, the technique we use can be said to be "doubly robust" (Callaway & Sant'Anna, 2021; Sant'Anna & Zhao, 2020) in that if *either* the weighting equation *or* the outcome are correctly specified, the estimate of the treatment effect will be unbiased.[3]

It is important to state clearly what is being estimated and what the applicability of the estimate is to the world at large. We require a relatively long pretreatment (no-coup) period both to make sure previous coups are not actually causing effects we erroneously would attribute to the coup under study and to allow us examine pretrends for evidence of reverse causality. This requirement leaves us with a maximum of 65 coups to analyze.[4] Given these strictures, we are studying the average treatment effect of the first coup in at least 10 years along with whatever happens in the following 4 years. We do this both because it is an important question and because we feel it is the cleanest case for causal inference. Thus, we are not addressing how countries achieve stability after a string of coups. That is an important, but analytically distinct, issue from what we are studying.

Our finding that coups have such a harmful and persistent effect on national income means that effective coup-prevention is a significant development issue. One could even wonder how such damaging events take place. On average, coup-makers are imposing a negative externality on the nation at large. Their private value is positive, but the overall effect is quite negative. One classic way to deal with a negative externality is to tax the "product," and indeed, some regional organizations, large countries, and international organizations have taken steps to prevent coups by sanctioning the regimes that emerge from them. However, these sanctions are not always applied in practice and the number of coups has increased in the last few years. Given the large and persistent average economic cost of coups, it is certainly worth considering whether tougher and more consistently applied anticoup policies might be warranted or indeed to consider more creative ways of achieving regime change without incurring the large economic costs that coups impose.[5]

LITERATURE

There is a large literature on the causes of coups, including papers examining the relationship between regime type, transition, and coup occurrence. Most of this work demonstrates that institutional fragility or weakness is associated with higher coup risks (see Gassebner et al., 2016; Powell, 2012; 2014; Thyne & Powell, 2016; Tomashevskiy, 2017).[6] Much less work has been done on the effect of coups on income and the empirical evidence on the topic has been mixed. Londregan and Poole (1990) find that economic performance has a dramatic and significant effect on the probability of coups; the inverse, however, is not true: past coups have no statistical effect on a country's economy.

Fosu (2002) investigates the economic implication of coup events in sub-Saharan Africa and finds that coups have a negative effect on economic growth except for in countries with low levels of investment. Meyersson (2016), in a cross-country sample of 94 countries from 1950 to 2010, shows that coups are associated with a 1% decrease in per capita income growth, though this effect was smaller and less significant in autocracies. Jong-A-Pin and Yu (2010), in a cross-sectional analysis of 10-year growth rates after coups, argue that in extremely poor countries, there is so much low hanging fruit that successful coups raise incomes in those countries.

Theoretically, there are reasons to think that coups could positively affect income. A coup may allow for the removal of undesirable leaders from office and

[2] However, as we discuss further below, parallel pretrends do not guarantee the validity of our estimates. We need parallel trends to hold in the treatment period as well, which is not testable.

[3] Our method is consistent with what Ho et al. (2007) advocate, namely using matching as a preprocessor before estimating regressions.

[4] Given data restrictions, not all cases are included in all the models estimated in the paper.

[5] As we note in the discussion, there is always a possibility that tougher anticoup policies can have unintended consequences.

[6] There is also a broad literature that focuses on the implications of coups for political survival, democratic erosion, accountability, and institutional consolidation. Scholars have shown the importance of the military as a significant pivotal player in regime survival with significant repercussions on the distribution of public and private goods (Bueno de Mesquita & Smith, 2010; Powell et al., 2018; Svolik, 2012).

the promotion of growth-producing policies.[7] Coups may reduce the levels of political instability in periods of rapid social and economic change, easing tensions and violence (Huntington, 1968). Overall, though, we think that there are strong reasons to doubt that coups would positively affect subsequent income levels.

First, coups are likely to be harmful to domestic and foreign investment. Coups often create weak executives that bias spending toward the armed forces to stay in power (Mbaku, 1994). This could distort priorities for new investment. If we conceptualize coups as rent-seeking behavior, where some groups plan to alter the composition of the winning coalition and the scope of economic policies, then coups increase uncertainty surrounding the economic activity and the creation of special privileges for some social actors (Izadi, 2022; Mbaku, 1994).

Coups also create political uncertainty and there is a large empirical literature linking instability and income (see Alesina et al., 1996; Bloom et al., 2007; Carmignani, 2003; Chen & Feng, 1996; Jong-a-Pin, 2009; Lachapelle, 2020; Ouedraogo et al., 2021). Higher uncertainty dampens economic activity, hampering economic agents' decision-making ability about the profitability of future investments in physical and human capital[8] (Alesina & Perotti, 1996; Özler & Rodrik, 1992). If new investment involves large sunk costs, then investors have incentives to relocate to countries where their property and resources are secure.[9] Rodrik (1991) argues that uncertainty imposes a special tax on economic agents, who must respond to the signals generated by the reform, sparking doubts about future investments.[10]

Besides a potential negative effect on domestic investment and FDI, it is likely that foreign aid will decrease after a coup. Some Western aid donors, like the United States, are pledged to cut off aid flows to countries that experienced an irregular transfer of power. For these reasons, we expect coups to have negatively affected income via their effect on investment and foreign aid flows.[11]

Coups are also likely to impact income because of the effect they have on governance and civil society.[12] (See, for example, Bennett et al., 2021; Bjørnskov & Pfaff, 2021; Bjørnskov & Rode, 2020; Miller, 2011.) It is true that some coups take out very repressive and autocratic governments. When that is the case, we might expect a flourishing of freedom of expression and a restoration (or establishment, as the case may be) of the rule of law. Miller (2012, 2016) and Thyne and Powell (2016), for instance, find that coups often result in democracy, which would typically be associated with greater rule of law and freedom of expression.

There are reasons not to be too optimistic, however. The new regimes will want to avoid their predecessors' fate and will likely prioritize making the coalition that brought them to power happy. They will be less interested in freedom of expression, especially if that freedom would result in a strengthening opposition that could undermine their agenda (Bueno de Mesquita & Downs, 2006; Downes & Monten, 2013). New regimes that are military in nature also tend to be less interested in shoring up democracy (Dahl, 1971; Linz & Stepan, 1996).

If the new leaders feel less accountable to the public (because they were not elected), then they might also be less concerned with protecting the rule of law, freedom of expression, and overall property rights. There is an empirical literature showing that these factors are important to economic development (Asoni, 2008; Butkiewicz & Yanikkaya, 2006; Fabro & Aixalá, 2012; Goldsmith, 1995; Haggard et al., 2008; Khan & Hudson, 2014; LeBlang, 1996). Empirically, Absher et al. (2023) find that Central Intelligence Agency (CIA)-led regime changes in Latin America led to significant decreases in freedom of expression and rule of law. Bennett et al. (2021) find that successful coups increase corruption and reduced judicial constraints, which are different measures that are also examining the effect of coup on civil society and governance.

THEORY

Do coups improve or worsen national income? An initial, perhaps naïve, evaluation of the effect coups would have on economic performance hinges simply on whether the policies of the new regime are better than those of the old one. We illustrate this approach

[7] On the other hand, Easterly and Pennings (2020) show that it is rare for the leader of a country to have a statistically significant effect on economic growth in either democracies or authoritarian regimes.

[8] Gyimah-Brempong and Camacho (1998) hypothesize that increases in uncertainty will cause the highly educated to emigrate, given that they are less likely to be able to use their educational investments in their home country.

[9] Duggan (2004), on the other hand, finds that right-leaning, bloodless coups prove less harmful to investment than other types of coups, suggesting that an investment-friendly postcoup government might attract investors to the country.

