Research Article*

Re-examining the Effects of Western Sanctions on Democracy and Human Rights in the 21st Century

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Abstract

Whether and how severely imposed economic sanctions affect ordinary citizens in targeted countries is a central question of International Relations (IR) research on sanctions. Although frequently intended to bring about improvements to democracy and human rights, their record of doing so has historically been mixed at best. However, many of the canonical quantitative studies on this critical issue only cover the 1980s and 1990s. Since then, sanctions practice has undergone major innovations in design and implementation following debates on unintended consequences and humanitarian harm. Given these significant changes towards 'targeted' or 'smart' sanctions, it stands to reason that sanctions may today be achieving their intended purposes. Furthermore, more current and precise data on sanctions, human rights, and democracy has since been published and new methods for causal inference using observational data have been developed. I take up these policy-related and methodological innovations to re-examine the effects of Western sanctions explicitly intended to improve democracy and human rights. I study this type of sanctions in the 1990–2021 timeframe by applying matching and weighting to account for the strategic selection process of sanctions imposition. This builds on work examining coups and fraudulent elections as key 'trigger events.' I then estimate the effects of such sanctions on human rights and democracy using difference-in-differences analysis. The results suggest that the negative effects of sanctions on human rights and democracy persist in the 21st century. This provides an important update to the empirical literature on the subject. As sanctions continue to grow in popularity and usage, policymakers must continue devoting attention to minimizing the civilian harm they often cause. Beyond these substantive contribution, this paper presents a template for replicating and extending country-year research in IR in general.

Keywords—Economic sanctions, democracy, human rights, coups, replication.

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Contents

1 Introduction

Economic sanctions are an increasingly popular foreign policy tool of Western states. As a self-admitted "tool of first resort" for the US (US Department of the Treasury 2021: 1; Drezner 2022: 1534), they are used in pursuit of policy aims ranging from curbing the proliferation of nuclear weapons and ending large-scale interstate war to fighting drug trafficking and punishing corruption. A further common aim of Western sanctions is the safeguarding and promotion of democracy and human rights abroad. Authoritarianism and democratic backsliding are on the rise globally: Since the early 2010s, the slow but steady global post-Cold War improvements in liberal and electoral democracy have reversed (Coppedge et al. 2024).[1] Meanwhile, human rights are undergoing a related phase of relative stasis and regression (Fariss 2014; Fariss, Kenwick, and Reuning 2020).

Despite an overall "decline in more overt forms of democracy promotion" (Hyde 2020: 1193), economic sanctions remain one of a small number of coercive tools immediately available to Western policymakers in the face of coups, electoral fraud, or egregious human rights violations. From the early 1990s onwards, an increasing share of Western sanctions has nominally aimed to improve democracy and human rights in the sanctioned country, with a renewed increase in the late 2000s (Felbermayr et al. 2020: 10–11, 21). This study examines this type of democracy- and human rights-related sanctions imposition, sometimes called "democratic sanctions" (Cox and Drury 2006; Soest and Wahman 2015b). Appendix A.1 illustrates these trends in declining democracy and human rights and the increasing imposition of "democratic sanctions."

Much of the canonical sanctions literature has found that economic sanctions negatively affect democracy and human rights outcomes in the targeted country, including sanctions explicitly intended to *improve* democracy and human rights (e.g., Peksen and Drury 2010; Dizaji and Bergeijk 2013; Wallace 2013). Increases in repression are argued to be caused by diminished fiscal capacity leading to less oversight of the security apparatus, poorer government services, and increased corruption. Furthermore, sanctions may cause economic conditions to deteriorate, leading to protest, which in turn may be brutally

[1] However, see Little and Meng (2024) for a more skeptical view.

repressed (Grauvogel, Licht, and Soest 2017; Liou, Murdie, and Peksen 2020). However, this literature largely examines the 1980s and 1990s.

Meanwhile, sanctions policy has undergone significant changes since the 1990s, with a major shift from indiscriminate, blanket embargoes to sanctions that target specific sectors of the economy and specific individuals within the target regime (see, e.g., Drezner 2011, 2018). Given these major policy innovations, there are reasons to expect a reduction in the negative effects of sanctions, including civilian suffering and repression. The present article therefore re-examines the important question of whether Western-supported democracy- and human rights-related sanctions – "democratic sanctions" (Cox and Drury 2006) – now facilitate the intended improvements in the targeted country, or whether they continue to negatively affect the outcomes they aim to improve into the 21st century. As sanctions gain even further popularity and prominence, this is a highly policy-relevant question.

Major changes in sanctions policy, novel data, and methodological innovations in international relations (IR) and political science make a close re-examination of this question worthwhile. This article uses new datasets for all major variables, taking up recent conceptual improvements and significantly extending past country-year sanctions research temporally. I apply new causal inference tools for panel data as a way to evaluate policy, to estimate longer-run effects, and to overcome the main observable biases on treatment selection with respect to sanctions imposition. In doing so, I also present a general framework for deliberately replicating and extending country-year research in IR.

