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**Working Paper**

## International Sanctions and Corruption

ILE Working Paper Series, No. 91

**Provided in Cooperation with:**

University of Hamburg, Institute of Law and Economics (ILE)

*Suggested Citation:* Gutmann, Jerg; Langer, Pascal; Neuenkirch, Matthias (2026) : International Sanctions and Corruption, ILE Working Paper Series, No. 91, University of Hamburg, Institute of Law and Economics (ILE), Hamburg

This Version is available at:

<https://hdl.handle.net/10419/340063>

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WORKING PAPER SERIES

# International Sanctions and Corruption\*

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Working Paper 2026 No. 91

April 2026



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# International Sanctions and Corruption<sup>\*</sup>

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First Draft: August 25, 2025

This Version: April 20, 2026

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<sup>\*</sup>The authors gratefully acknowledge comments and suggestions by Raul Caruso, Mohammad Reza Farzanegan, Pierre-Guillaume Méon, Günther Schulze, Jan-Egbert Sturm, and participants of the 9th PEDD Conference in Münster 2026 and the 53rd Annual Meeting of the European Public Choice Society in Madrid 2026.

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# International Sanctions and Corruption

## Abstract

A major concern about the imposition of international sanctions is that they may permanently deteriorate the quality of institutions in target countries, potentially causing an increase in corruption. While case studies suggest that this is frequently the case, systematic evidence is so far missing. We provide the first cross-country statistical analysis of the impact of sanctions on public-sector corruption. Using a panel difference-in-differences model and an event study approach, we analyze sanctions against 125 countries from 1971 to 2019. Our results show that Western (and UN) sanctions cause a significant decline of corruption in democracies, while non-Western sanctions and those targeting autocracies have no systematic impact. Event study estimates time the reductions in corruption at about three to four years into the sanctions episode. They persist throughout the sanctions period, but once sanctions are lifted, corruption levels revert to their pre-treatment baseline, indicating that the corruption-reducing effect is limited to the duration of the sanctions episode. Further analysis reveals that the effect is stronger when sanctions explicitly target democratization or human rights improvements.

**JEL Codes:** D73; F51; K33; K42.

**Keywords:** International sanctions; Corruption; Governance.

# 1 Introduction

Empirical research has demonstrated the detrimental consequences of international sanctions for target economies (Gutmann et al. 2023a, 2023b; Neuenkirch and Neumeier 2015). Given that many economic and social effects of sanctions are well-documented (see Drezner 2024; Gutmann and Neuenkirch 2025; Gutmann et al. 2025, for surveys of the literature), recent research has begun to examine the development path of countries after sanctions are lifted. For example, Allen and Webb (2025) study changes in government health spending in the aftermath of sanctions and find that increases are small compared to the spending cuts incurred during sanctions episodes. Gutmann et al. (2023a) detect no evidence of a recovery of the economy in the three years after sanctions are lifted. To determine if and how sanctions can permanently shift targets onto a new development path, it is crucial to understand whether they change not only government policies but also the quality of institutions.<sup>1</sup> Tan and La (2025) provide the first empirical evidence linking post-sanctions economic recovery to domestic institutional quality, identified as a key determinant of success.

A particularly interesting transmission channel is the effect of sanctions on corruption, that is, on the abuse of entrusted power for private gain. Corruption is often described as a cancer due to its persistence.<sup>2</sup> Once societies develop a culture in which corruption is normalized, rooting out bribery and grand corruption has proven itself to be extremely challenging. Although it is frequently claimed that sanctions affect corruption, systematic empirical evidence on their relationship is surprisingly scarce.

This is the first cross-country statistical analysis of how sanctions affect public-sector corruption. Bierstecker et al. (2016) suggest that the majority of targeted UN sanctions coincide with an increase in corruption. Drawing on post-Cold War case study evidence, Andreas (2005) argues that comprehensive sanctions by the interna-

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1. Gutmann, Neuenkirch, and Neumeier (2020) find no effect of sanctions on the protection of economic rights, but an improvement in women's economic rights. Peksen (2017) and Lee et al. (2023), in contrast, find a negative effect on economic freedom and the protection of foreign property.

2. For example, James D. Wolfensohn, who later became the World Bank President, urged the Bank in a speech in 1986 to confront "the cancer of corruption".

tional community promoted organized crime and corruption in target countries, which blocked much needed reforms even long after sanctions were lifted.

At the same time, international sanctions have become a popular tool for governments to fight corruption (Moiseienko 2024).<sup>3</sup> And in many cases, government officials sanctioned for corruption were successfully forced out of office (Hamilton et al. 2024). Sanctions have been used, more generally, to replace unaccountable political leaders and to induce democratic change. They frequently target powerful business people or firms with close connections to the government and might, thereby, undermine corruption networks. In light of this theoretical and empirical ambiguity, a systematic evaluation of the corruption effects of sanctions appears overdue.

In our empirical analysis, we employ two estimation strategies: a panel difference-in-differences (DiD) model and an event study approach. The dataset combines information from the Global Sanctions Data Base (GSDB; Felbermayr et al. 2020) and corruption measures from the Varieties of Democracy (V-Dem) dataset (Coppedge et al. 2025), covering 5,942 country-year observations and sanctions against 125 countries from 1971 to 2019. We differentiate the effects of sanctions by three categories of senders: UN; Western (EU or US); and non-Western (China or Russia) and two types of target regimes: democracies and autocracies.

Our results indicate that Western sanctions have a strong and statistically significant negative effect on corruption in democracies. Similar patterns emerge for UN sanctions targeting democracies, although based on fewer observations and with lower statistical significance. Non-Western sanctions and sanctions targeting autocracies do not exert systematic effects on corruption. Our event study estimates reveal no significant pre-trends before Western sanctions, strengthening the case for a causal interpretation. Significant reductions in corruption emerge from year 3 or 4 onward and remain robust throughout the remainder of the sanctions episode. Once sanctions are lifted, corruption levels return to their pre-treatment baseline, suggesting that the

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3. In the United States, the Magnitsky Act 2016 for the first time provided an explicit legal basis for imposing international sanctions to punish corruption. Hamilton et al. (2024) discuss comparable legislation in other Western jurisdictions.

corruption-reducing effect of longer-lasting sanctions is confined to the duration of the sanctions episode and does not persist in the post-treatment period. An analysis of effect heterogeneity across different sanction objectives reveals that the effect is stronger when sanctions are explicitly aimed at promoting democratization or improving human rights. Finally, our results remain robust when modeling persistence in corruption and when applying the two-stage DiD estimator proposed by Gardner et al. (2025) and Butts and Gardner (2022) to account for treatment effect heterogeneity and staggered treatment timing.

The remainder of this article is structured as follows. Section 2 presents theoretical arguments for corruption inducing and inhibiting effects of international sanctions and discusses whether these effects may differ between political regime types. Next, Section 3 introduces difference-in-differences estimation and event study designs as our empirical estimation strategy, as well as the corruption and sanctions data used in our empirical analysis. In Section 4, we discuss our empirical findings alongside several extensions and robustness tests, before Section 5 concludes.

## 2 Theory

Corruption can be defined as any abuse of entrusted power for private gain.<sup>4</sup> Since we are interested here only in public-sector corruption, a narrower definition commonly associated with the World Bank is adopted in our theoretical discussion and empirical analysis: Corruption is, accordingly, the abuse of public office for private gain. Public-sector corruption is limited to the vertical relationships between citizens and representatives of the state and excludes abuse of entrusted power in horizontal relationships among citizens or businesses (Gutmann and Lucas 2018; Hogdson and Jiang 2007).

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4. Corruption is frequently associated with the shadow economy. While significantly correlated in practice, conceptually these activities are distinct. The shadow economy refers to economic activity that is concealed from public authorities and typically does not involve any abuse of power. There is, however, a symbiotic relationship between the two, as bribes may have to be paid to avoid the detection or prosecution of shadow economy activity.

A key reason why sanctions are expected to promote corruption in the public sector is that sanctions create scarcity, which in turn provides strong incentives for sanction busting. Such black market activities are often connected to and protected by government officials (as argued, e.g., by Andreas 2005). Consistent with this argument, empirical evidence suggests that sanctions shift a significant share of a country's economic activity into the informal sector (Early and Peksen 2019; Farzanegan 2013; Kelishomi and Nisticò 2024). For firms in the formal economy, evading sanctions is more risky, as they and their management might themselves become the target of sanctions and criminal prosecution.<sup>5</sup> Corruption can also result from powerful industries in target countries enjoying the protection from international competition due to sanctions (Pond 2017). Alternatively, the government may even be directly involved in cartelizing markets under sanctions (Kaempfer and Lowenberg 1999). In sum, the market distortions created by economic sanctions pose various opportunities for rent-seeking activity, including outright public-sector corruption. This argument yields our first hypothesis.