[10] Coups could harm average real per capita income if they are violent and result in numerous fatalities. We think this is unlikely to be a significant factor because De Bruin (2019) shows in a sample of 377 coup attempts that less than half of them (45.4%) were violent. Interestingly, it was the failed coups that were more violent on average. Of the 124 successful coups, more than 64% of them did not result in any fatalities. Of the ones that did, 56% had between one and 25 fatalities.

[11] Meyersson (2016) finds that coups that overthrow democratic governments decrease investment.

[12] There is also reason to believe that coups negatively affect democracy, which could then indirectly affect national income. Acemoglu et al. (2019) show that countries that become democratic have a 20% higher average income than those that stay autocracies. The authors argue that democratic regimes tend to have policies that are more favorable to economic growth, can raise more money in taxes, and invest in public goods more than nondemocratic ones.

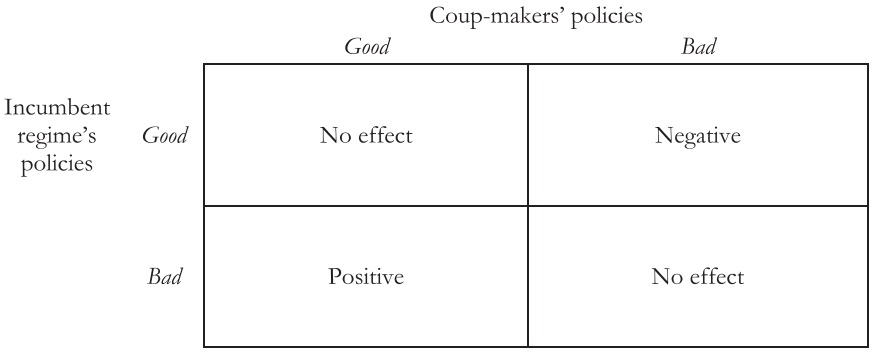

FIGURE 1 A first look at the effect of coups on economic performance. *Note*: This figure illustrates the potential effects of a coup on economic performance assuming that the relative quality of the new policies is all that matters.

in Figure 1, where we consider a world with two types of policy mixes, good and bad.[13]

If the coup-makers implement better (worse) policies than the incumbent regime, then economic performance should improve (decline). If the policy mix is unchanged, then the economic outcome should be unchanged as well. In this scenario, because we have one negative cell, one positive cell, and two zero effect cells, the average effect of coups would simply depend on the distribution of coups across the four scenarios.

However, we do not believe the above scenarios are accurate, mostly because of the increased uncertainty accompanying extra-legal regime change. We propose that coups increase uncertainty in two ways. First, the (possibly violent) removal of previous elites creates a commitment problem regarding the new rent distribution in the coup-born regime. Coups undermine previous elite arrangements, privileges, guarantees, and access to resources, making new governments struggle to assure economic actors about the profitability of their activities in the country. Yu and Jong-A-Pin (2016) argue that the competencies needed to execute a successful coup and hold on to power are not the same as the competencies necessary to allow the economy to flourish and grow.[14] In addition, poor policymaking, and bad decisions in general, may also be magnified under military rule because military leaders tend to dislike checks and balances from other institutions.[15]

Second, even if the new government assures investors that it will not engage in predatory behavior or curtail their profits, coup-makers might not have a track record in government, and economic actors lack information about the new policy mix. Hence, an informational problem about the new regime's policies is likely to occur. This issue compounds when we consider the international responses to the new regime. The international community has encouraged the implementation of the anticoup norm that requires states to condemn diplomatically and, if necessary, punish coup behavior economically. Therefore, not only are investors likely to fear expropriation from coup-makers but also financial punishment from the international community.

Overall, there will be less certainty/credibility accompanying the coup-makers' policy mix than there was over the incumbent regime's policy mix. This is partly because coup-makers may be unable to commit themselves to follow through with desirable economic policies or may have incentives to renege on them, partly because there is more uncertainty about the time horizons of the new regime, and partly because there is more uncertainty about what international responses will be. When we account for this increased uncertainty, the predicted economic effects of coups changes, as shown in Figure 2.

When bad policies replace good policies, we continue to predict economic decline. However, when the new policies are the same as the old ones, we now predict economic performance will decline because of the increased uncertainty. Finally, even when the new regime has a superior policy mix to the previous one, the effect on the economy can be ambiguous. Whereas the policy improvement should improve the economy, the uncertainty will hurt it, so the net effect is not clear, especially in the short run. Thus, after factoring in the likely effects of increased uncertainty, we find three negative scenarios and one ambiguous scenario. The distribution of coups across these scenarios will still matter, but it is much clearer here that the average effect of coups will be to reduce economic performance, at least in the short run.[16]

[13] There are obviously more than two possible categories of policies, and "good" may be quite optimistic for describing any of the policies observed in these cases. It would suffice to determine whether the new regime's policies were better, worse, or the same as the incumbent regime.

[14] In a sample of more than 1500 political leaders from 1946 and 2011, the authors find that (1) military leaders are much less likely to be highly educated and (2) there is a significant relationship between a leader's education level and the rate of economic growth.

[15] Although military rulers may dislike any other institutions having veto powers over their decision making, Bjørnskov (2020) shows that they are strongly in favor of this type of counterbalancing power when the country democratizes.

[16] In the longer term, it may well be that superior policies would win out as uncertainty subsides over time.

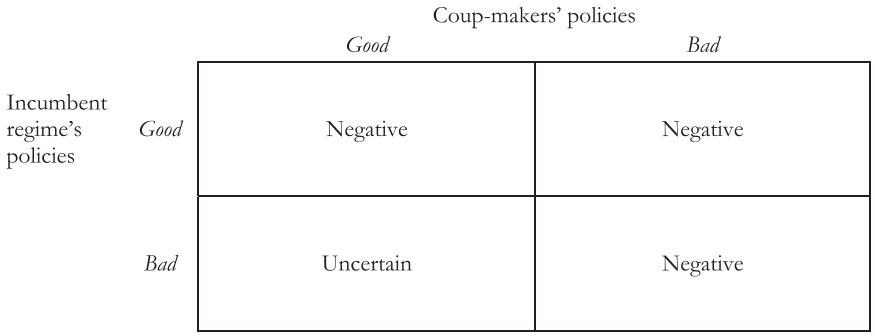

FIGURE 2 The effect of coups on economic performance once uncertainty is considered. *Note*: This figure illustrates the potential effects of a coup on economic performance allowing for uncertainty about the new regime.

EMPIRICAL STRATEGY

As noted in the introduction, there are several challenges to identifying the causal effect of a coup on incomes and governance. First, coups are staggered over time, so standard panel regressions may not be the appropriate way of estimating their effects. The major concerns are that in a standard panel regression, (a) early treated units are mechanically used as control for later treated units and (b) perhaps declines in income are causing the coups. In addition, untreated units on average may not be good counterfactuals for the treated units, again making causal inference difficult.

We address all these issues. First, we study the effect of coups where there is a clear pretreatment period with no coups. The pretreatment period allows us to test for pretrends. Specifically, we can investigate whether national income is falling in the treated countries leading up to their coup as compared with the untreated counterfactual countries. It also allows for any effects of previous coups to dissipate, so that we are not confounding their effects with the coups we study. We thus require that to be in our treated sample for analysis, a coup must have been preceded by 10 years without a coup, though we do allow additional coups in the 4 years following the qualifying coup. We then have a 10-year pretreatment period and a 5-year treatment period for a total "window" of 15 years.

This process leaves us with 65 qualifying cases to analyze. Table 1 lists these cases. Although this is clearly a subsample of all coups that occurred, it is nonetheless representative of the overall geographic distribution of coups and the type of coup (civilian vs. military), as we demonstrate below.