The results show that the negative effects of sanctions on democracy persist in the late 20th and early 21st century, from 1990–2021. Results for human rights outcomes are mixed, but also show negative trends. These results are robust to various specifications. This suggests that despite greater sophistication, the move towards 'targeted' Western sanctions has – on the whole – likely not had the desired effects of minimizing civilian harm. Even if we consider sanctions as a "least bad choice" in times of crisis abroad (Peksen 2019a), Western decisionmakers do not seem to have solved the major problem of humanitarian fallout. Imposing targeted sanctions to support democracy and human rights abroad remains ethically fraught (Early and Schulzke 2018). Policymakers should

therefore devote further attention to sanctions design, implementation, and enforcement and consider alternative policy tools under a principle of 'do no harm.'

The article proceeds as follows. Section 2 summarizes the main findings of past sanctions research and lays out how this research will be extended and updated given new data and tools. Section 3 defines and discusses the main treatment and outcome variables. Section 4 then re-examines the treatment assignment mechanism of democracy- and human rights-related sanctions, i.e., who get sanctioned. On this basis, Section 5 presents the study's main analysis and results, estimating whether sanctions intended to improve democracy and human rights in fact do so. Finally, Section 6 discusses the implications of the analysis.

2 Sanctions and sanctions research in the 21st century

The main contributions of this article are empirical. This largely follows the approach of "holding theory constant" while focusing on the "difficult business" of applying insights from causal inference frameworks and methods (Samii 2016: 950). "New theories are necessary only when sufficient evidence demands them" (Besbris and Khan 2017: 148), and the effects of sanctions on democracy and human rights can be usefully examined using the extant theoretical and conceptual frameworks applied here.[2]

This section summarizes the key results of sanctions research on this question (2.1), reviews changes to sanctions policy since the 1990s (2.2), discusses novel IR datasets on sanctions and other primary variables (2.3), presents new tools for causal inference using country-year IR data (2.4), and finally lays out the study's framework for replicating and extending past research (2.5). It concludes by arguing that taken together, these elements are the basis for empirically reconsidering a key question for Western policymakers – whether democracy- and human rights-related sanctions do in fact improve these outcomes in the short and medium term.

[2]That said, such theoretical work should be pursued. Drezner (2018: 252) points out a lack of pure theory in sanctions research.

2.1 The effect of sanctions on democracy and human rights

Most studies show a worsening of democracy and human rights in sanctioned countries, while a small handful show improvements. Appendix A.2 summarizes canonical quantitative studies on the domestic consequences of economic sanctions and plots the timeframes they examine. Despite a large breadth of studies, the majority of research – and therefore much of the conventional wisdom on the subject – draws from the Cold War era and 20th century. This suggests that studies on sanctions focusing on years after 1990 and especially from about 2005 onwards will be instructive for probing contemporary sanctions practice and effectiveness, and whether the move towards targeted, more sophisticated sanctions has delivered on its intentions.

Based on these mixed results of past research, this article examines two sets of competing hypotheses, one on democracy and the other on human rights. We might expect modern-day sanctions practice to continue to negatively affect these outcomes (Early and Peksen 2022: 4–5), or that the major policy shifts surrounding targeted or "smart" sanctions since the 1990s have had the desired effect of minimizing negative fallout.

> **H1a:** Democracy- and human rights-related sanctions imposition causes a deterioration of democracy.
>
> **H1b:** Democracy- and human rights-related sanctions imposition causes an improvement of democracy.
>
> **H2a:** Democracy- and human rights-related sanctions imposition causes a deterioration of human rights.
>
> **H2b:** Democracy- and human rights-related sanctions imposition causes an improvement of human rights.

2.2 Changes to sanctions policy and world politics

The drastic humanitarian consequences of the comprehensive UN trade embargo on Iraq under Saddam Hussein in the 1990s prompted UN, US, and EU decisionmakers to shift their sanctions policy from broad blockades to sanctions targeted at political elites (e.g.,

asset freezes) or particular economic sectors (e.g., oil) (Biersteker, Eckert, and Tourinho 2016: 25–27; Early and Schulzke 2018: 61–62).[3] Giumelli dates this change at the UN to the mid-1990s (2015: 1352), Hawkins and Lloyd find that the normative shift had gained "substantial support" by the early 2000s (2003: 441, 451–452), and Drezner summarizes that the UN and the US had fully "internalized" the notion of targeted sanctions by 2010 (2011: 99–101). Thus, if we roughly date the shift towards targeted sanctions to around 2005, the late 2000s, 2010s, and 2020s are critical for assessing the effectiveness of contemporary sanctions practice. Consequently, if these changes were successful, we would expect to see a decrease in negative domestic consequences around the mid-2000s.

Panel A in Figure 1 illustrates the increasing precision of UN, EU, and US economic sanctions since the 1990s (Attia and Grauvogel 2023). Panel B shows an increase in post-Cold War sanctioning, a slight decrease around the late 1990s reconsideration towards targeted sanctions, and a slow increase that has taken up speed since 2015 (Felbermayr et al. 2020; Attia and Grauvogel 2023; Drezner 2022). While only around 25% of sanctioned countries in the early to mid-1990s were subject to asset freezes and travel bans against political elites – two particularly prominent types of targeted sanctions – this has risen to about 60% in 2020s. Conversely, one quarter of all sanctioned countries were sanctioned comprehensively in the early 1990s, but this has halved to 12%, and these are not military blockades but rather highly restrictive unilateral sanctions regimes.[4] The figure also shows the mid-2000s as the starting point for the growth of targeted sanctions more generally.