**Hypothesis 1.** *Sanctions cause an increase in public-sector corruption.*

If pervasive, corruption can become persistent despite anti-corruption measures and incentives once it establishes itself in social norms (Mishra 2006). Tirole (1996) demonstrates formally how a short-run increase in corruption due to a shock may yield permanently higher levels of corruption. Andreas (2005, p. 337) argues in the specific context of international sanctions that “reestablishing societal acceptance of legal norms can be one of the most challenging tasks after sanctions are lifted, as old habits can be difficult to break.” This can be expressed as our second hypothesis, conditional on Hypothesis 1 being supported by the data:

**Hypothesis 2.** *The increase in public-sector corruption from sanctions persists even after sanctions are lifted.*

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5. Former world chess champion Robert Fisher had to spend the remaining 16 years of his life abroad, after violating US sanctions on Yugoslavia by playing a rematch against Boris Spassky. This case illustrates the potentially exorbitant personal cost of being caught violating international sanctions.

However, for sanctions to establish social norms that support an equilibrium of corruption, sanctions episodes would have to extend over longer periods of time. Since most sanctions episodes only last for a few years, one may question if a typical sanctions episode is indeed impactful enough to cause a lasting increase in corruption. In stark contrast to some experts' concerns that sanctions inevitably corrode target countries' institutional fabric and open the door to pervasive and persistent public-sector corruption is some policymakers' optimism that sanctions are a powerful tool in fighting corruption internationally (Moiseienko 2024). Some have even argued that, in practice, the line between international sanctions and anti-corruption enforcement is blurred, as they can serve the same purposes (Tomashevskiy 2021). Today, sanctions are frequently imposed with the explicit goal to reduce corruption.<sup>6</sup> Even if not imposed with the explicit goal of reducing corruption, sanctions often have the goal to induce democratic change, to enhance governance quality and political accountability, or to replace the political establishment of a country. This mix of institutional disruption and incentives for reform may allow sanctions to weaken corruption networks. This yields our third hypothesis, which predicts the opposite of Hypothesis 1.

**Hypothesis 3.** *Sanctions cause a decline in public-sector corruption.*

Differences between regime types in how corruption levels respond to international sanctions should be caused primarily by how responsive different political regimes are to the pressure exerted by sanctions. Allen (2005) argues and provides evidence that democracies are more likely forced to make concessions due to sanctions than autocracies. Many studies have confirmed the result that it is primarily democratic sanction targets that are willing to make concessions (e.g., Allen 2008; Eichenberger and Stadelmann 2022; Lektzian and Souva 2007; Major 2012). If this applies also to concessions regarding governance-enhancing reforms, fighting corruption, and removing or extraditing corrupt public officials, sanctions against democracies should be more effectively reducing corruption than those against non-democracies. This argumenta-

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6. In 2021, the United States, for example, sanctioned numerous businesspeople and (former) public officials and their family members across Latin America for their involvement in corruption or obstruction of corruption investigations.

tion can be summarized in our fourth and final hypothesis, which is a more refined, conditional version of Hypothesis 3.

**Hypothesis 4.** *Sanctions cause a decline in public-sector corruption specifically in democracies.*

### 3 Estimation Strategy and Data

#### 3.1 Estimation Strategy

To evaluate how sanctions affect target country public-sector corruption, we follow Gutmann et al. (2023a) and Gutmann et al. (2024) and estimate two complementary empirical specifications: a *panel DiD model* and an *event study*.

First, the panel DiD model can be formally described as follows:

$$y_{i,t} = \sum_{s=1}^6 \beta_{sanc}^s D_{sanc,i,t-1}^s + \gamma_1 X_{i,t-1}^{pol} + \gamma_2 X_{i,t-1}^{econ} + \alpha_i + \tau_t + \epsilon_{i,t} \quad (1)$$

The dependent variable,  $y_{i,t}$ , measures the level of corruption based on country expert assessments.<sup>7</sup> The analysis is conducted at the country-year level, where  $i$  denotes a country in year  $t$ . Our key explanatory variables,  $D_{sanc,i,t-1}^s$ , are a set of binary sanction indicators that are specific to both the sanction sender (Western, UN, or non-Western) and the regime type of the target country (democracy or autocracy).<sup>8</sup> Western sanctions are defined here as those non-UN sanctions imposed by the US or the EU, non-Western sanctions refer to those non-UN sanctions imposed by China or Russia. Each dummy takes the value 1 if a sanction of the corresponding type is in effect against country  $i$  in year  $t$ , and 0 otherwise. To address potential reverse causality, the sanc-

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7. Subjective corruption indicators can systematically deviate from more objective measures of corruption (Gutmann, Padovano, et al. 2020; Olken 2009). Given that the existing qualitative empirical literature suggests that sanctions lead to an increase in corruption (e.g., Andreas 2005), one would expect that perception-based indicators, if at all, overestimate corruption during sanctions episodes.

8. Specifically, the six dummies capture: (i) Western sanctions against democracies, (ii) Western sanctions against autocracies, (iii) UN sanctions against democracies, (iv) UN sanctions against autocracies, (v) non-Western sanctions against democracies, and (vi) non-Western sanctions against autocracies.

tion variables are lagged by one year in the DiD specification, such that the treatment precedes the observed outcome.

To account for unobserved heterogeneity across countries and over time, Eq. (1) includes country,  $\alpha_i$ , and year fixed effects,  $\tau_t$ . Country fixed effects absorb all time-invariant characteristics of countries, such as their geography, colonial history, or long-standing institutional features. Year fixed effects account for global shocks or trends that may affect corruption levels or sanctioning behavior simultaneously across countries.  $\epsilon_{i,t}$  is an idiosyncratic error.

In addition, the model includes time-varying control variables that could confound the relationship between sanctions and corruption (Gutmann et al. 2023a; Gutmann et al. 2024; Lohaus and Bussmann 2021). These consist of a vector of political indicators,  $X_{i,t-1}^{pol}$ , and economic variables,  $X_{i,t-1}^{econ}$ . Both sets of control variables are described in the following subsection. As it is reasonable to assume that economic and political conditions in a country affect public-sector corruption with some delay, and to reduce the potential for reverse causality, all control variables are also lagged by one year.

Second, to explore how the impact of Western sanctions unfolds over time, combined with an evaluation of potential pre- and post-treatment trends, we supplement the DiD analysis with an event study design (Dai et al. 2021; Gutmann et al. 2023a; Gutmann et al. 2024; Schmidheiny and Siegloch 2023). This specification includes a series of binary indicators for years before and after a sanctions episode, allowing for the estimation of dynamic treatment effects and enabling a test of the parallel trends assumption by examining whether corruption levels were already changing prior to the imposition of sanctions. Moreover, it enables us to test Hypothesis 2, which suggests that the effect of sanctions on corruption persists even after the sanctions are lifted. Specifically, we include indicators for the years three and two preceding the imposition of sanctions, dummies for each of the first ten years under sanctions, a pooled indicator for the years eleven and beyond (as only few sanctions episodes last that long), and indicators for each of the first three years following the lifting of sanctions.

The year immediately prior to the start of sanctions serves as the reference category (Callaway and Sant’Anna 2021; Miller 2023).

$$\begin{aligned}
y_{i,t} = & \beta_{no} D_{no,i,t}^{West \times Regime} + \sum_{l=2}^3 \beta_{pre,-l} D_{pre,i,t-l}^{West \times Regime} + \sum_{l=1}^{11+} \beta_{sanc,l} D_{sanc,i,t,l}^{West \times Regime} \\
& + \sum_{l=1}^3 \beta_{post,+l} D_{post,i,t+l}^{West \times Regime} + \sum_{s=1}^5 \beta_{sanc}^s D_{sanc,i,t}^s + \gamma_1 X_{i,t-1}^{pol} + \gamma_2 X_{i,t-1}^{econ} + \alpha_i + \tau_t + \epsilon_{i,t}
\end{aligned} \tag{2}$$