Table 2 compares our sample's distributions to the global distribution. As can be seen, our sample is very similar to the overall distribution of successful coups in geography, coup type, and even temporally (Cold War and post–Cold War). We are slightly underweighted in Latin America, where several countries had serial coups.

TABLE 1 Qualifying coups, by country and year.

Afghanistan	1978	Liberia	1980
Algeria	1965	Libya	1969
Algeria	1992	Madagascar	2009
Bangladesh	1975	Mali	2012
Benin	1972	Mauritania	1980
Brazil	1964	Niger	1974
Burkina Faso	1966	Niger	1996
Burma/Myanmar	1962	Nigeria	1966
Burundi	1966	Pakistan	1999
Burundi	1996	Panama	1968
Cambodia	1970	Paraguay	1989
Central African Republic	1966	Peru	1963
Chad	1975	Portugal	1974
Chile	1973	Vietnam	1965
Comoros	1978	Rwanda	1973
Dem. Rep. of the Congo	1965	Rwanda	1994
Ecuador	1966	Seychelles	1977
Egypt	2013	Sierra Leone	1967
El Salvador	1961	Solomon Islands	2000
Equatorial Guinea	1979	Somalia	1969
Fiji	1987	South Korea	1961
Georgia	1992	Suriname	1980
Ghana	1966	Thailand	1977
Greece	1967	Thailand	2014
Guatemala	1982	The Gambia	1994
Guinea	1984	Togo	1963
Haiti	1986	Togo	2005
Honduras	1975	Tunisia	1987
Honduras	2009	Turkey	1960
Indonesia	1966	Uganda	1971
Ivory Coast	1999	Uruguay	1973
Laos	1964	Yemen	1967
Lesotho	1986		

Note: Qualifying coups are the 65 successful coups with at least a 10-year, no-coup, pretreatment period. Due to data limitations, we cannot use all these coups in all of our statistical models.

TABLE 2 Sample representativeness.

	Our sample	All successful coups
Geography		
Middle East and Northern Africa	10.77	13.71
Sub-Saharan Africa	44.62	45.97
Asia and Pacific	20.00	14.52
Latin America and Caribbean	20.00	23.39
Eastern Europe	1.54	0.81
Western Europe	3.08	1.61
Coup type		
Civilian	12.31	11.29
Military	87.69	88.71
Time Period		
Post–Cold War	23.08	25.81

Note: Numbers are in percentages.

Second, following Cengiz et al. (2019), we take these staggered cases with their possibly unique 15 calendar year windows, stack them in event time (along with all the untreated units in each of the calendar windows), and estimate a simple, canonical, event study model on the stacked data. This procedure completely avoids any issues of "forbidden comparisons," where early treated units are inappropriately used as controls for later treated units. Baker et al. (2022) show that this estimator performs well in cases of staggered, dynamic, heterogeneous treatment effects (see especially their fig. 7).

Third, we deal with the possibility of poor counterfactuals in the untreated by preweighting the data using weights derived from entropy balancing (hereafter EB) developed in Hainmueller (2012). EB allows us to exactly balance a set of covariate means and variances between the treated and the controls. EB directly achieves covariate balance subject to the constraint that the weights used be as close as possible (in the entropy sense) to the equal weights implied by the unweighted data.[17] To avoid introducing posttreatment data and bias into the model via the weighting scheme, we estimate the EB weights in the stacked data at the last period before treatment and then apply those weights to every observation in our 15-year window. Appendix A contains a further discussion of the merits of EB.

Zhao and Percival (2016) show that using EB as a preprocessor also creates a doubly robust overall estimator of the average treatment effect. Cefalu et al. (2020) argue that using EB as a preprocessor can reduce bias in difference-in-differences estimators when parallel trends are not satisfied in the unweighted data.

[17] In contrast, propensity score methods do not guarantee covariate balance, and the researcher may have to repeatedly change the functional form of the propensity score estimating equation until balance is achieved.

DATA

We begin by testing for the effect of coups on real per capita GDP. To define our cases, we use three popular databases of coup occurrences and code a successful coup when at least two of the sources agree that one has taken place. (We use Bjørnskov & Rode, 2020; the variable called "regime end type" in Varieties of Democracy [VDEM] by Coppedge et al., 2021; Powell & Thyne, 2011.)

Our data for our outcome variable, real per capita GDP, are from the Penn World Tables (hereafter PWT) version 10 (Feenstra et al., 2015). We also evaluate the mechanisms behind the relationship between coups and income. More specifically, we test whether coups significantly affect domestic investment, FDI, and foreign aid. We measure domestic investment with data from the PWT, whereas the latter two (measured as percentages of GDP) come from the World Bank (2023).

We also investigate the possibility that the mechanisms behind our main result stem from the effect that coups have on governance and civil society. We do so with three variables: the rule of law, which refers to the extent that laws are transparent, independent, predictable, impartial, and equally enforced and the extent to which government officials comply with existing laws; freedom of expression, which measures the extent that a government respect press and media freedom, and the ability of citizens to discuss political matters in the public space; and an index of property rights protection, which measures the extent to which citizens have rights to acquire, possess, inherit, and sell private property. These variables come from the VDEM data set created by Coppedge et al. (2021). We discuss these governance variables in more detail in Appendix B. It is important to note though that there is little to no overlap between the property rights and rule of law indices, and they can potentially not move in lock step. For example, left-wing coups that upset the existing order are likely going to reduce measures of property rights protection, whereas military coups that protect elite interests may actually protect or even improve the measure. We would expect both types to be detrimental to rule of law.

As we discussed above, we use EB to ensure exact covariate balance between the treated and untreated groups. To do so, we include several covariates reflecting the political–economic environment. Specifically, we include an index of electoral democracy from VDEM. We also include export share and government spending as a percentage of GDP. We lag these

TABLE 3 Summary statistics.

	Mean	Standard deviation	Minimum	Maximum
Real per capita GDP	15,090.39	14,285.40	519.47	112,941.45
Domestic investment	0.24	0.12	−0.10	3.17
Government consumption/GDP	0.19	0.10	0.02	2.11
Exports/GDP	0.26	0.26	0.00	3.52
Foreign direct investment/GDP	3.77	15.11	−57.53	449.08
Foreign aid/GDP	0.05	0.09	0.00	0.94
Electoral democracy index	0.54	0.30	0.01	0.93
Political violence	−0.93	1.23	−3.44	3.19
Freedom of expression index	0.65	0.34	0.01	0.99
Rule of law index	0.70	0.29	0.00	1.00
Property rights index	0.70	0.27	0.00	0.97

Note: Real per capita GDP, domestic investment, government consumption, and exports are measured with the following Penn World Tables (PWT) variables: rgdpe/pop, csh_i, csh_g, csh_x, respectively (Feenstra et al., 2015). Data on foreign direct investment (FDI) inflows and foreign aid come from the World Bank (2023). The variables are called BX.KLT.DINV.WD.GD.ZS and DT.ODA.ALLD.KD, respectively. We measure electoral democracy, rule of law, freedom of expression, and property that come with the following variables from VDEM: v2x_polyarchy, v2x_rule, v2x_freexp, and v2xcl_prpty, respectively (Coppedge et al., 2021).

variables by 1 year before the treatment. These two variables also come from the PWT.[18] We also include a measure of political violence from VDEM. Finally, we include lags of the outcome variables in each analysis as covariates. Table 3 presents the summary statistics, sources, and database names of the variables used in our analysis. Appendix B also discusses the nature and definitions of the electoral democracy and political violence variables.