Alongside these changes to sanctions policy, broader shifts in world politics also make a re-examination of past findings on economic sanctions worthwhile. For instance, the US and the EU have increasingly "weaponized interdependence," using their central positions in global financial, technological, and informational networks for strategic gains and to pressure other states (Farrell and Newman 2019). This has changed the coercive capacity and reach of Western sanctions in recent decades, potentially also affecting their consequences in sanctioned states.

[3] In line with Biersteker et al., 'targeted sanctions' are here defined as all sanctions that are not (quasi-) embargoes; Biersteker, Tourinho, and Eckert (2016): 27; Early and Schulzke (2018): 61.

[4] In 2021, the targets of these stringent measures were North Korea (US), Iran (US), Cuba (US), Syria (US), Sudan (US), and the Russian-occupied areas of Ukraine (US, EU). Refer to the IST dataset for more detailed descriptions of the nature of these measures; Attia and Grauvogel (2023).

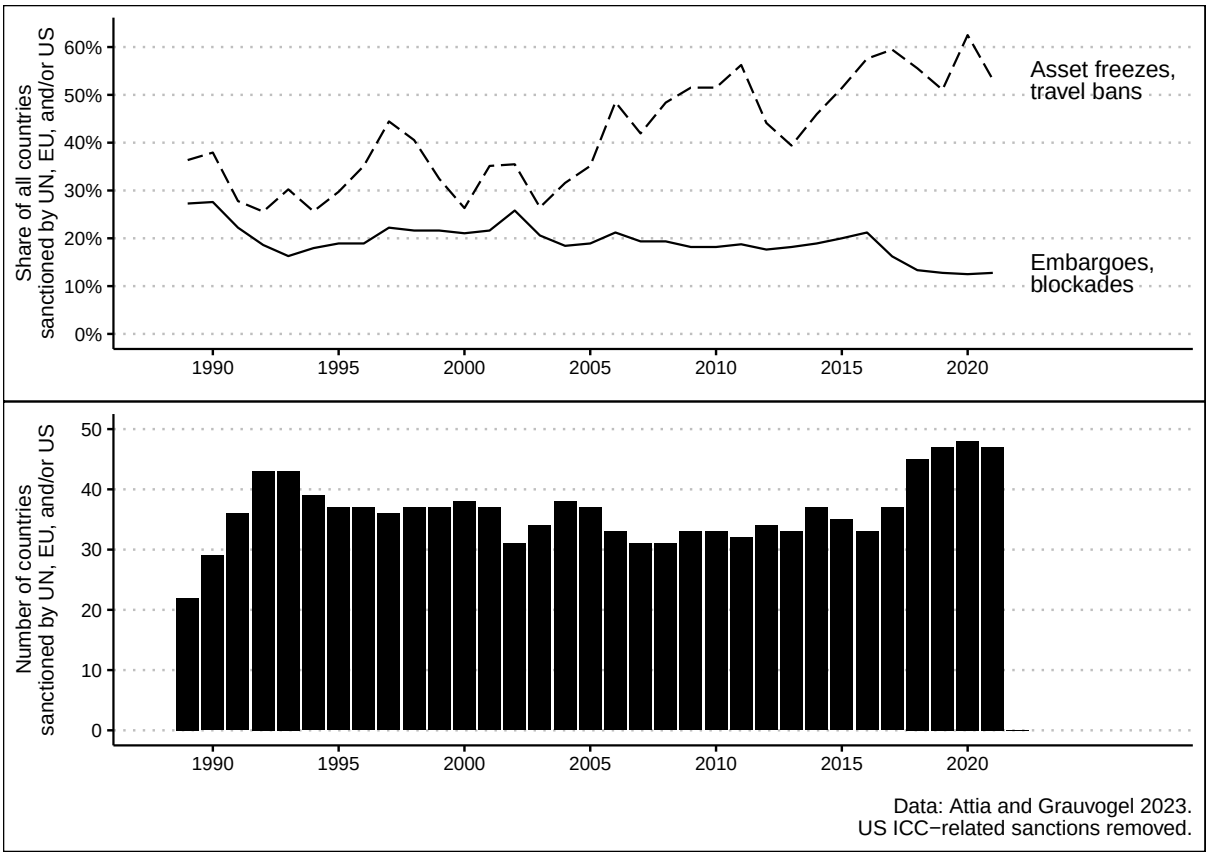

Figure 1: Comprehensive sanctions and targeted financial sanctions, 1989–2021.

2.3 New data on sanctions, democracy, and human rights

Many of the canonical quantitative studies on sanctions cover the later Cold War era through 2005, often using the influential TIES dataset (T. C. Morgan, Bapat, and Kobayashi 2014, see Appendix A.2). Only recently have datasets with more recent coverage been published: EUSANCT (1989–2015, Weber and Schneider 2022), IST (1990–2018/2021, Attia and Grauvogel 2023), and the GSDB (1950–2019, Felbermayr et al. 2020). The former covers sanctions threats and impositions, the latter two only cover imposed sanctions. This article uses IST for its main analyses and EUSANCT for supplementary analyses and illustrations. IST has good temporal coverage (Version 2.0; 1990–2021) and focuses on key sanctions senders and aims we are particularly interested in – the UN, US, and EU's support of democracy and human rights abroad.