The event study specification incorporates all control variables,  $X_{i,t-1}^{pol}$  and  $X_{i,t-1}^{econ}$ , country fixed effects,  $\alpha_i$ , year fixed effects,  $\tau_t$ , and an idiosyncratic error term,  $\epsilon_{i,t}$ , as defined in the DiD model in Eq. (1). These estimations focus on Western sanctions, which account for the vast majority of country-years subject to sanctions (see Section 3.2 below). UN and non-Western sanctions are included only as control variables, as disaggregating each into eleven separate dummies would yield too few observations per coefficient estimate for reliable inference. The core explanatory variables are a series of dummies,  $D_{sanc,i,t,l}^{West \times Regime}$ , which indicate whether target country  $i$  is subject to a Western sanction in the  $l^{th}$  consecutive year since the onset of the sanctions episode in year  $t$ . As previously explained, we further distinguish between autocratic and democratic regime types in the target country in two separate estimations.<sup>9</sup>  $D_{pre,i,t-l}^{West \times Regime}$  and  $D_{post,i,t+l}^{West \times Regime}$  capture potential pre-trends and the persistence of sanction effects beyond the duration of a sanctions episode. A vector of additional sanction dummies,  $D_{sanc,i,t}^s$ , is included to control for other concurrent sanction types not under direct examination.<sup>10</sup> This ensures that the estimated effects can be attributed exactly to the targeted category of Western sanctions by regime type. Finally,  $D_{no,i,t}^{West \times Regime}$  redefines the reference category by identifying all observations not exposed to the treatment or its surrounding pre- and post-periods. Therefore, the estimated coefficients on the sanction indica-

9. In contrast to the DiD estimations above, the sanction variables are not lagged, as it is designed to trace the dynamic effects of sanctions over time and relative to the year before imposition. This allows for the estimation of both pre-treatment trends and post-treatment dynamics, providing a more detailed temporal perspective on how corruption levels evolve around the imposition of sanctions.

10. These are (i) UN sanctions against democracies, (ii) UN sanctions against autocracies, (iii) non-Western sanctions against democracies, (iv) non-Western sanctions against autocracies, and (v) Western sanctions against democracies or autocracies, when the focus is on the other type, respectively.

tors reflect differences to the baseline year immediately preceding sanction imposition ( $t - 1$ ) (Callaway and Sant'Anna 2021).

All regressions are estimated using ordinary least squares with standard errors clustered at the country level, ensuring robust inference in the presence of potential country-specific serial correlation and heteroskedasticity. As part of our robustness tests, we repeat the baseline DiD estimations with the two-stage DiD estimator by Gardner et al. (2025) and Butts and Gardner (2022).

### 3.2 Data

Corruption, our dependent variable, is measured using four conceptually overlapping indices from the V-Dem dataset (Coppedge et al. 2025). V-Dem data has a much greater time coverage than other established datasets, which is particularly important for our event study analysis.<sup>11</sup> All four V-Dem indices are continuous variables ranging from 0 to 1, with higher values indicating more public-sector corruption. The *Political Corruption Index* measures how pervasive political corruption is. It reflects corruption across all branches of government, including the executive, legislature, and judiciary corruption (McMann et al. 2016; Coppedge et al. 2025). The *Regime Corruption Index* is a narrower measure focused on the extent to which political office is used by regime actors for private or political gain (Sigman and Lindberg 2019; Pemstein et al. 2025; Coppedge et al. 2025). The *Executive Corruption Index* captures the frequency with which members of the executive engage in bribery, embezzlement, or other forms of inappropriate use of public funds or personal enrichment (McMann et al. 2016; Coppedge et al. 2025). Finally, the *Public Sector Corruption Index* measures to what extent public sector employees grant favors in exchange for bribes, kickbacks, or other material inducements, and how often they steal, embezzle, or misappropriate public funds or other state resources (McMann et al. 2016; Coppedge et al. 2025). The bi-

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11. For instance, the Worldwide Governance Indicators (WGI) are only available from 1996 onward, and the Corruption Perceptions Index (CPI) provides annual coverage only from 2012 onward.

ivariate correlations between the indices range from 0.92 to 0.98. They are, thus, very closely aligned, but not identical.<sup>12</sup>

Our main explanatory variable is a binary indicator for the presence of international sanctions. The data is from the *Global Sanctions Data Base* (GSDB) (Felbermayr et al. 2020; Kirikakha et al. 2021; Syropoulos et al. 2024), which provides detailed coverage of sanctions episodes initiated by major actors, such as the United Nations, the United States and the European Union (referred to here as Western countries), as well as Russia and China (which we label non-Western countries).

To control for confounding factors that may influence corruption and are possibly associated with the probability of being sanctioned, we include several time-varying country-level covariates. Economic conditions are accounted for via the lagged natural logarithm of real GDP per capita and the lagged share of natural resource rents in GDP. Additionally, we control for income inequality using the lagged top 1 percent pre-tax income share from the World Inequality Database (World Income Database 2025).

Political and institutional factors are also considered. These comprise a binary democracy indicator by Bjørnskov and Rode (2020), which is based on a minimalist definition of electoral democracy, as well as the latent human rights protection score developed by Fariss (2019). Finally, we control for the occurrence of armed conflict using data from the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002; Davies et al. 2022). We distinguish between minor conflicts (25–999 battle-related deaths) and wars (1,000+ deaths). We further distinguish interstate and intrastate conflicts, with and without foreign military intervention. All political and institutional control variables are lagged by one year to mitigate concerns about potential reverse causality.

Our dataset comprises 5,942 observations for the period 1971 to 2019, including 1,545 country-years under sanctions (see Table A4). Western sanctions account for the vast majority of cases (1,046 observations against 96 countries), while UN and

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12. To further validate these measures, we compare them with the CPI and WGI's Control of Corruption indicator (CoC) for the time periods for which these data are available. The correlations between the V-Dem indices and the CPI range from  $-0.84$  to  $-0.90$ , while the correlations with CoC range from  $-0.86$  to  $-0.91$ . The negative sign reflects the inverse scaling of the measures. However, the magnitude of these correlations indicates a strong alignment between the V-Dem measures and alternative established corruption indicators.

non-Western sanctions are observed in only 249 country-years (against 26 countries) and 284 country-years (against 44 countries), respectively. A closer look at these two categories reveals that UN sanctions against democracies (43 observations) and non-Western sanctions against autocracies (32 observations) are particularly rare. Accordingly, the corresponding estimates should be interpreted with caution.

The countries included in the analysis are listed in Table A1. Definitions and data sources for all variables are provided in Table A2, while descriptive statistics are reported in Table A3. Sanctioned countries tend to exhibit higher levels of corruption, lower economic development, more severe human rights violations, are less likely democratic, and experience more conflict. These differences underscore the importance of accounting for selection effects to ensure conditional parallel trends when identifying the causal impact of sanctions.

A closer look at countries that improved or worsened considerably during a sanctions episode reveals some interesting patterns. For example, Sudan experienced a marked increase in all four corruption dimensions under Western sanctions between 1989 and 1995, following the 1989 military coup that ended a short-lived democratic regime. In contrast, Myanmar showed substantial improvement in corruption under Western sanctions between 1988 and 2019, despite remaining predominantly under autocratic rule, suggesting gradual institutional change. Under UN sanctions, Eritrea deteriorated noticeably between 2000 and 2001, following the end of the war with Ethiopia. During this period, the country remained a highly consolidated autocracy with declining civil liberties. In contrast, Serbia improved considerably between 1998 and 2001 under UN sanctions, a period that coincides with the transition from the Milošević regime to a more democratic political system after 2000. Overall, the largest swings in the corruption indices in either direction are mostly observed in autocratic regimes. However, a more rigorous empirical analysis is warranted given that several of these sanction episodes coincide with major economic and political disruptions, such as wars, coups, or regime transitions.

## 4 Empirical Results

### 4.1 Baseline Results

**Average Treatment Effects.** Table 1 presents the results of the DiD estimations for the four corruption indices. Western sanctions have a strong and statistically significant negative effect on corruption in democracies, supporting Hypotheses 3 and 4. Depending on the specific indicator, the estimated effect ranges from  $-0.052$  to  $-0.067$  on the normalized 0–1 scale.<sup>13</sup> Similar patterns emerge for UN sanctions targeting democracies, although based on fewer observations and with lower statistical significance; here, the estimated reduction in corruption ranges from  $-0.055$  to  $-0.090$ . Hypothesis 1 receives partial support only in the case of UN sanctions against autocracies: for the broadest index, political corruption, we observe a statistically significant increase by 0.043. In general, we find no systematic effects of non-Western sanctions and of sanctions targeting autocracies. One possible explanation for the latter null result is that the survival of autocratic regimes often relies on patronage networks and rent distribution, in which corruption plays a central role. In such settings, corruption is a key mechanism for maintaining political loyalty. As a result, external sanctions may be less effective in reducing corruption, as doing so could undermine the regime’s stability.