RESULTS

The causal effect of coups on income

Figure 3 presents the effects of successful coups on growth and development using the weights obtained from the EB and our stacked difference-in-differences event study estimator. We test for pretrends by including treatment dummies for 9 of the 10 years before the coup event (the 10th is normalized to zero), and none of them are significant. In the year of the coup and in each of the four following years, our estimated average treatment effect on the treated is negative and significant at the .01 level.

Two things are worth pointing out here. First, we do not see any anticipation effects, where income is falling relative to the controls before the coup occurs. Second, the estimated effects are persistent. The dip in income is not short-lived, the coefficients do not trace out a V-shaped pattern of loss and recovery. Row 1 of Table 4 presents the coefficients corresponding to the graphed treatment effects in Figure 3 and shows that the average decline in income is about 10%–12% relative to average precoup income.[19]

Our technique does not make forbidden comparisons (i.e., does not use early treated units as controls for later treated ones) and it minimizes possible extrapolation and reliance on a specific functional form from the weighting step. This approach is "doubly robust" in the sense that only one of the two models (the weighting model or the outcome model) needs to be correctly specified to produce an unbiased estimate of the average treatment effect on the treated. Given all that, we are confident we are identifying a real causal effect of coups on income. In what follows below, we investigate the robustness of these reported results.

Robustness tests

In this section, we address some potential objections or caveats about our results. First, there is a question of whether countries that do not experience coups in the subsequent 4 years after an initial coup are the same as ones that do. Perhaps the large effect we find of coups on income is driven by the instability in situations where there are multiple coups in a short amount of time. To test this, we restrict the sample to cases where there is only one coup (no further coups in the following 4 years). The second row of Table 4 presents these results. They are remarkably similar to our main

[18] In other models not reported in the paper, we add an index of human capital to the EB equation with very similar results. However, using this index reduces the number of cases we can analyze.

[19] This percentage figure is derived using the average income level of our treated units the year before treatment, which is around $3500.

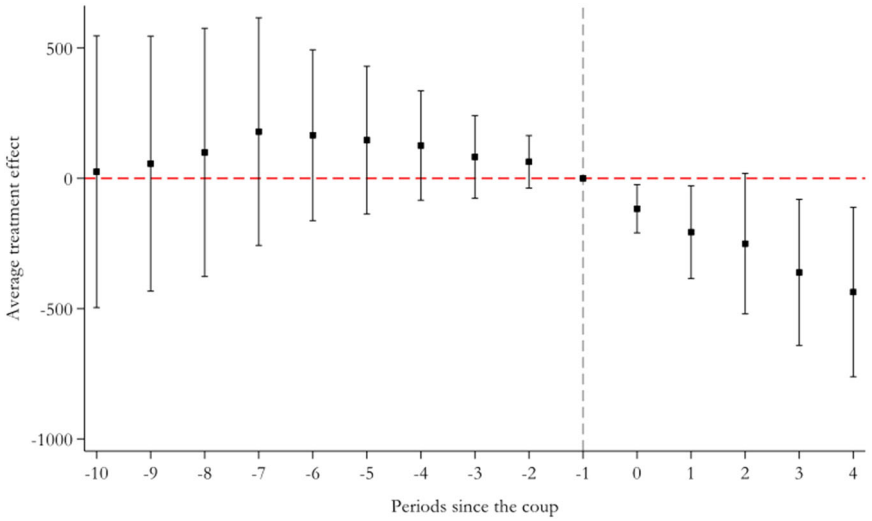

FIGURE 3 The effect of coups on real GDP per capita. *Note*: This figure displays the estimated treatment effects and their 95% confidence intervals, both for the 10-year pretreatment period and the 5-year treatment period. They are estimated in a stacked event study model where the coups are lined up in event time. The controls are the never treated countries and are weighted using entropy balancing. The underlying model also includes country, year, and cohort dummies.

TABLE 4 The effect of coups on real per capita income.

	T	T+1	T+2	T+3	T+4
Main results	−116.7*	−206.7*	−250.7†	−361.4*	−436.4**
	(46.8)	(89.8)	(136.3)	(141.7)	(164.2)
No further coups	−147.6*	−242.9*	−313.7†	−436.8*	−501.4*
	(61.9)	(120.1)	(181.7)	(175.5)	(199.9)
Only military coups	−139.6**	−240.2**	−294.4*	−336.2*	−370.7*
	(36.9)	(73.5)	(112.8)	(137.5)	(157.8)
Excluding civil wars	−105.6*	−206.2*	−212.4	−324.9*	−417.8*
	(49.2)	(91.6)	(133.4)	(135.0)	(161.3)
Post–Cold War	−358.3**	−652.5**	−812.2**	−1022.6**	−1212.7**
	(71.5)	(135.4)	(194.5)	(259.2)	(330.6)

Note: In each case, the dependent variable is real per capita GDP adjusted for deviations from purchasing power parity from the Penn World Tables (rgdpe/pop). Clustered standard errors are in parentheses. All the models include country, year, and cohort dummies along with the estimated treatment effects and the placebo pretreatment effects. Control observations are weighted using entropy balancing. $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$.

results, which are shown in the first row of the table for comparison, indicating that even a single coup has a disastrous effect on income.

Second, it is possible that military coups are fundamentally different in some ways from civilian-led coups. To investigate this further, we drop the nonmilitary coups from our full sample and reestimate our model. The third row of Table 4 reports these results. All five treatment coefficients are significant, and the magnitudes are just a bit smaller than our main results, indicating that the results hold up even when excluding nonmilitary coups.

Third, we attempt to deal with the possibility of there being other factors going on at the same time as the coups we are studying. Although we do not find evidence of parallel trends violations pretreatment, that is not the same as having parallel trends in the treatment period. We consult the VDEM civil war variable and exclude the seven treated cases that VDEM codes as having civil war during our treatment period.[20] If a coup happens concurrently with a civil war, perhaps our results are driven by the effects of the latter rather than the former. The fourth row of Table 4 displays these results, which are largely the same as our main results. Finally, we examine whether coups were mainly costly during the Cold War period and not afterwards by estimating our model only on cases that were treated after 1990. These results are in the final row of Table 4 and show strongly negative and significant results.

Next, we report results from a jackknife experiment where we drop one treated case at a time and reestimate the model repeatedly on this smaller sample. Table 5 summarizes the results.

[20] The name of the variable is *e_civil_war* and it is originally from Haber and Menaldo (2011). The coups that overlap with civil war include the Democratic Republic of the Congo (1965), Chile (1973), Guatemala (1982), Algeria (1992), Rwanda (1994), Vietnam (1965), and Burundi (1996).

TABLE 5 Summary of jackknife experiment.

	T	T+1	T+2	T+3	T+4
Mean	−116.7	−206.7	−250.5	−360.8	−435.2
Standard deviation	6.2	11.2	17.3	16.0	16.4
Minimum	−148.4	−264.5	−338.7	−405.9	−478.8
Maximum	−105.5	−186.8	−209.7	−321.4	−384.2

Note: Here, we drop one coup at a time from the sample and reestimate our main equation at each iteration. The table reports the summary statistics from this exercise for each of our five treatment periods.

TABLE 6 The effect of coups on real per capita income excluding one region at a time.