Significant data improvements have also been made in the study of democracy and human rights through human-coded datasets (Weidmann 2024: 923–924). While the classic

studies on sanctions largely use Polity IV or Freedom House data to measure democracy and autocracy Freedom House (2022), V-Dem now provides more detailed and granular measures (Coppedge et al. 2024; Boese 2019). Similarly, sanctions studies most commonly used CIRI or PTS human rights data (Cingranelli and Richards 2010; Wood and Gibney 2010), which can now be compared to Human Rights Scores (HRS) as a "theoretically informed measurement" with greater coverage (Fariss 2014).

2.4 New tools for causal inference using country-year data in IR

The imposition of economic sanctions is a clear and easily identifiable policy intervention. Therefore, it can be usefully examined with causal inference methods designed for policy evaluation (Athey and Imbens 2017). Given the country-year data at hand, matching and weighting, combined with difference-in-differences estimation, are a useful, applicable methodological approach that will be implemented via `PanelMatch` in this article (Imai, Kim, and Wang 2023). This 'selection on observables' identification strategy draws upon past work on the treatment assignment mechanism of economic sanctions, i.e., the factors determining sanctions imposition (Soest and Wahman 2015a; Licht 2017). Although a recognized challenge, potential endogeneity and confounding are often not explicitly tackled in sanctions research.[5] For the case of sanctions research, matching has attractive properties compared to country-year regression because it explicitly focuses on and makes claims about the Average Treatment Effect on the Treated (ATT); the effect of sanctions on countries that typically get sanctioned (see Samii 2016). That said, selection on observables remains a weaker identification strategy than designs building upon random or quasi-random treatment assignment (Keele 2015; Doleac 2019). Within the predominant country-year framework for sanctions research, such designs remain largely out of reach (see also Demena et al. 2021). Nonetheless, matching and weighting of this sort are useful alternatives to the prevailing methodological approaches. This study therefore presents a

[5]Gutmann, Neuenkirch, and Neumeier (2020): 160 and Licht (2017): 162 make this argument, Gutmann, Neuenkirch, and Neumeier (2021): 140 list several examples.

faithful application of Imai, Kim, and Wang (2023) to the question of economic sanctions aiming to improve democracy and human rights.

2.5 Framework: replication, extension

The causal effects of sanctions on domestic conditions in the sanctioned country is a highly policy relevant research question, and comparatively straightforward conceptually (see also Demena et al. 2021). Nonetheless, there are many plausible ways of answering the same empirical research question, even when using identical data (Silberzahn and Uhlmann 2015; Auspurg and Brüderl 2021; Breznau et al. 2022). Considering the "researcher degrees of freedom" and the "garden of forking paths" inherent in social inquiry (Gelman and Loken 2013), this article closely builds on previous work on the subject. It uses and extends one article in particular, von Soest and Wahman's "Not all dictators are equal: Coups, fraudulent elections, and the selective targeting of democratic sanctions" (2015a). As this article examines the 'treatment assignment mechanism' for democracy- and human rights-related sanctions, it is a well-suited building block for estimating their subsequent causal effects.[6]

3 Definitions: Outcome, treatment, and estimand

This section summarizes the study's key concepts and variables. Appendix A.3 presents summary statistics for these and all other variables outlined in the following and used in the analysis.

3.1 Outcomes: democracy and human rights measures

This study's main substantive research interest is whether Western sanctions cause a deterioration or improvement of human rights and democracy in the countries they target. As covered in Section 1, EU and US sanctions increasingly contain human rights and democracy components and often aim to improve these conditions. Both concepts are

[6] See also Licht (2017) for a similar approach.

very broad in their most general uses, so the analysis zooms in on one particular salient dimension for each of the two.

The state of *democracy* is here defined with a narrow focus on the electoral dimension, understood as a "minimalist" definition (Coppedge et al. 2020: 42–44; Przeworski 1991; Dahl 1998; Kurki 2010). Electoral democracy reflects "the core value of making rulers responsive to citizens, achieved through electoral competition" under fair conditions, free civil society participation, and sound electoral organization (Coppedge et al. 2020: 42). Democracy-related sanctions are often threatened and imposed following anti-democratic coups to return to constitutional order or after fraudulent elections to bring about fairly-run ones. An emphasis on this electoral dimension examines the shorter-term changes and improvements sanctions senders often hope to achieve, as opposed to slower-moving societal aspects of democracy such as egalitarianism and liberal values (Coppedge et al. 2020: 45; Lindberg et al. 2014: 158, 160–161). This paper uses V-Dem's measure of electoral democracy ('polyarchy'). Although it represents a significant innovation in the measurement of democracy and in IR country-year data, V-Dem has not been widely used in sanctions research.[7]

Human rights are "those fundamental moral rights of the person that are necessary for a life with human dignity" (Forsythe 2017: 3). The state of human rights practices is here more narrowly defined as the degree to which government agents violate or uphold the physical integrity rights of their population. This covers the absence or presence of extreme repressive practices such as arbitrary imprisonment, torture, and execution (Fariss 2014: 297; see also Poe and Tate 1994; Richards, Webb, and Clay 2015). This definition emphasizes the violation or safeguarding of immediate physical safety within a country, reflecting the conditions that human rights-related sanctions most commonly intend to improve. Narrow definitions of this sort focusing on physical integrity rights reflected in the International Covenant on Civil and Political Rights (ICCPR) are commonly used in cross-sectional work on human rights conditions (Cope, Crabtree, and Lupu 2018; Cordell et al. 2022: 6). It is usually argued that physical integrity rights are the foundation of all