Turning to the control variables, only real GDP per capita exhibits a consistent effect across all four specifications: more economically developed countries tend to display lower levels of corruption (a well-established pattern in the corruption literature, see Treisman 2007). Given the persistent nature of corruption, much of the variation is likely captured by the country fixed effects. This is supported by the high  $R^2$  values, which range from 0.89 to 0.93, dramatically higher than the within- $R^2$  of 0.07 to 0.08. We revisit the issue of persistence in the robustness checks below, where we additionally include a lagged dependent variable.

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13. Further disaggregation reveals that the effect is primarily driven by country-years in which both US and EU sanctions were in place. However, the point estimates for country-years with only US or only EU sanctions are qualitatively similar. We do not report these results in detail (available upon request), as they are based on relatively few observations for some sender-target combinations.

Table 1: Baseline Results

	(1) Regime Corruption	(2) Political Corruption	(3) Executive Corruption	(4) Public Sector Corruption
Sanctions				
... lag Western ag. Democracies	-0.0619*** (0.0195)	-0.0519*** (0.0148)	-0.0672*** (0.0225)	-0.0558*** (0.0163)
... lag Western ag. Autocracies	0.0166 (0.0143)	0.0109 (0.0118)	0.0215 (0.0159)	0.0147 (0.0164)
... lag UN against Democracies	-0.0748** (0.0289)	-0.0549* (0.0327)	-0.0902*** (0.0272)	-0.0734*** (0.0273)
... lag UN against Autocracies	0.0143 (0.0254)	0.0431** (0.0213)	-0.0071 (0.0298)	0.0343 (0.0271)
... lag Non-Western ag. Democ.	-0.0003 (0.0202)	-0.0100 (0.0189)	0.0017 (0.0202)	-0.0237 (0.0199)
... lag Non-Western ag. Autoc.	0.0732 (0.0693)	0.0300 (0.0443)	0.0927 (0.0699)	0.0132 (0.0264)
lag $\log(\text{GDP pc})$	-0.0621** (0.0247)	-0.0537** (0.0207)	-0.0683** (0.0275)	-0.0588** (0.0242)
lag Democracy	-0.0337 (0.0258)	-0.0224 (0.0237)	-0.0432 (0.0265)	-0.0245 (0.0274)
lag Human Rights	-0.0108 (0.0093)	-0.0122 (0.0077)	-0.0095 (0.0103)	-0.0170* (0.0093)
lag Resources Rents Share	0.1359 (0.0867)	0.1043 (0.0748)	0.1523 (0.0935)	0.1270 (0.0928)
lag Income Inequality	-0.1565 (0.2992)	-0.2435 (0.2381)	-0.1094 (0.3378)	-0.2850 (0.2725)
Interstate Conflicts				
... lag Minor	-0.0103 (0.0212)	-0.0180 (0.0247)	-0.0033 (0.0172)	-0.0126 (0.0173)
... lag Major	-0.0047 (0.0149)	-0.0253 (0.0156)	0.0015 (0.0158)	-0.0236* (0.0122)
Internal Conflicts w/o Interv.				
... lag Minor	-0.0027 (0.0122)	-0.0040 (0.0102)	-0.0032 (0.0140)	-0.0092 (0.0112)
... lag Major	-0.0056 (0.0222)	-0.0160 (0.0172)	-0.0021 (0.0269)	-0.0267 (0.0180)
Internal Conflicts w/ Interv.				
... lag Minor	-0.0220 (0.0190)	-0.0256 (0.0166)	-0.0192 (0.0259)	-0.0188 (0.0224)
... lag Major	-0.0247 (0.0218)	-0.0414 (0.0291)	-0.0227 (0.0212)	-0.0464 (0.0433)
Observations	5,843	5,843	5,843	5,843
R <sup>2</sup>	0.9119	0.9322	0.8913	0.9049
Within-R <sup>2</sup>	0.0747	0.0769	0.0765	0.0688

Notes: Coefficient estimates of Eq. (1). Standard errors in parentheses are clustered at the country level. Models include country and year fixed effects. \*\*\*/\*\*/\* indicates significance at the 1%/5%/10% level.

**Effects of Sanctions over Time.** Figure 1 shows the point estimates and corresponding 95% confidence intervals for Western sanctions targeting democracies, covering

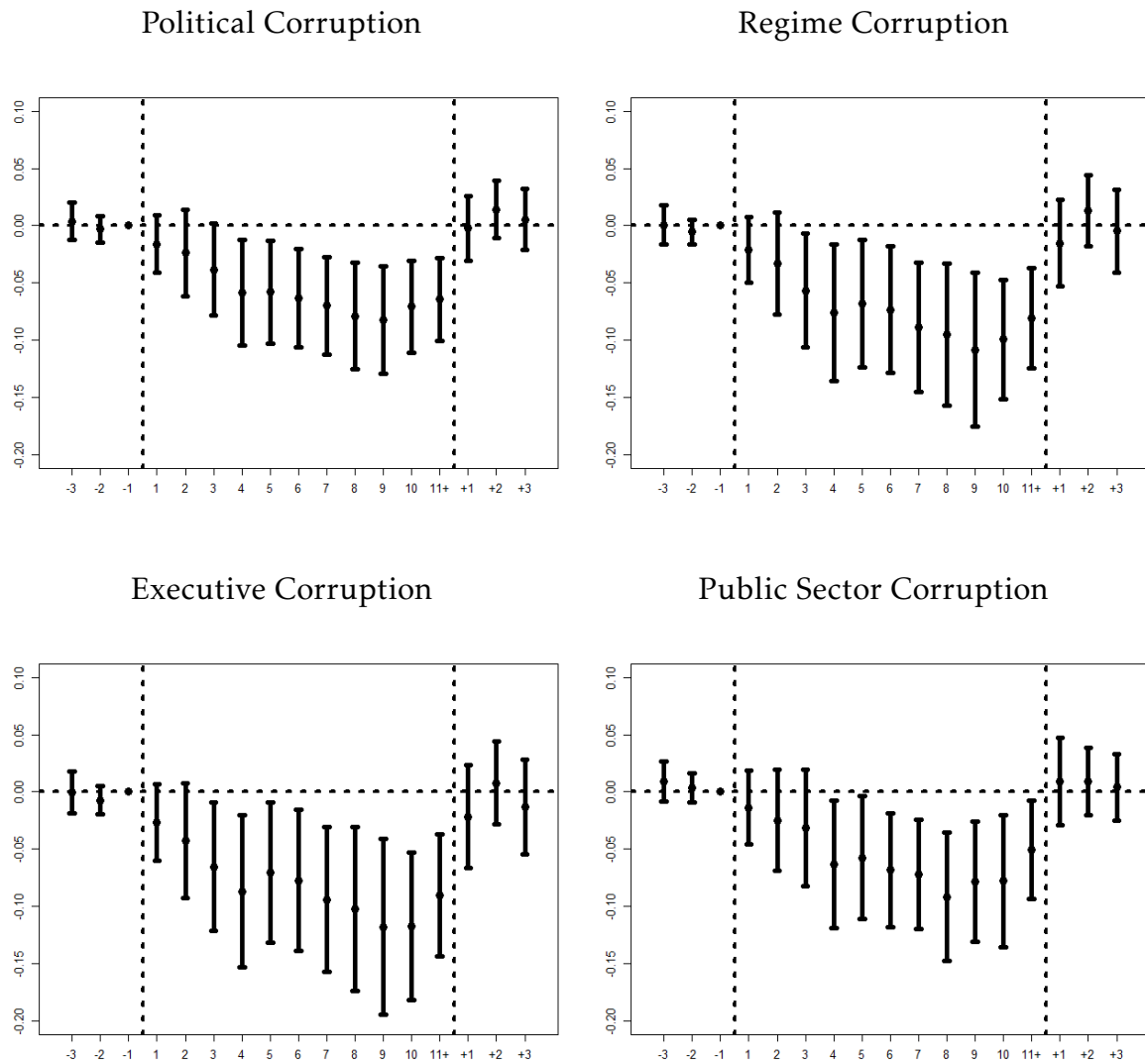
the three years prior to a sanctions episode ( $-3, -2, -1$ ), the duration of the episode ( $1, 2, \dots, 11+$ ), and the three years following its conclusion ( $+1, +2, +3$ ). The level of corruption in the year immediately preceding implementation ( $-1$ ) is normalized to zero (Callaway and Sant’Anna 2021; Miller 2023). Consequently, all estimates are interpreted relative to this reference year, allowing for an assessment of dynamic treatment effects and pre-/post-trends. The focus on Western sanctions against democracies is motivated by the results in Table 1, which show that they are the only sanctions with a robust effect on corruption, while also providing a sufficient number of observations for a meaningful event study (see also Table A4).<sup>14</sup>

No significant pre-trends are observed prior to the imposition of Western sanctions. The estimated effects during the treatment period, therefore, represent a clear departure from the pre-treatment situation and trajectory, making the case for a causal interpretation of the results. Significant reductions in corruption emerge from year 3 onward for regime and executive corruption, and from year 4 for political and public sector corruption. These effects remain statistically significant throughout the remainder of the sanctions episode. The estimated impact peaks in year 9, ranging from  $-0.082$  to  $-0.118$ , except in the case of public sector corruption, which reaches its maximum in year 8. Once sanctions are lifted, corruption levels return to their pre-treatment level. In an extension (available upon request), we split Western sanctions against democracies into two subgroups: those lasting up to five years and those lasting longer. The significant effects from year 3 or 4 onward are indeed driven by longer-lasting sanctions. However, we find the same post-sanction pattern for both groups, suggesting that the corruption-reducing effect of longer-lasting sanctions is confined to the sanctions episode and does not persist beyond it. This result is similar to the finding of Blanton and Peksen (2025) that the presence of a UN peacekeeping mission in a country suppresses the shadow economy, but this effect dissipates after the mission’s conclusion.