Excluded region	T	T+1	T+2	T+3	T+4
Eastern Europe and Central Asia	−104.2*	−181.8†	−208.8	−300.1*	−356.1*
	(47.9)	(92.7)	(138.6)	(144.5)	(166.5)
Latin America and Caribbean	−121.8*	−228.6*	−248.0	−380.2*	−469.5*
	(56.5)	(113.1)	(163.0)	(163.8)	(184.8)
Middle East and North Africa	−106.5*	−142.4†	−179.1	−283.0*	−359.6*
	(48.5)	(84.3)	(134.7)	(133.6)	(156.6)
Sub-Saharan Africa	−161.0**	−318.9**	−372.2*	−440.8*	−547.9*
	(49.1)	(104.8)	(160.3)	(194.4)	(233.1)
Western Europe and North America	−106.5*	−179.0*	−225.6†	−335.6*	−404.5*
	(47.0)	(89.7)	(135.4)	(137.0)	(154.7)
Asia and Pacific	−98.4†	−207.7*	−270.9†	−399.7*	−450.4*
	(54.0)	(102.5)	(155.7)	(160.1)	(186.3)

Note: In each case, the dependent variable is real per-capita GDP adjusted for deviations from purchasing power parity from the Penn World Tables (rgdpe/pop). Clustered standard errors are in parentheses. All the component models include country, year, and cohort dummies along with the estimated treatment effects and the placebo pretreatment effects. Control observations are weighted using entropy balancing. † $p < .1$; * $p < .05$; ** $p < .01$.

The first row of the table shows that the average of the treatment effects estimated by dropping one coup at a time is very similar to the results we found when we estimate the model once over the full sample. The second row gives the standard deviation of the estimated coefficients across the individual submodels. These relatively small numbers show that dropping any one country does not greatly alter the estimated results. Rows 3 and 4, which given the smallest and largest effects estimated in the submodels, reinforce this result. They are all negative and there are no extreme values.[21]

Second, instead of dropping individual coups, we drop all coups from one region of the world at a time, reporting the results of estimating our model on the remaining regions. Table 6 reports these results and shows that our overall average result is not being driven by a single region of the world.

Taken together, the above robustness checks make us confident that our results are not because of a peculiarity or an extreme case or region in our data, or that they depend on using the exact sample we originally chose for analysis.

[21] Dropping Egypt in 2013 produces the smallest effect in the final period, while dropping Greece in 1967 produces the largest effect in the final period.

MECHANISMS

We have found a significant, persistent, and sizable negative effect of coups on national income. But what are the mechanisms whereby the coup causes income to fall? As described above, we investigate six potential mechanisms: three economic and three political. We do this by estimating the causal effect of coups on these mechanisms. As we show below, we find that declines in investment, rule of law, and repression are the likely mechanisms driving income to fall when coups occur.

Economic factors

We investigate how coups affect domestic investment, FDI, and foreign aid. We use the same set up as we used for our main results on income. Figure 4 shows

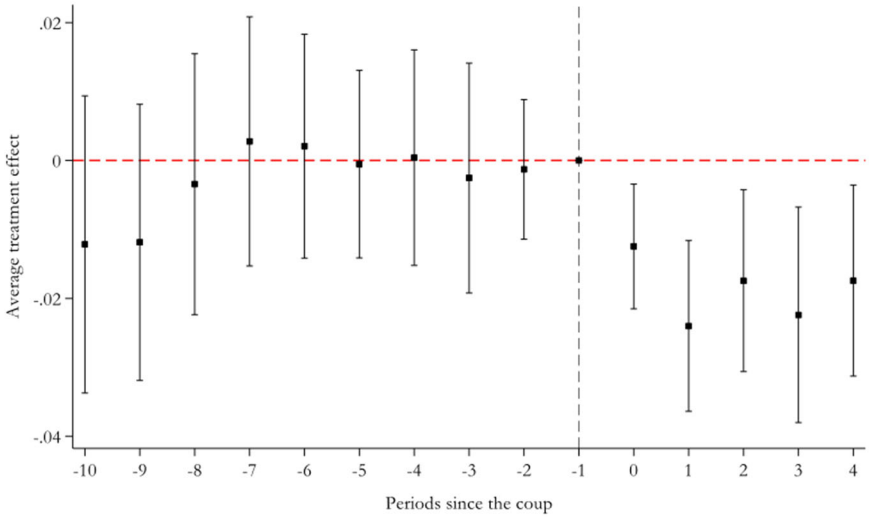

FIGURE 4 The effect of coups on domestic investment. *Note*: This figure displays the estimated treatment effects and their 95% confidence intervals, both for the 10-year pretreatment period and the 5-year treatment period. They are estimated in a stacked event study model where the coups are lined up in event time. The controls are the never treated countries and are weighted using entropy balancing. The underlying model also includes country, year, and cohort dummies.

TABLE 7 Testing the mechanisms of how coups might affect national incomes

	T	T+1	T+2	T+3	T+4
Economic mechanisms					
Domestic investment	−.012**	−.024**	−.017**	−.022**	−.017*
	(.005)	(.006)	(.007)	(.008)	(.007)
Foreign direct investment/GDP	.148	−.288	−.676	.009	−.675
	(.571)	(.567)	(.608)	(1.043)	(.952)
Foreign aid/GDP	.008	.001	−.005	−.008	−.007
	(.015)	(.010)	(.008)	(.006)	(.008)
Governance mechanisms					
Rule of law	−.053**	−.086**	−.087**	−.084**	−.087**
	(.013)	(.020)	(.021)	(.022)	(.023)
Freedom of expression	−.084**	−.094**	−.098**	−.094*	−.092*
	(.023)	(.034)	(.035)	(.037)	(.038)
Property rights	.007	.026†	.02	.025	.025
	(.008)	(.014)	(.014)	(.015)	(.017)

Note: Clustered standard errors are in parentheses. All the models include country, year, and cohort dummies along with the estimated treatment effects and the placebo pretreatment effects. Control observations are weighted using entropy balancing. †$p < .1$; *$p < .05$; **$p < .01$.

the causal effect of coups on domestic investment over our 15-year window and row 1 of Table 7 reports the exact values of the estimated treatment effects.

We find a negative, significant, and persistent effect of coups on investment in the first two treatment periods. The effect size ranges from −10% to −15% declines (the precoup average investment level is just below 20%).[22] This is consistent with our main theoretical argument that coups create uncertainty, which deters/delays investment and reduces national income.

We next consider FDI. Figure 5 displays our event study results and row 2 of Table 7 gives the exact treatment effect coefficients. We find that FDI falls, but the effect is not significant.[23]

Finally, we consider the effect of coups on foreign aid. Figure 6 displays the event study and row 3 of Table 7 reports the treatment effect coefficients.

Surprisingly, coups cause foreign aid to increase, although the effect is not significant. To the extent that this postcoup nondecline in aid is real, the

[22] This number was calculated by hand by the authors from PWT data.

[23] It is worth noting that the data on FDI are much more limited for our treated countries so that our sample of coups under study is smaller than for the rest of our results presented up to this point.

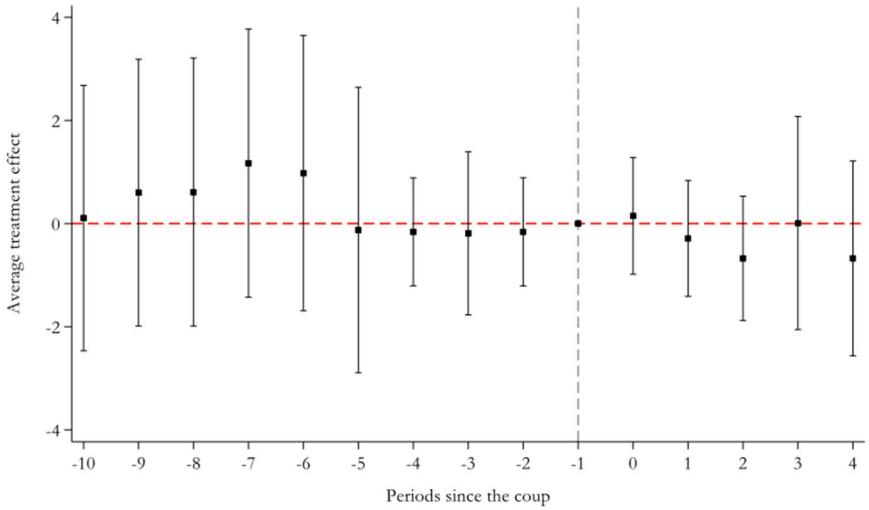

FIGURE 5 The effect of coups on foreign direct investment/GDP. *Note*: This figure displays the estimated treatment effects and their 95% confidence intervals, both for the 10-year pretreatment period and the 5-year treatment period. They are estimated in a stacked event study model where the coups are lined up in event time. The controls are the never treated countries and are weighted using entropy balancing. The underlying model also includes country, year, and cohort dummies.