[7]See Appendix A.2; for an exception, see Hellmeier (2021).

other human rights, and respect for other human rights is therefore expected to closely correlate with respect for physical integrity rights.[8]

3.2 Treatment: democracy- and human rights-related sanctions imposition

The treatment is the imposition of democracy- or human rights-related sanctions, more widely called "democratic sanctions" (Cox and Drury 2006; Soest and Wahman 2015a: 18; Soest and Wahman 2015b). The relevant literature often bundles these two objectives of sanctions because they are conceptually related and often imposed simultaneously. Appendix A.4 lays out how these sanctions goals coincide.[9]

The precise binary treatment measure used here is whether a given country was sanctioned by either the UN, US, or the EU in a given year. Among sanctions datasets, the IST dataset (International Sanctions Termination) is best suited for studying the research questions of this paper (Attia and Grauvogel 2023): It covers sanctions from 1990–2021 and provides detailed information on sanctions objectives. As we are interested Western sanctions policy surrounding democracy and human rights, the sender coverage is also appropriate (see also Portela and Charron 2022). This simplification of sanctions imposition as a binary variable – when sanctions in fact vary in intensity and 'treatment dosage' – should be revisited in future work.[10]

This study focuses on Western sanctions, as is often done in sanctions research (e.g., Soest and Wahman 2015a; Pospieszna and Weber 2020). Western states are the most active sanctioners, data coverage for Western senders is plausibly complete,[11] and sanctions goals and mechanisms are fairly similar across Western states, making generalizations justifiable. Studies of this type typically only cover sanctions threatened and imposed by the US

[8] See Cope, Crabtree, and Lupu (2018) for a discussion.

[9] Among 870 country-years facing either type of sanctions objective from 1990–2021, 62.2% faced both (542 of 870).

[10] `PanelMatch` currently does not offer an analysis of varying treatment dosages, i.e., a continuous treatment, but this is an ongoing debate in the difference-in-differences literature; see Callaway, Goodman-Bacon, and Sant'Anna (2024).

[11] The main sanctions datasets are largely sourced from news archives, which most reliably cover English-language and Western sources. Assumedly, Russian-, Chinese-, or Arabic-language sources are far less completely covered in these datasets.

and/or the EU/EEC (see Appendix A.2). In contrast, this study adds UN sanctions to the analysis. The common exclusion of UN sanctions from 'Western' santions overlooks that UN sanctions require the support of three key Western states as members of the Security Council: the US, UK, and France (the "P3"). Indeed, given the considerable normative power of UNSC resolutions and these sanctions' binding nature in accordance with Article 41 of the UN Charter, the P3 often strongly push for multilateral UN measures before also imposing their own. Therefore, for the purposes of many sanctions-related research questions, I argue that UN sanctions are by definition "Western-supported" sanctions, and should therefore be included in these types of analyses.[12]

Given this definition, Appendix A.6 examines treatment variation from 1990–2021. Overall, 'democratic sanctions' onset is somewhat rare: The timeframe contains 86 cases of onset, or about 3 instances per year on average. 68 of 181 country units (38%) had democracy- and human rights-related sanctions imposed on them at some point in the examined timeframe. There are no strong or apparent temporal clusters. Therefore, there is enough variation in treatment across time and units, strengthening the internal and external validity of the causal effects estimated in the analysis (Imai, Kim, and Wang 2023: 591–592).

Sanctions *threats* are often overlooked in the sanctions literature, although the implicit or explicit threat of economic sanctions itself already significantly affects government behavior (Drezner 2003). However, this concern is less relevant for the issue at hand, as explicit threats of democracy- and human rights-related sanctions only rarely result in target acquiescence. This is likely because concessions on democracy represent a major risk to an autocrat's hold on power (Soest and Wahman 2015b: 961–962). Appendix A.5 illustrates this for the 1989–2015 timeframe.[13] Therefore, this paper focuses on imposed sanctions only.

[12]The results presented in the following hold regardless of whether UN sanctions are included or not; democracy and human rights are rarely the main goals of UN sanctions, Biersteker, Tourinho, and Eckert (2016): 24.

[13]Only 11.7% of such threats were effective from 1989–2015, compared to 45% of sanctions threats in pursuit of other objectives.

3.3 Estimand: ATT

The estimand is the average treatment effect on the treated (ATT); the average effect of democracy- and human rights-related sanctions onset on democracy and human rights in sanctioned countries.