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14. Figure A1 presents the corresponding event study results for Western sanctions against autocracies, where no statistically significant effects are observed before, during, or after the sanctions episodes.

Figure 1: Effects of Western Sanctions against Democracies on Corruption over Time



*Notes:* Effects of sanctions over time (1, 2, ..., 11+) alongside pre-trend (-3, -2, -1) and post-trend (+1, +2, +3) according to an estimation of Eq. (2) for different dependent variables. Standard errors are clustered at the country level. Models include control variables (other lagged sanction indicators, lag  $\log(\text{GDP pc})$ , lag Democracy, lag Human Rights, lag Resources Rents Share, lag Income Inequality, and six lagged conflict indicators), country fixed effects, and year fixed effects. 95% confidence bands are indicated by whiskers.

## 4.2 Extensions and Robustness Tests

**Effect Heterogeneity.** The GSDB identifies nine reasons for imposing sanctions based on official documentation: democracy promotion, human rights protection, regime destabilization, policy change, war prevention, war termination, territorial conflict, counterterrorism, and other motives. A priori, one would expect sanctions explicitly aimed at democratization or human rights improvements to have a more positive effect

on corruption than those driven by other objectives.<sup>15</sup> To examine this effect heterogeneity, we disaggregate Western sanctions into two categories: one for sanctions with the stated objective of promoting democracy or human rights, and another for sanctions pursuing any of the remaining goals. Sanctions with both types of objectives are excluded from the analysis to ensure a clean separation of the two effects. We re-run the baseline estimations while excluding sanction cases in the two categories one category at a time. The results of this jackknife-style extension are presented in Table 2. It is worth noting that UN and non-Western sanctions explicitly and specifically aimed at promoting democracy or human rights are too rare to allow for a meaningful disaggregation of these categories (see also Table A4). Accordingly, these variables are included only as controls in this extension.

As one would expect, the corruption-reducing effect is stronger when sanctions are explicitly aimed at promoting democratization or improving human rights. This difference is particularly pronounced for regime and executive corruption, compared to the broader indices of political and public sector corruption.

**Accounting for Autocracy-Democracy Transitions.** Next, we examine whether our results are driven by sanctions that coincide with countries' transition from autocracy to democracy, or vice versa. To this end, we add ten dummy variables identifying the first to fifth year after each type of transition to our baseline DiD specification. Our key results remain virtually unchanged, and the transition dummies are almost never statistically significant.<sup>16</sup> Hence, our findings are not driven by transition phases.

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15. However, human rights sanctions are also not successful in improving human rights (Steinbach et al. 2023).

16. Results available upon request.

Table 2: Effect Heterogeneity for Sanction Objectives

	<b>Regime Corruption</b>		<b>Political Corruption</b>	
	<b>Democrat. &amp; Human Rights</b>	<b>Other Objectives</b>	<b>Democrat. &amp; Human Rights</b>	<b>Other Objectives</b>
Sanctions				
... lag Western ag. Democracies	-0.1035*** (0.0379)	-0.0502** (0.0214)	-0.0624** (0.0260)	-0.0493*** (0.0167)
... lag Western ag. Autocracies	0.0250 (0.0202)	-0.0218 (0.0171)	0.0249 (0.0186)	-0.0268 (0.0141)
Observations	4,664	5,159	4,664	5,159
R <sup>2</sup>	0.919	0.919	0.937	0.936
Within-R <sup>2</sup>	0.066	0.049	0.067	0.058
	<b>Executive Corruption</b>		<b>Public Sector Corruption</b>	
	<b>Democrat. &amp; Human Rights</b>	<b>Other Objectives</b>	<b>Democrat. &amp; Human Rights</b>	<b>Other Objectives</b>
Sanctions				
... lag Western ag. Democracies	-0.1257*** (0.0471)	-0.0494** (0.0239)	-0.0630** (0.0303)	-0.0496*** (0.0183)
... lag Western ag. Autocracies	0.0294 (0.0219)	-0.0174 (0.0187)	0.0475 (0.0315)	-0.0352** (0.0142)
Observations	4,664	5,159	4,664	5,159
R <sup>2</sup>	0.900	0.901	0.908	0.911
Within-R <sup>2</sup>	0.067	0.046	0.060	0.051

Notes: Coefficient estimates of Eq. (1). Standard errors in parentheses are clustered at the country level. \*\*\*/\*\*/\* indicates significance at the 1%/5%/10% level. The variation in the number of observations reflects the exclusion of sanctions by objective: when analyzing cases aimed at democratization or human rights, sanctions with other stated goals are omitted, and vice versa. Models include further sanctions variables as well as control variables (lag  $\log(\text{GDP pc})$ , lag Democracy, lag Human Rights, lag Resources Rents Share, lag Income Inequality, and six lagged conflict indicators), country, and year fixed effects. Estimates for control variables are available on request..

**Accounting for Corruption Persistence.** As previously noted, corruption is commonly understood to be a slow-moving variable. It is therefore natural to test the robustness of our results to accounting explicitly for persistence in the level of corruption. To this end, we re-estimate Eq. (1) after including a lagged dependent variable.<sup>17</sup> The results are shown in Table A5. Corruption indeed exhibits strong persistence, with coefficients on the lagged dependent variable ranging from 0.900 to 0.915. Relative to the baseline results in Table 1, the inclusion of the lagged dependent variable increases the  $R^2$  values further, reaching 0.98 to 0.99. This suggests that the lagged dependent variable further reduces the potential for omitted variable bias. Crucially, the corruption-reducing effect of Western (and UN) sanctions used against democracies remains robust even when persistence is taken into account. The estimated “long-run effects” of sanctions (calculated by dividing the short-run effect by  $1 - \text{LDV coefficient}$ ) are slightly larger than those in the baseline models.<sup>18</sup>

**Two-Stage DiD Estimator.** As a final robustness check, we apply the two-stage DiD estimator by Gardner et al. (2025) and Butts and Gardner (2022) to account for potential biases arising from the combination of treatment effect heterogeneity and staggered treatment. Rather than estimating group, time, and treatment effects simultaneously, the approach first uses only untreated and not-yet-treated observations to generate adjusted outcomes, controlling for all covariates as well as country and year fixed effects. In a second stage, these adjusted outcomes are regressed on the sanction dummies to estimate treatment effects.<sup>19</sup> Table A6 presents the results. The estimated effects of Western sanctions against democracies remain virtually unchanged compared to Table 1, while the corruption-reducing effects of UN sanctions against democracies appear slightly stronger.

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17. Including a lagged dependent variable in a panel fixed effects model may introduce Nickell bias (Nickell 1981). However, this bias is declining in  $T$  and it has been suggested that already at a panel length of 30 years OLS becomes preferable over GMM estimators for dynamic panel models (Beck and Katz 2011; Judson and Owen 1999). Thus, we report least squares estimates rather than using alternative estimators such as those of Arellano and Bond (1991) or Blundell and Bond (1998).

18. Specifically, these long-run effects are as follows:  $-0.109$  (regime corruption),  $-0.062$  (political corruption),  $-0.134$  (executive corruption), and  $-0.088$  (public sector corruption).

19. Covariates can also be included in the second stage to improve efficiency. In our case, doing so yields virtually identical results (available on request) to those reported in Table A6.