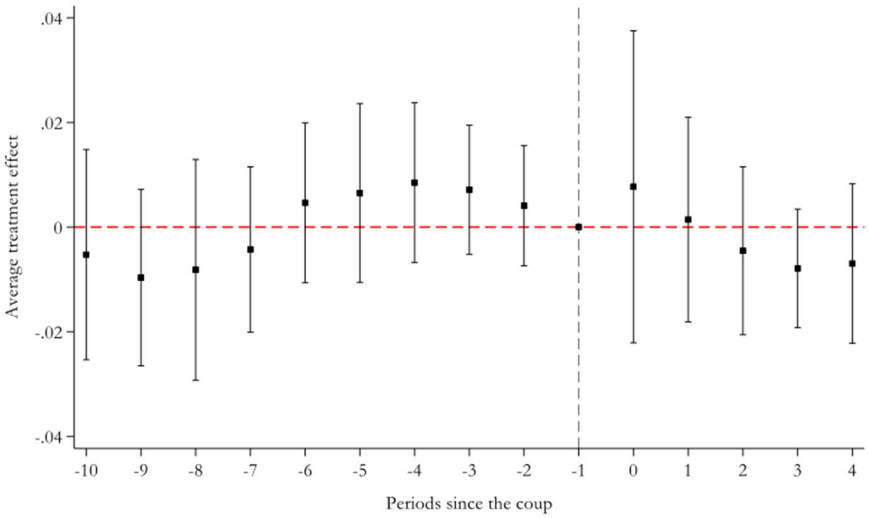

FIGURE 6 The effect of coups on foreign aid/GDP. *Note*: This figure displays the estimated treatment effects and their 95% confidence intervals, both for the 10-year pretreatment period and the 5-year treatment period. They are estimated in a stacked event study model where the coups are lined up in event time. The controls are the never treated countries and are weighted using entropy balancing. The underlying model also includes country, year, and cohort dummies.

international community should perhaps consider rethinking their aid distribution efforts.

Political factors

Now, we turn to the three political factors we study: rule of law, freedom of speech (a measure of repression), and property rights. Figures 7, 8, and 9 report the event study graphs, respectively.

We find that coups cause a significant reduction in rule of law and freedom of speech but not in property rights. Rows 4, 5, and 6 of Table 7 report the exact treatment effect coefficients. Four years after a coup, rule of law has declined on average by a bit more than 20% relative to the average in the year before the coup. Freedom of expression also declines by slightly more than 20%, whereas the (insignificant) increases in property rights are around 4%.

Finally, it is interesting to consider some direct policy undertaken by the coup-makers. Although a full analysis of the histories of all our cases is beyond the scope of this paper, we did investigate whether constitutional changes could be an important part of the story. We do this in a simple before and after comparison for our treated cases. Appendix C reports the results. The rate of constitutional change greatly accelerates following the coups. For instance, there are 47 cases of amendments (up 27% from the prior 5 years), 34 new constitutions (up 42%), 11 interim constitutions (up 57%), five cases of a previous constitution being reinstated (compared with zero in the prior 5 years), and 23 cases of suspending the constitution (up from two).[24] Overall, there is a 70% increase in constitutional changes for our treated cases compared with the changes in the prior 5 years. This constitu-

[24] These amendments sometimes occurred to a constitution that was suspended at the time of the amendment!

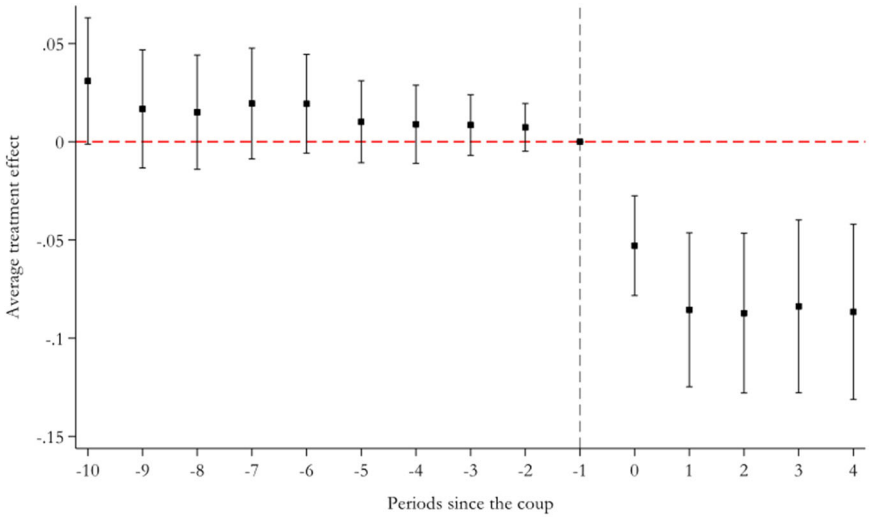

FIGURE 7 The effect of coups on rule of law. *Note*: This figure displays the estimated treatment effects and their 95% confidence intervals, both for the 10-year pretreatment period and the 5-year treatment period. They are estimated in a stacked event study model where the coups are lined up in event time. The controls are the never treated countries and are weighted using entropy balancing. The underlying model also includes country, year, and cohort dummies.

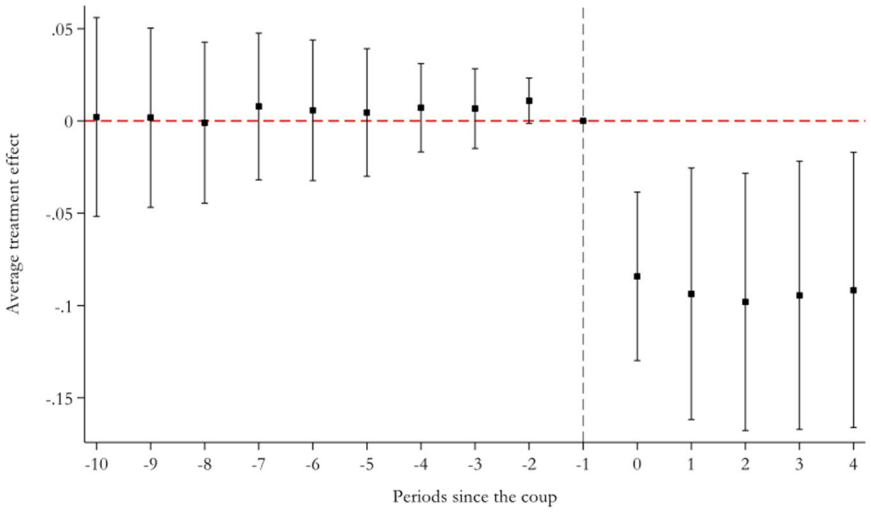

FIGURE 8 The effect of coups on freedom of expression. *Note*: This figure displays the estimated treatment effects and their 95% confidence intervals, both for the 10-year pretreatment period and the 5-year treatment period. They are estimated in a stacked event study model where the coups are lined up in event time. The controls are the never treated countries and are weighted using entropy balancing. The underlying model also includes country, year, and cohort dummies.

tional variation may well be an additional source of increased uncertainty.[25]