The assumption for estimating the ATT is $Y^0 \perp\!\!\!\perp D|X$ – i.e., that the distribution of outcomes for untreated units is the same for both (matched and weighted) control units and (counterfactually) treated units. For the case of sanctions, this assumption means that, once appropriately matched and weighted, human rights and democracy would on average have developed similarly in sanctioned countries as they did in unsanctioned countries if the former had not been sanctioned. In other words, in the absence of sanctions, the trajectories of sanctioned countries would have developed in parallel to those of unsanctioned countries similar to them on key observable characteristics. This latter group's observed outcomes are thus used to interpolate the former's potential outcomes (see Cunningham 2023; S. L. Morgan and Winship 2015: 37–76; Blackwell and Glynn 2018: 1068–1069). Following Imai, Kim, and Wang (`PanelMatch`), the ATT is estimated as follows (2023: 592–593), seperately for each of the two outcomes of interest – democracy and human rights:

$$\begin{aligned}\delta(F, L) = \mathbb{E}[Y_{i,t+F}(X_{it} = 1, X_{i,t-1} = 0, [X_{i,t-\ell}]_{\ell=2}^{L}) - \\ Y_{i,t+F}(X_{it} = 0, X_{i,t-1} = 0, [X_{i,t-\ell}]_{\ell=2}^{L}) | X_{it} = 1, X_{i,t-1} = 0]\end{aligned} \tag{1}$$

The estimated ATT is the observed difference δ in outcomes between treated and untreated units $X_{it} = 1$ and $X_{it} = 0$, after both had previously been untreated $X_{i,t-1} = 0$. The ATT can be estimated for different specified timeframes. F and L represent leads and lags, respectively, i.e., the timeframe after treatment for which the ATT is estimated (*t+0*, *t+1*, *t+2*...) and the past timeframes based on which matches are created (...*t–2*, *t–1*). In Equation (1), the potential outcome of treated units is shown in line one and that of untreated units is shown in the first part of line two (Imai, Kim, and Wang 2023: 593). This follows common potential outcomes notation (S. L. Morgan and Winship 2015).

This framework can be readily applied to study and re-examine the effects of economic sanctions on democracy and human rights in targeted countries.[14]

4 Treatment assignment: Who sanctions whom, and when?

Economic sanctions are imposed strategically, and the logic behind the approach used in the following is the creation of treatment and control groups of countries that face similar *ex ante* risks of sanctions imposition and have similar trends in electoral democracy and human rights (Licht 2017: 160). This article builds on past work by von Soest and Wahman (2015a) examining dramatic events such as coups and fraudulent elections as key triggers for Western sanctions imposition. Adopting a potential outcomes approach, these types of trigger events are an important part of the treatment assignment mechanism.[15] However, this critical role and the predictive capacity of trigger events for sanctions imposition has been overlooked in sanctions scholarship using matching techniques (e.g., Neuenkirch and Neumeier 2016; Gutmann, Neuenkirch, and Neumeier 2020; Early and Peksen 2022). Besides trigger events and in line with past work (Soest and Wahman 2015a), this section also examines target vulnerability and sender–target relations as likely determinants of sanctions imposition by the UN, EU, and US. The following subsections lay out these and other confounders that will be used in the subsequent analysis. Appendix A.3 summarizes and shows descriptive statistics for all variables discussed in the following.

4.1 Trigger events

A central insight of previous work is the importance of dramatic domestic incidents in triggering Western sanctions imposition (Soest and Wahman 2015a).

Coups – Successful coups often engender violence and disorder. Anti-democratic coups

[14] Likewise using country-year data, Imai, Kim, and Wang (2023) use it to re-examine country-year studies on (1) the effects of democratization on economic growth (Acemoglu et al. 2019) and (2) whether war mobilization increases inheritance tax rates (Scheve and Stasavage 2012).

[15] Other plausible trigger events might be unconstitutional extensions of term limits, violent crackdowns on peaceful protests, or the detention of prominent and internationally visible dissidents.

in particular are frequently met with international condemnation, including sanctions imposition. Coup data is drawn from the recent update of the Colpus dataset (Chin, Carter, and Wright 2021; Chin and Kirkpatrick 2023).

Controversial elections – Sanctions are also frequently imposed when Western monitors raise fraud allegations, are denied participation, or refuse to monitor an election in the first place (see Soest and Wahman 2015a: 23). This data is taken from V-Dem and NELDA (Hyde and Marinov 2012; Coppedge et al. 2024).

These two critical 'trigger events' variables are generally not lagged in the relevant literature because empirically they almost always precede sanctions imposition, ruling out reverse causality (Soest and Wahman 2015a: 23, fn8).

4.2 Target vulnerability and sender–target relations

Beyond immediate, drastic trigger events, the target's potential vulnerability to external coercion and its extant links to Western states also affect the likelihood of Western sanctions imposition (Soest and Wahman 2015a).

Target vulnerability and instability – Pro-democracy protests may be interpreted by Western senders as target vulnerability, and may encourage them to impose targeted sanctions as a sign of support to protesters (Grauvogel, Licht, and Soest 2017). This information is drawn from V-Dem's indicators for civil soviety repression and pro-democracy protest (Coppedge et al. 2024). Meanwhile, economic weakness – e.g., stagnating growth or rising inflation – may also imply a vulnerable target and encourage sanctions imposition. This study uses World Bank data on GDP, GDP per capita, economic growth, and inflation (Arel-Bundock 2022).

Intrastate conflict – An increase in intrastate conflict may also make the imposition of human rights-related sanctions and arms embargoes more likely, while also causing decreases in our outcomes of interest. This is measured by whether there is an increase in the number of ongoing intrastate conflicts in a given year or not (UCDP/PRIO 2024).

Neo-patrimonialism – The turn towards 'targeted' sanctions took up the insight that

certain regime types may be more pliable with sanctions than others – namely, personalist systems highly dependent on 'buying off' domestic elites and rivals. Neo-patrimonial states offer clear openings for targeted sanctions and personal asset freezes (Escribà-Folch and Wright 2010; Peksen 2019b), and sanctions against such states may be more likely than against others. This is operationalized with V-Dem's 'neo-patrimonialism' variable (Coppedge et al. 2024).