## 5 Conclusion

We provide the first cross-country statistical analysis of how sanctions affect public-sector corruption. Treatment effects are estimated using two main approaches: a panel DiD model and an event study analysis. Our dataset combines information from the Global Sanctions Data Base (Felbermayr et al. 2020) and corruption measures from the Varieties of Democracy dataset (Coppedge et al. 2025), covering 5,942 country-year observations and sanctions against 125 countries between 1971 and 2019. We differentiate sanctions according to three categories of senders: UN; EU or US; and “non-Western” states (i.e., China or Russia) and two types of target regimes: democracies and autocracies.

Our results indicate that Western sanctions have a strong and statistically significant negative effect on corruption in democracies. The estimated effect ranges from  $-0.052$  to  $-0.067$  on the normalized 0–1 scale. Similar patterns emerge for UN sanctions targeting democracies, although based on fewer observations and with lower statistical significance; here, the estimated reduction in corruption ranges from  $-0.055$  to  $-0.090$ . Non-Western sanctions and sanctions targeting autocracies do not exert systematic effects.

Our event study estimates show no significant pre-trends prior to the imposition of Western sanctions. The estimated effects during the treatment period mark a clear departure from the pre-treatment trajectory, strengthening the case for a causal interpretation. Significant reductions in corruption emerge from year 3 or 4 onward and remain robust throughout the remainder of the sanctions episode. The estimated impact peaks in year 8 or 9, ranging from  $-0.082$  to  $-0.118$ . Thereafter, the effects gradually decline. Once sanctions are lifted, corruption levels return to their pre-treatment baseline, suggesting that the corruption-reducing effect of longer-lasting sanctions is confined to the duration of the sanctions episode and does not persist in the post-treatment period (see Blanton and Peksen 2025, for a comparable recovery of the shadow economy after UN peacekeeping missions).

An analysis of effect heterogeneity across different types of objectives reveals that the corruption-reducing effect is stronger when sanctions are explicitly aimed at promoting democratization or improving human rights. Finally, our results remain robust when explicitly modeling persistence in corruption and when applying the two-stage DiD estimator proposed by Gardner et al. (2025) and Butts and Gardner (2022) to account for treatment effect heterogeneity and staggered treatment timing.

Our findings indicate that Western (and UN) sanctions can reduce public-sector corruption in democracies, particularly when they are aimed at promoting democratization or improving human rights. However, the absence of lasting effects once sanctions are lifted suggests that they should not be seen as an effective tool for fighting corruption. While sanctions may temporarily constrain corrupt behavior, they do not appear to generate enduring institutional change. However, more important than any potential reductions in corruption is the mere fact that our results contradict the frequently cited claim derived from qualitative case studies that public-sector corruption would systematically increase under sanctions (Andreas 2005; Bierstecker et al. 2016). Across different types of sanction senders and targets, we do not find support for this popular criticism against the use of sanctions (e.g., Farzanegan 2013; Özdamar and Shahin 2021; Rowhani 2019; Sharma 2024).

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# Appendix

## Data Description

Table A1: List of Countries

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**Target Countries** (Number of total observations; total sanctions; democratization or human rights sanctions; non-democratization and non-human rights sanctions)

Afghanistan (17; 17; 0; 17), Albania (35; 5; 0; 5), Algeria (39; 11; 0; 0), Angola (39; 18; 0; 7), Argentina (39; 10; 8; 1), Armenia (29; 0; 0; 0), Australia (49; 6; 0; 6), Austria (39; 7; 0; 7), Azerbaijan (28; 10; 0; 10), Bahrain (15; 0; 0; 0), Bangladesh (39; 0; 0; 0), Barbados (39; 0; 0; 0), Belarus (29; 20; 17; 2), Belgium (39; 6; 0; 6), Benin (39; 12; 0; 12), Bhutan (39; 0; 0; 0), Bolivia (39; 11; 2; 9), Botswana (39; 0; 0; 0), Brazil (39; 1; 0; 1), Bulgaria (39; 10; 2; 8), Burkina Faso (39; 2; 0; 2), Burundi (39; 12; 0; 1), Cambodia (26; 14; 11; 0), Cameroon (39; 8; 8; 0), Canada (49; 17; 0; 17), Cape Verde (32; 0; 0; 0), Central African Republic (39; 15; 8; 0), Chad (39; 2; 0; 2), Chile (39; 11; 1; 0), China (41; 32; 26; 1), Colombia (39; 25; 0; 25), Comoros (39; 0; 0; 0), Costa Rica (39; 19; 0; 19), Croatia (24; 12; 0; 12), Cuba (40; 40; 0; 40), Cyprus (39; 33; 0; 33), Czech Republic (29; 7; 0; 7), Denmark (39; 6; 0; 6), Djibouti (6; 0; 0; 0), Dominican Republic (39; 9; 0; 9), Ecuador (39; 6; 0; 6), Egypt (39; 13; 0; 6), El Salvador (39; 5; 0; 5), Equatorial Guinea (35; 9; 9; 0), Eritrea (19; 9; 0; 9), Estonia (24; 9; 0; 9), Ethiopia (38; 14; 0; 3), Fiji (39; 22; 21; 0), Finland (39; 6; 0; 6), France (49; 29; 0; 29), Gabon (39; 0; 0; 0), Gambia (39; 15; 12; 0), Georgia (29; 8; 0; 8), Germany (39; 6; 0; 6), Ghana (39; 2; 0; 2), Greece (39; 7; 0; 7), Guatemala (39; 31; 23; 9), Guinea (33; 17; 13; 0), Guinea-Bissau (39; 10; 10; 0), Guyana (39; 0; 0; 0), Haiti (39; 34; 8; 0), Honduras (39; 2; 1; 1), Hungary (28; 6; 0; 6), Iceland (39; 5; 0; 5), India (49; 38; 0; 38), Indonesia (39; 27; 18; 9), Iran (37; 35; 0; 3), Iraq (36; 36; 0; 35), Ireland (39; 22; 0; 22), Israel (24; 0; 0; 0), Italy (39; 6; 0; 6), Jamaica (39; 9; 0; 9), Japan (39; 0; 0; 0), Jordan (39; 4; 0; 4), Kazakhstan (29; 2; 0; 2), Kenya (39; 10; 4; 6), Kuwait (39; 2; 0; 2), Kyrgyzstan (29; 0; 0; 0), Laos (35; 13; 0; 13), Latvia (24; 9; 3; 6), Lebanon (31; 24; 0; 11), Lesotho (39; 5; 0; 5), Liberia (19; 16; 0; 3), Libya (29; 29; 0; 20), Lithuania (24; 7; 0; 7), Luxembourg (39; 6; 0; 6), Madagascar (39; 9; 0; 0), Malawi (39; 5; 2; 3), Malaysia (39; 0; 0; 0), Maldives (39; 0; 0; 0), Mali (39; 5; 3; 0), Malta (39; 6; 0; 6), Mauritania (39; 4; 3; 0), Mauritius (39; 0; 0; 0), Mexico (39; 0; 0; 0), Moldova (24; 17; 0; 17), Mongolia (38; 0; 0; 0), Montenegro (14; 5; 0; 5), Morocco (39; 0; 0; 0), Mozambique (28; 0; 0; 0), Myanmar (39; 32; 30; 0), Namibia (39; 0; 0; 0), Nepal (39; 1; 1; 0), Netherlands (39; 6; 0; 6), New Zealand (48; 0; 0; 0), Nicaragua (39; 16; 0; 14), Niger (39; 8; 5; 0), Nigeria (39; 24; 1; 10), North Macedonia (29; 0; 0; 0), Norway (39; 10; 0; 2), Oman (39; 0; 0; 0), Pakistan (39; 17; 2; 12), Panama (39; 12; 0; 9), Papua New Guinea (39; 0; 0; 0), Paraguay (39; 1; 1; 0), Peru (39; 7; 3; 4), Philippines (39; 18; 0; 18), Poland (29; 7; 0; 7), Portugal (39; 6; 0; 6), Qatar (39; 0; 0; 0), Romania (29; 12; 6; 6), Russia (30; 7; 0; 5), Rwanda (39; 17; 0; 0), Saudi Arabia (39; 2; 2; 0), Senegal (39; 0; 0; 0), Serbia (23; 19; 8; 2), Seychelles (39; 0; 0; 0), Sierra Leone (39; 17; 0; 15), Singapore (49; 0; 0; 0), Slovakia (27; 6; 0; 6), Slovenia (24; 6; 0; 6), Solomon Islands (39; 0; 0; 0), Somalia (17; 9; 3; 0), South Africa (39; 19; 0; 10), South Korea (39; 0; 0; 0), South Sudan (4; 4; 0; 0), Spain (39; 6; 0; 6), Sri Lanka (39; 2; 0; 2), Sudan (39; 31; 3; 0), Suriname (39; 7; 7; 0), Swaziland (39; 0; 0; 0), Sweden (39; 6; 0; 6), Switzerland (39; 0; 0; 0), Syria (39; 25; 0; 16), Tajikistan (29; 0; 0; 0), Tanzania (31; 5; 3; 2), Thailand (39; 11; 11; 0), Timor Leste (8; 0; 0; 0), Togo (39; 16; 16; 0), Tunisia (39; 9; 0; 9), Turkey (39; 8; 6; 1), Turkmenistan (29; 5; 0; 3), United Arab Emirates (39; 0; 0; 0), United Kingdom (39; 6; 0; 6), United States of America (49; 6; 0; 6), Ukraine (29; 12; 0; 11), Uruguay (39; 2; 2; 0), Uzbekistan (29; 10; 10; 0), Vanuatu (39; 0; 0; 0), Vietnam (34; 23; 0; 13), Yemen (29; 9; 0; 3), Zambia (39; 3; 3; 0), Zimbabwe (39; 23; 18; 5).