DISCUSSION

We have uncovered a quite large, negative, and persistent effect of coups on national income. Meyersson (2016) found a 1% yearly growth penalty from coups in democracies, but that would (with only a little violence to arithmetic) be about a 5% decline in GDP after 5 years, where we find effects over twice as large. To the extent that this is new information, it has real implications for how countries and international organizations deal with coups and coup-makers. In a very real sense, coups are a development issue. Preventing them would be a more positive policy outcome than most of what development agencies are currently promoting. There is a lot of research about how incumbent rulers can take actions to "coup-proof" their regimes, but here we are thinking about how external actors might discourage costly coups.[26]

The problem with this large cost of coups is that the coup-makers do largely not bear it. We have the classic political problem of concentrated benefits and diffuse costs. It is worth considering further adapting the classic externality solution; making agents face the full costs of their activities to the case of coups. Countries could contract with regional organizations to root/drive out coup-makers militarily, along the lines of what Economic Community of West African States has done. Or, countries could give permission to the United States and the European Union to immediately and irrevocably freeze all assets of coup-makers and disallow their international travel. Countries could consider cutting military aid to regimes that come to

[25] Bjørnskov and Voigt (2023) document a similar phenomenon in a long time series of Latin American countries.

[26] See, for example, Powell (2012).

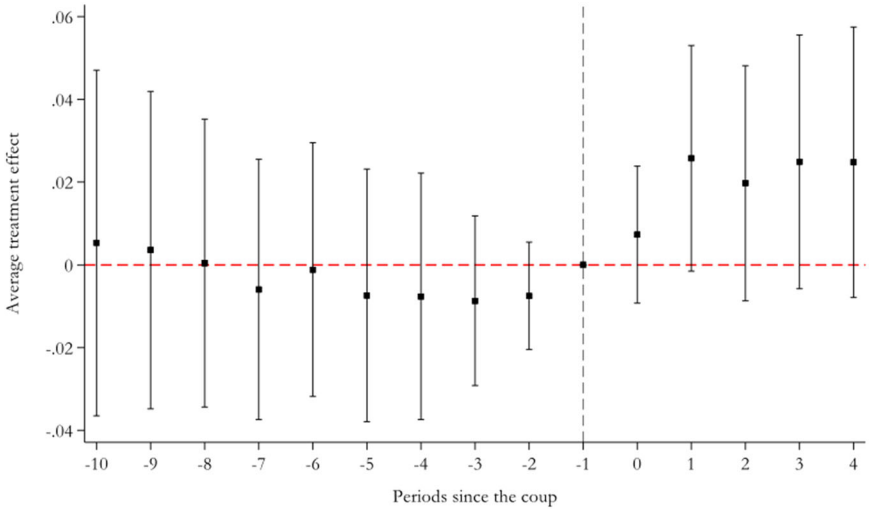

FIGURE 9 The effect of coups on property rights. *Note*: This figure displays the estimated treatment effects and their 95% confidence intervals, both for the 10-year pretreatment period and the 5-year treatment period. They are estimated in a stacked event study model where the coups are lined up in event time. The controls are the never treated countries and are weighted using entropy balancing. The underlying model also includes country, year, and cohort dummies.

power in a coup.[27] Cutting military aid would help force coup-makers to face more of the costs of their actions as military spending often rises dramatically in these new, coup-born regimes (Bove & Nisticò, 2014). Additionally, countries could pledge to cut off all aid to coup-making regimes or ban trade with them.

On the other hand, it is possible that the threat of a coup is actually constraining the incumbent autocrat from implementing even worse policies (from the point of view of the average citizen), similar to the way the threat of a hostile takeover may constrain incumbent managers from ignoring shareholder interests. To the extent coup-threats are effectively disciplining incumbent rulers, fully insulating them from coups could become counterproductive. An alternative approach could be encouraging/allowing the incumbent autocrat to step down with asylum guaranteed and some part of their fortune intact. However repugnant, this approach might help to mitigate the economic damage from a coup that removes the leader.

The above discussion is clearly preliminary but given our findings on the large economic costs of coups, further work on the costs and benefits of possible actions by the international community to reduce their frequency seems well-warranted.

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[27] Egypt, where the United States continued to provide substantial military aid after the 2013 coup, is a case in point.

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APPENDIX A: WEIGHTING THE SAMPLE

Here, we provide more details on entropy balancing (EB) and compare it with inverse propensity score weighting (hereafter IPW) with our data. Figure A1 shows that before balancing, our covariate averages were very different between the treated and the controls, as shown by the open circles. The treated had significantly higher political violence, lower real per capita GDP, less democracy, and lower export shares. Only government spending was balanced between the two groups. The *X*-axis measures the difference between the treated and controls for each variable in terms of standard deviations of the distribution of the treated. Applying EB to the controls completely eliminates these imbalances, as shown by the closed circle dots. The weighted controls have exactly the same means as the treated. Further, we balance on both the means and variances, so the variances are also essentially identical between the treated and the weighted controls (not pictured here).

By contrast, IPW uses the propensity score for weighting and does not directly aim for covariate balance (though it can produce balance). Figure A2 shows that IPW reduces, but does not nearly eliminate, imbalances between the treated and controls. The closed circle dots (differences after IPW weighting) are still quite large, on the order of one-half a standard deviation. This is one reason why we prefer EB to IPW, especially in an estimation format where the researcher is rarely going to check covariate balance and go back to alter the functional form of the IPW equation if balance is not initially achieved.

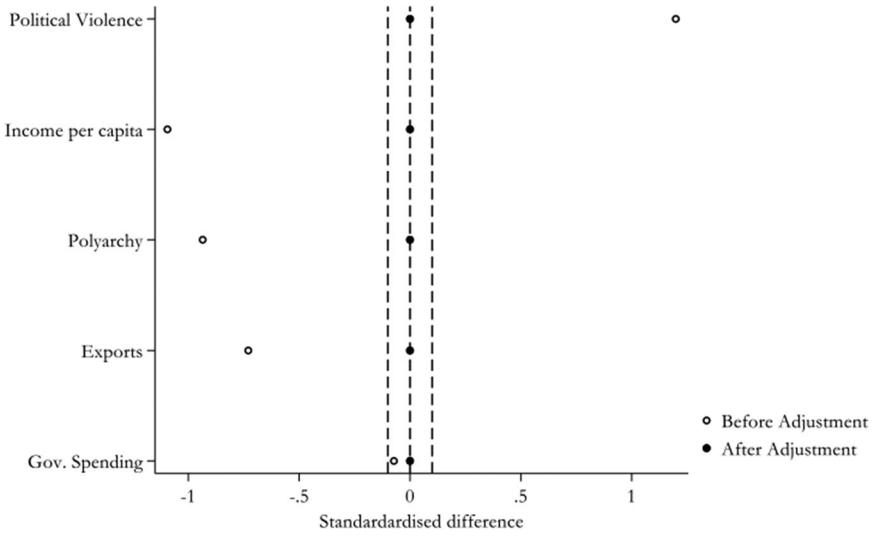

FIGURE A1 Covariate balance using entropy balance.