Political proximity – Finally, UN General Assembly voting proximity to the US and EU (Bailey, Strezhnev, and Voeten 2017) also affects Western sanctions imposition (Soest and Wahman 2015a). Potential sanctions senders consider proximity and vulnerability strategically: Sanctions are most likely against allied states when those allies are stable, or against adversaries when those adversaries are unstable. Conversely, sanctions are less likely against politically unstable allies and against stable adversaries (McLean and Radtke 2018).

4.3 Predicting 'democratic sanctions' imposition

Von Soest and Wahman's original results could be successfully reproduced, i.e. computationally duplicated, moving from their original `Stata` code to `R`. The results also hold when replicated for the full 1990–2021 timeframe with the newer datasets summarized above. Appendix A.8 presents these replications.[16] While coups are quite regularly met with sanctions imposition, fraudulent elections are today sanctioned more rarely than they once were. Nonetheless, both remain an important predictor of sanctions onset. Appendix A.7 descriptively visualizes coups and fraudulent elections as triggers for the imposition of democracy- and human rights-related sanctions for the 1990–2021 timeframe.

Based on von Soest and Wahman's original approach and taking up the outcome and treatment variables outlined in Sections 3.1 and 3.2 and the confounders in Sections 4.1 and 4.2, Table 1 shows three rare events logit models predicting the onset of democracy- and human rights-related sanctions by the UN, EU, and US from 1990–2021. Model 1 contains a larger range of potentially relevant factors derived from the literature (see

[16] See National Academies (2019) on the distinction between reproduction and replication. See also Gleditsch and Janz (2016) on reproduction and replication in IR.

above). Model 2 drops the variables found to be statistically insignificant to predicting sanctions onset. The effects of the remaining variables are largely retained. Finally, Model 3 combines the two 'trigger events' variables. These trigger events are individually rare (see Appendix A.7 – e.g., there are only a small handful of successful coups in any given year – making exact annual matching difficult. Combining coups and fraudulent elections assumes – as has been argued elsewhere (Soest and Wahman 2015a) – that they function similarly as "triggers" for sanctions imposition. Model 3 shows that this trigger events count performs similarly to the individual variables.

The upshot of this theoretical and empirical examination of the treatment assignment mechanism is that four main characteristics determine the imposition and non-imposition of UN, EU, and US sanctions intended to improve democracy and human rights:

1. major trigger events (coups, fraudulent elections),
2. pro-democracy protests in the target country,
3. political proximity between sender and target, and
4. increases in intrastate war.

This supports previous findings on the importance of sender–target relations, dramatic trigger events, and adds further insights on how senders might consider domestic opposition and protest (Soest and Wahman 2015a; Grauvogel, Licht, and Soest 2017). These four confounders will be used to match and weight sanctioned and unsanctioned country-years in the following section and main analysis.

5 Results: Do sanctions still hurt democracy and human rights?

Having established four key predictors for 'democratic sanctions' imposition in the preceding exposition of the treatment assignment mechanism, this section estimates their effects on democracy and human rights. These four predictors are also potential confounders, meaning they potentially affect both sanctions imposition *and* democracy and human rights outcomes at time t and onwards. The effect of sanctions on domestic outcomes

Table 1: Predicting the imposition of democracy- and human rights-related sanctions, 1990–2021.

	DV: 'democratic sanctions' onset		
	Full	Limited	Simplified
Constant	−2.115	−1.645†	−2.197**
	(1.357)	(0.864)	(0.836)
Any successful coup (t)	3.474***	3.455***	
	(0.374)	(0.369)	
Controversial election (t)	0.778*	0.651†	
	(0.358)	(0.357)	
Trigger events (count; t)			1.735***
			(0.246)
Increase in intrastate war (t)	1.394***	1.152**	0.898*
	(0.347)	(0.343)	(0.344)
Increase in interstate war (t)	−0.162		
	(1.444)		
Pro-democracy protests (t)	0.122	0.133	0.221*
	(0.108)	(0.101)	(0.096)
GDP growth (t-1)	−0.021		
	(0.018)		
GDP inflation (t–1)	0.000		
	(0.000)		
GDP per capita (log; t-1)	−0.214		
	(0.133)		
Political proximity (t-1)	−0.206†	−0.244*	−0.136
	(0.101)	(0.094)	(0.091)
Change in civil soc. repression (t-1)	−0.660		
	(0.469)		
Neopatrimonialism (t-1)	0.038		
	(0.906)		
Democracy (Polyarchy; t-1)	−0.702	−0.767	−0.203
	(1.392)	(1.031)	(1.005)
Human rights (HRS; t-1)	−0.600***	−0.743***	−0.695***
	(0.158)	(0.148)	(0.145)
Num.Obs.	1981	2112	2112
R2	0.137	0.119	0.083
RMSE	0.17	0.17	0.18

Note:

Cubic polynomials not displayed. Robust SEs.

Models are Firth's bias-reduced logistic regressions.

† p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

is calculated by matching, weighting, and difference-in-differences estimation (DiD) via `PanelMatch` (Imai, Kim, and Wang 2023).