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*Notes:* If sanctions pursue aside from democratization or human rights improvements also other objectives (i.e., destabilizing the regime, policy change, preventing war, ending war, territorial conflict, counterterrorism, or other objectives), the corresponding observation is not counted as a particular sanction type to allow for a clear separation of the effects of sanctions by objectives. Therefore, the numbers of “democratization or human rights sanctions” and “non-democratization and non-human rights sanctions” do not necessarily add up to the number of “total sanctions.”

Table A2: Definitions of Variables and Data Sources

<b>Variable</b>	<b>Definition &amp; Source</b>
Regime Corruption Index	Measures the extent to which political actors use political office for private or political gain. Interval from 0 to 1, with higher scores indicating more corruption ( <i>v2xnp_regcorr</i> ). This index is related to but narrower than the political corruption index. <i>Source:</i> V-Dem Dataset, v15 (Sigman and Lindberg 2019; Pemstein et al. 2025; Coppedge et al. 2025)
Political Corruption Index	Measures how pervasive political corruption is. Covers executive, legislative, and judicial corruption, including different forms of corruption and corruption and different political levels. Interval from 0 to 1, with higher scores indicating more corruption ( <i>v2x_corr</i> ). This index is related to but broader than the regime corruption index. <i>Source:</i> V-Dem Dataset, v15 (McMann et al. 2016; Coppedge et al. 2025)
Executive Corruption Index	Measures how routinely members of the executive, or their agents grant favors in exchange for bribes, kickbacks, or other material inducements, and how often they steal, embezzle, or misappropriate public funds or other state resources. Interval from 0 to 1, with higher scores indicating more corruption ( <i>v2x_execorr</i> ). This index is part of the political corruption index and its constituent elements are part of the regime corruption index. <i>Source:</i> V-Dem Dataset, v15 (McMann et al. 2016; Coppedge et al. 2025).
Public Sector Corruption Index	Measures to what extent public sector employees grant favors in exchange for bribes, kickbacks, or other material inducements, and how often they steal, embezzle, or misappropriate public funds or other state resources. Interval from 0 to 1, with higher scores indicating more corruption ( <i>v2x_pubcorr</i> ). This index is part of the political corruption index. <i>Source:</i> V-Dem Dataset, v15 (McMann et al. 2016; Coppedge et al. 2025).
lag Sanctions	Binary indicators for country-years with sanctions in place, lagged by one year. <i>Source:</i> GSDB, v4 (Felbermayr et al. 2020; Kirikakha et al. 2021; Syropoulos et al. 2024).

Table A2: Definitions of Variables and Data Sources (continued)

lag $\log(\text{GDP pc})$	Natural logarithm of real GDP per capita in 2015 USD, lagged by one year. <i>Source:</i> World Development Indicators (World Bank 2025).
lag Resources Rents Share	Total natural resources rents as percentage of GDP by year, lagged by one year. Total natural resources rents are the sum of oil, natural gas, coal, mineral, and forest rents. <i>Source:</i> World Development Indicators (World Bank 2025).
lag Income Inequality	Pre-tax national income share held by the top 1 percent income group, lagged by one year. <i>Source:</i> World Income Database (2025).
lag Democracy	Binary variable indicating whether a country is democratic or not, lagged by one year. <i>Source:</i> Bjørnskov and Rode (2020), v5.2.
lag Human Rights	Latent human rights variable with higher values indicating a better protection of human rights, lagged by one year. <i>Source:</i> Human Rights Protection Scores, v4 (Fariss 2019).
lag Minor Conflict / lag Major Conflict	Armed conflicts resulting in 25 to 999 / at least 1,000 battle-related deaths in a given year, lagged by one year. <i>Source:</i> UCDP/PRIO Armed Conflict Dataset, v25.1 (Gleditsch et al. 2002; Davies et al. 2022).
lag Interstate Conflict	Conflicts between two states, lagged by one year. <i>Source:</i> UCDP/PRIO Armed Conflict Dataset, v25.1 (Gleditsch et al. 2002; Davies et al. 2022).
lag Intrastate Conflict w/ Intervention / w/o Intervention	Conflicts between a government and rebel groups with / without military intervention by foreign governments, lagged by one year. <i>Source:</i> UCDP/PRIO Armed Conflict Dataset, v25.1 (Gleditsch et al. 2002; Davies et al. 2022).

Table A3: Descriptive Statistics

	All Observations		No Sanctions		Sanctions	
	Mean	<i>N</i>	Mean	<i>N</i>	Mean	<i>N</i>
Corruption						
... Regime Corruption	0.49	5,843	0.46	4,312	0.56	1,531
... Political Corruption	0.49	5,843	0.46	4,312	0.56	1,531
... Executive Corruption	0.48	5,843	0.46	4,312	0.55	1,531
... Public Sector Corruption	0.47	5,843	0.44	4,312	0.55	1,531
lag <i>log</i> (GDP pc)	8.30	5,843	8.39	4,312	8.06	1,531
lag Resources Rents Share	0.07	5,843	0.07	4,312	0.08	1,531
lag Income Inequality	0.16	5,843	0.16	4,312	0.16	1,531
lag Human Rights	0.29	5,843	0.50	4,312	-0.32	1,531
	<i>X</i> = 1	<i>N</i>	<i>X</i> = 1	<i>N</i>	<i>X</i> = 1	<i>N</i>
lag Democracy	3,097	5,843	2,408	4,312	689	1,531
lag Minor Conflict	760	5,843	427	4,312	333	1,531
... Interstate	56	5,843	31	4,312	25	1,531
... Internal w/ Intervention	89	5,843	42	4,312	42	1,531
... Internal w/o Intervention	615	5,843	349	4,312	266	1,531
lag Major Conflict	248	5,843	105	4,312	143	1,531
... Interstate	23	5,843	9	4,312	14	1,531
... Internal w/ Intervention	67	5,843	13	4,312	54	1,531
... Internal w/o Intervention	158	5,843	83	4,312	75	1,531

*Notes:* Mean values and non-zero observations ( $X = 1$ ) for all LHS variables and control variables. Columns '*N*' show the number of observations in the full dataset, without sanctions in place, and with sanctions in place.