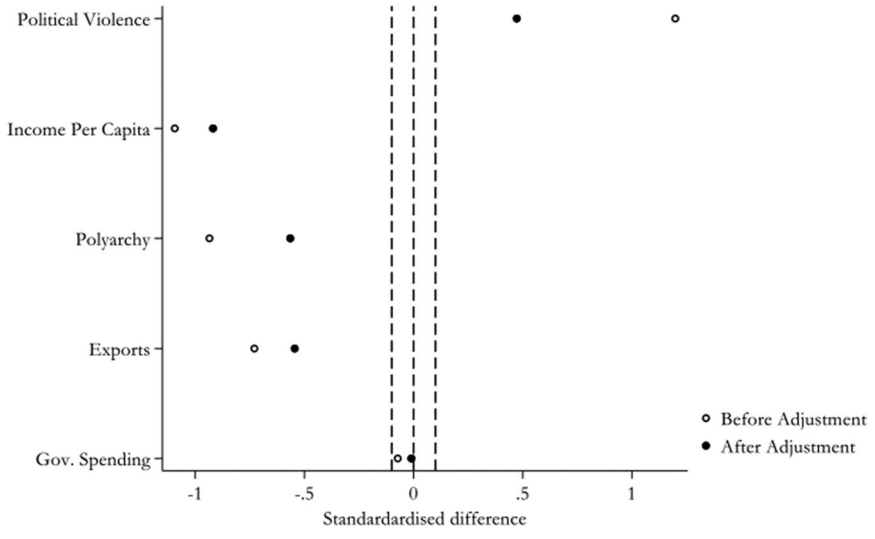

FIGURE A2 Covariate balance using inverse propensity score weighting (IPW).

APPENDIX B: DESCRIPTION OF GOVERNANCE VARIABLES

Rule of law

Our measure of the rule of law comes from the VDEM database (Coppedge et al., 2021). We chose this variable because of its comprehensive coverage and its effectiveness in capturing the key dimensions of the concept. It is worth noting that other measures of the rule of law often overlap substantially with VDEM's measure. They also sometimes include subcomponents measuring crime and violence, making it less relevant to what we are studying. However, this variable does *not* include a measure of property rights, which allows us to study both concepts separately. See Kołczyńska and Bürkner (2021) for a good examination of the most popular measures of the rule of law.

The variable is called *v2x_rule* and asks, "to what extent are laws transparently, independently, predictably, impartially, and equally enforced, and to what extent do the actions of government officials comply with the law?" The variable is scaled and ranges from 0 to 1 (with 1 representing the highest level of rule of law).

More specifically, "the index is formed by taking the point estimates from a Bayesian factor analysis model of the indicators for compliance with high court *(v2juhccomp)*, compliance with judiciary *(v2jucomp)*, high court independence *(v2juhcind)*, lower court independence *(v2juncind)*, executive respects constitution *(v2exrescon)*, rigorous and impartial public administration *(v2clrspct)*, transparent laws with predictable enforcement *(v2cltrnslw)*, access to justice for men *(v2clacjstm)*, access to justice for women *(v2clacjstw)*, judicial accountability *(v2juaccnt)*, judicial corruption decision *(v2jucorrdc)*, public sector corrupt exchanges *(v2excrptps)*, public sector theft *(v2exthftps)*, executive bribery and corrupt exchanges *(v2exbribe)*, executive embezzlement and theft *(v2exembez)*" (Coppedge et al., 2021; Pemstein et al., 2022, V-Dem Working Paper Series 2022:21).

Freedom of expression

We use VDEM's measure of the freedom of expression. It is called *v2x_freexp* and it asks, "to what extent does government respect press and media freedom, the freedom of ordinary people to discuss political matters at home and in the public sphere, as well as the freedom of academic and cultural expression?" The variable is an index that ranges from low to high (0 to 1). It is constructed by "taking the point estimates from a Bayesian factor analysis model of the indicators for print/broadcast censorship effort (*v2mecenefm*), harassment of journalists (*v2meharjrn*), media self-censorship (*v2meslfcen*), freedom of discussion for men/women (*v2cldiscm*, *v2cldiscw*) and freedom of academic and cultural expression (*v2clacfree*)."

There has been concern expressed about the coding of a similar VDEM variable measuring press freedom. Ruohonen (2021) finds that there is little variation in the several different measures of press freedom over long periods of time. Fortunately, we are using a more overall freedom of expression rather than just freedom of the press. We calculated the variation in our freedom of expression and rule of law variables for our treated cases and find that both vary substantially in the years before their coups (Coppedge et al., 2021; Pemstein et al., 2022).

Property rights

We use a measure of property rights from VDEM. The variable is called *v2xcl_prpty* and it asks whether citizens have the right to private property. The creators clarify that "private property includes the right to acquire, possess, inherit, and sell private property, including land. Limits on property rights may come from the state which may legally limit rights or fail to enforce them; customary laws and practices; or religious or social norms. This question concerns the right to private property, not actual ownership of property." As mentioned above, this variable does not overlap with our measure of the rule of law, allowing us to study both concepts separately.

The variable is an index that ranges from 0 to 1, with higher numbers representing higher protection of property rights. It is created by averaging property rights for men *(v2clprptym)* and women *(v2clprptyw)* (Coppedge et al., 2021).

As we noted in the main text, there are reasons to think that military coups may be more protective of property rights than left-wing coups. Military coups are frequently driven by tensions between elite interests, state policies, and property rights. This can be attributed to the military's historical role in serving elite interests. (See Needler, 1978; Nun, 1976; O'Donnell, 1973). David (1987), for instance, introduced the idea of a "veto coup," where threats to property rights can precipitate a coup, with the aftermath emphasizing the restoration of these rights. Perlmutter and Bennet (1980) argue that the high-ranking generals behind most coups in the region have often been from wealthy families, studied in the West, and were interested in protecting property rights to keep international aid flowing. This phenomenon is not limited to Latin America either. Military coups in sub-Saharan Africa and Asia often follow the same pattern, indicating that the effect of nonconstitutional turnover on property rights is not completely obvious a priori.[28]

[28] Tusalem (2000) finds that developing countries with secure property rights are much less likely to experience coups. For more on military coups and

Even so, we believe there are still reasons to be pessimistic about the effect of coups on property right protection. If the new regime is determined to undo the effects of previous redistribution, this will of course lead to lowered property right protection for those who recently received rights. In addition, coup leaders often implement constitutional change soon after a coup, which could cause considerable uncertainty about what property rights will look like afterwards. In Table C1 of Appendix C, we show that constitutional variation increases substantially in the period after a coup.

Electoral democracy

One of our covariates is VDEM's measure of electoral democracy *(v2x_polyarchy)*. It measures "to what extent is the ideal of electoral democracy in its fullest sense achieved?" It is an index that ranges from 0 to 1, with larger numbers representing more democracy.

More specifically, the index is created by "taking the average of, on the one hand, the weighted average of the indices measuring freedom of association thick *(v2x_frassoc_thick)*, clean elections *(v2xel_frefair)*, freedom of expression *(v2x_freexp_altinf)*, elected officials *(v2x_elecoff)*, and suffrage *(v2x_suffr)* and, on the other, the five-way multiplicative interaction between those indices" (Coppedge et al., 2021; Teorell et al., 2019).

Political violence

We also include in our estimations a variable measuring political violence. It is from VDEM and is called *v2caviol*. It asks, "how often have non-state actors used political violence against persons this year?" There are four possible responses (Coppedge et al., 2021; Pemstein et al., 2022):

0: Not at all
1: Rare
2: Occasionally
3: Frequently
4: Often

APPENDIX C: CONSTITUTIONAL CHANGES BEFORE AND AFTER COUPS

We use data from the Chronology of Constitutional Events, version 1.3, from the Comparative Constitutions Project to compile a list of constitutional changes before and after coups in our treated countries (Elkins et al., 2020). Pretreatment consists of the 5 years before the coup and posttreatment consists of the coup year and the subsequent 4 years. Table C1 reports the results.

TABLE C1 Constitutional changes in our treated cases.

Change	Pretreatment	Posttreatment	Percent increase
Amendment	37	47	27
New	24	34	42
Interim	7	11	57
Reinstated	0	5	Infinite
Suspension	2	23	1,000

property rights in Africa, see Ayittey (1998). Tusalem (2000) provides a good summary of research on this topic for Asia.