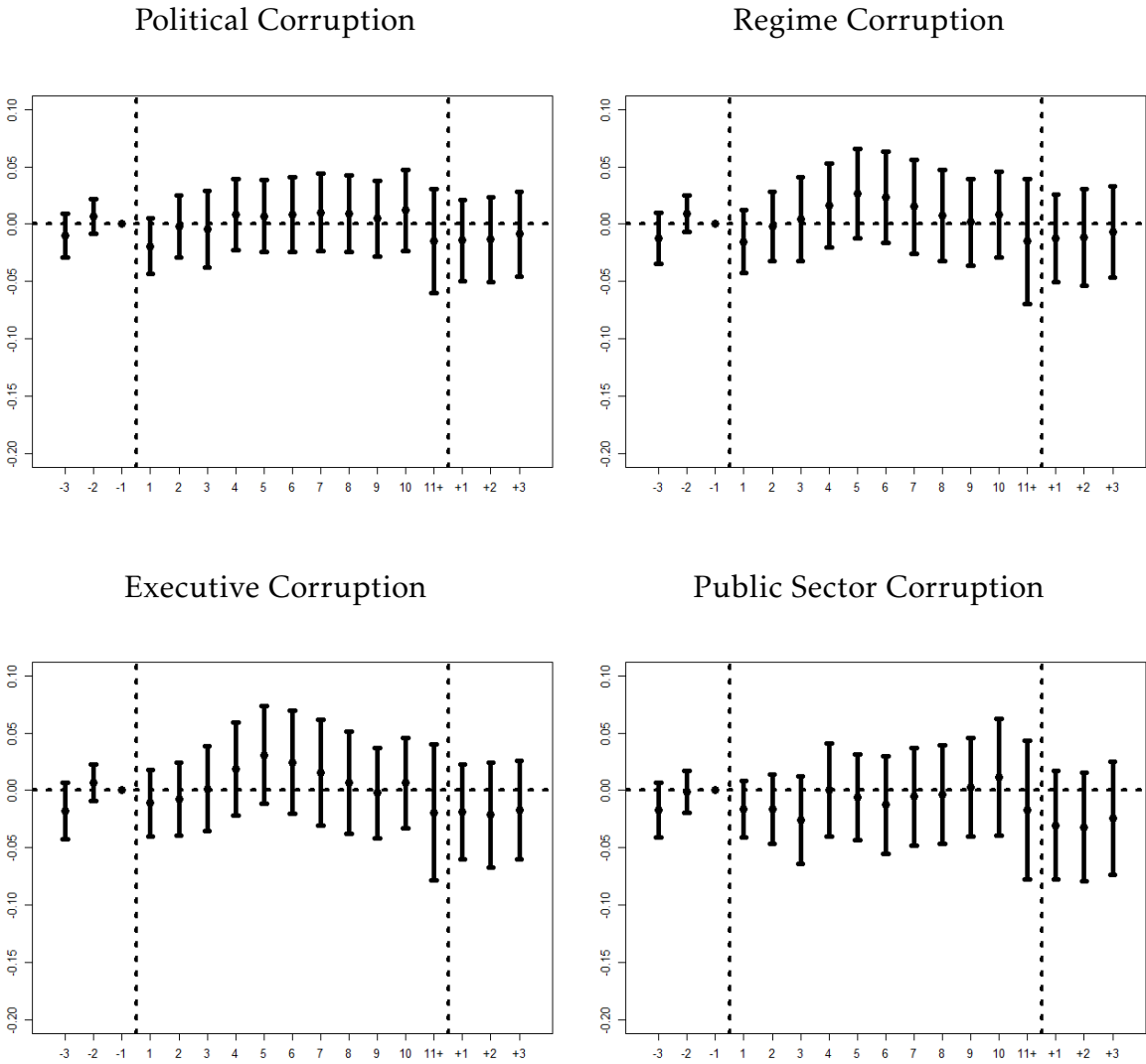
Table A4: Frequency of Sanctions

<b>Panel A: Panel DiD Estimations</b>		<b>Panel B: Event Study Approach</b>	
Sanctions in General		Western Sanctions	
... No Sanctions	4,312	... Pre-Trend -3 Years	104
... Sanctions	1,531 [358; 852]	... Pre-Trend -2 Years	109
		... Pre-Trend -1 Year	118
Sanction Senders			
... Western	1,046 [338; 524]	... Year 1	129
... UN	249 [11; 100]	... Year 2	112
... Non-Western	284 [3; 259]	... Year 3	88
		... Year 4	75
		... Year 5	70
Against Democracies		... Year 6	65
... Western	429 [81; 328]	... Year 7	57
... UN	43 [8; 20]	... Year 8	47
... Non-Western	252 [3; 239]	... Year 9	46
		... Year 10	34
Against Autocracies		... Year 11+	323
... Western	617 [257; 196]	... Post-Trend +1 Year	90
... UN	206 [3; 80]	... Post-Trend +2 Years	82
... Non-Western	32 [0; 20]	... Post-Trend +3 Years	77

*Notes:* Frequency of observations of the different sanction indicators for which all control variables (see Table A3) are available. Total number of observations in the dataset: 5,843. Sanctions enacted by the UNSC are not counted as Western or Non-Western sanctions. However, Western and Non-Western sanctions can coincide. Numbers in brackets represent the number of sanctions with the objective democratization or human rights improvements and the number of sanctions with other objectives (i.e., destabilizing the regime, policy change, preventing war, ending war, territorial conflict, counterterrorism, or other objectives), respectively. If sanctions pursue aside from democratization or human rights improvements also other objectives, the corresponding observation is not counted as a particular sanction type to allow for a clear separation of the effects of sanctions by objectives. Therefore, the numbers of “democratization or human rights sanctions” and “non-democratization and non-human rights sanctions” do not necessarily add up to the number of “total sanctions.”

# Additional Results

Figure A1: Effects of Western Sanctions against Autocracies on Corruption over Time



Notes: Effects of sanctions over time (1, 2, ..., 11+) alongside pre-trend (-3, -2, -1) and post-trend (+1, +2, +3) according to an estimation of Eq. (2) for different dependent variables. Standard errors are clustered at the country level. Models include control variables (other lagged sanction indicators, lag  $\log(\text{GDP pc})$ , lag Democracy, lag Human Rights, lag Resources Rents Share, lag Income Inequality, and six lagged conflict indicators), country fixed effects, and year fixed effects. 95% confidence bands are indicated by whiskers.

Table A5: Results Controlling for Persistence in Corruption

	(1) <b>Regime Corruption</b>	(2) <b>Political Corruption</b>	(3) <b>Executive Corruption</b>	(4) <b>Public Sector Corruption</b>
lag Dependent Variable	0.9063*** (0.0103)	0.9150*** (0.0090)	0.8994*** (0.0108)	0.9070*** (0.0093)
Sanctions				
... lag Western ag. Democracies	-0.0102** (0.0042)	-0.0053* (0.0031)	-0.0135*** (0.0050)	-0.0082** (0.0038)
... lag Western ag. Autocracies	0.0019 (0.0030)	0.0008 (0.0024)	0.0031 (0.0033)	-0.0015 (0.0023)
... lag UN against Democracies	-0.0151* (0.0091)	-0.0082 (0.0078)	-0.0197** (0.0100)	-0.0129* (0.0071)
... lag UN against Autocracies	0.0031 (0.0041)	0.0048 (0.0035)	0.0001 (0.0045)	0.0006 (0.0041)
... lag Non-Western ag. Democ.	0.0040 (0.0035)	0.0038 (0.0025)	0.0050 (0.0039)	0.0055** (0.0025)
... lag Non-Western ag. Autoc.	0.0094 (0.0155)	0.0067 (0.0092)	0.0108 (0.0179)	0.0087* (0.0050)
Observations	5,839	5,839	5,839	5,839
R <sup>2</sup>	0.9849	0.9898	0.9794	0.9842
Within-R <sup>2</sup>	0.8410	0.8614	0.8247	0.8449

*Notes:* Coefficient estimates of Eq. (1), including a lagged dependent variable. Standard errors in parentheses are clustered at the country level. \*\*\*/\*\*/\* indicates significance at the 1%/5%/10% level. Models include control variables (lag  $\log(\text{GDP pc})$ , lag Democracy, lag Human Rights, lag Resources Rents Share, lag Income Inequality, and six lagged conflict indicators), country, and year fixed effects. Estimates for control variables are available on request.

Table A6: Two-Stage Difference-in-Differences Results

	(1) <b>Regime Corruption</b>	(2) <b>Political Corruption</b>	(3) <b>Executive Corruption</b>	(4) <b>Public Sector Corruption</b>
Sanctions				
... lag Western ag. Democracies	-0.0664*** (0.0199)	-0.0602*** (0.0164)	-0.0662*** (0.0226)	-0.0532*** (0.0182)
... lag Western ag. Autocracies	0.0089 (0.0183)	0.0118 (0.0147)	0.0104 (0.0201)	0.0238 (0.0184)
... lag UN against Democracies	-0.1050*** (0.0350)	-0.0782* (0.0403)	-0.1207*** (0.0307)	-0.0893** (0.0347)
... lag UN against Autocracies	0.0409 (0.0547)	0.0682 (0.0445)	0.0204 (0.0625)	0.0744 (0.0567)
... lag Non-Western ag. Democ.	0.0123 (0.0194)	0.0030 (0.0172)	0.0122 (0.0201)	-0.0143 (0.0175)
... lag Non-Western ag. Autoc.	0.0706 (0.0811)	0.0252 (0.0519)	0.0923 (0.0823)	0.0012 (0.0343)
Observations	5,717	5,717	5,717	5,717
R <sup>2</sup>	0.0459	0.0565	0.0392	0.0434

*Notes:* Coefficient estimates of the Two-Stage DiD approach by Gardner et al. (2025) and Butts and Gardner (2022). The first stage includes all control variables as well as country and year fixed effects. Standard errors in parentheses are clustered at the country level. \*\*\*/\*\*/\* indicates significance at the 1%/5%/10% level.