5.1 Matching

Having mapped all observations and their treatment variation (see above and Appendix A.6), treated and untreated countries in a given year t are matched. Treated observations – countries with 'democratic sanctions' onset in a given year – are matched with countries in the same year that have an identical treatment history for a specified lag period L, i.e. a certain number of years preceding sanctions imposition, but were not sanctioned at t. Treatment history is a potential confounder for the present analysis (see also Blackwell and Glynn 2018: 1069): Governments previously sanctioned may react to new sanctions imposition differently than those who have not experienced them in the recent past. This includes learning effects regarding issues such as sanctions evasion, 'sanctions-proofing' the domestic economy, and strengthening relations with 'black knight' actors (Brzoska and Lopez 2016; Cilizoglu and Bapat 2020). Matching exactly on the time period also alleviates concerns of annual global particularities and the effects of world events such as the end of the Cold War or the 2007–09 recession going unrecognized (Imai, Kim, and Wang 2023: 594–595). Within these "matched sets," the single sanctioned country acts as the treated unit and all remaining unsanctioned countries as the control group. Applying this procedure with a lag time of $L = 4$ and a lead time of $F = 3$ provides a total of $n = 45$ matched sets from which estimates are derived (out of a total of $N = 86$ cases of 'democratic sanctions' onset in the examined timeframe). Appendix A.9 lists these 45 country-years included in the main analysis.[17]

5.2 Weighting and balance assessment

As matches are simply made on treatment history, the matched sets contain many control units very different from the treated unit. The next step therefore weights the observations in accordance with theoretical expectations and past empirical work regarding potential

[17]Note that there are concerns around samples of roughly this size in staggered difference-in-differences analysis; Egerod and Hollenbach (2024). However, Imai, Kim, and Wang (2023) themselves present two analyses with similar sample sizes, at $n = 26$ and $n = 103$ matched sets. See also footnote 14.

confounders (see Section 4). The aim is to refine the matched sets so as to have very similar treatment and control groups. From a methodological and epistemological perspective, the approach is similar to Mill's Most Similar Systems Design (Plümper, Troeger, and Neumayer 2019: 5, 15–16). It is also preferable to the often idiosyncratic "effective samples" produced by regular time-series regression analysis because it is theory-driven and explicit in its weighting criteria (Samii 2016; Aronow and Samii 2016).

The confounders on which control units will be weighted are drawn from the above analysis of the treatment assignment mechanism (Section 4). In other words, unsanctioned countries that are very similar to sanctioned countries on the four key factors identified above are assigned greater weight: domestic trigger events, pro-democracy protest, political proximity to the EU and US, and civil conflict. Appendix A.10 illustrates the matching and weighting procedure for an example case, the 2015 imposition of EU and US sanctions on Burundi following a power grab by incumbent president Pierre Nkurunziza and subsequent violence against anti-government protesters. It shows that the procedure acts as we expect it to, assigning the most weight to countries similar on the four identified predictors of sanctions imposition (e.g., Azerbaijan, Ethiopia, and Nigeria) and little to no weight to very different countries (e.g., Norway, Slovakia, and Georgia).

Summarizing all matched sets contained in the analysis, Table 2 displays the balance between treatment and control groups on the four main confounders, using the procedure of covariate balancing propensity score weighting (CBPS) (Imai and Ratkovic 2014). CBPS produces the best balance for the present data. The corresponding model is fitted to all observations in the treated and control groups in the entire timeframe, producing robust estimates (Imai, Kim, and Wang 2023: 595–596).

The variables for trigger events (coups and controversial elections) and civil war onset are balanced and weighted from t_{-4} through t_0 (i.e., not through t_{-1}). As von Soest and Wahman show, trigger events virtually always occur *before* sanctions imposition (hence 'trigger events'), and their measurement at t_0 is therefore generally not post-treatment in practice (Soest and Wahman 2015a: 23 n8). All other variables are examined from t_{-4} through t_{-1} to avoid post-treatment bias. As shown in Table 2, after CBPS weights are

applied, sufficient balance between treated and control groups is achieved for the potential confounders in the mentioned timeframes, using a threshold of $<|0.25|$, $<|0.2|$, or even $<|0.1|$ standardized mean differences for all variables (Stuart 2010: 11, 15; Stuart, Lee, and Leacy 2013: 3; Linden and Samuels 2013: 969; Rubin 2001: 177; Imai, Kim, and Wang 2023; Hainmueller 2012; Greifer 2021).

5.3 Difference-in-differences estimation of the causal effect

Finally, the ATT is estimated using the above matches, weights, and parameters using difference-in-differences (DiD) estimation. DiD estimation is intended to account for unobserved, time-invariant confounding factors and differences between the treated and the control groups. Figures 2 and 3 illustrate the estimated results for the two outcomes of interest.[18] The effect of treatment on outcome – of 'democratic sanctions' imposition on democracy and human rights – is estimated from t through t_{+3}. This lead time for which effects are estimated (F) is generally flexible.[19] The pre-treatment trends are estimated with a placebo test.[20]

First, for electoral *democracy*, democratic sanctions imposition is estimated to have a negative contemporaneous (t_0) causal effect of about –0.05 on a 0–1 scale, further dipping to –0.07 at t_{+1} and –0.05 in t_{+2} (Figure 2). Substantively, this suggests a notable negative effect of sanctions on democracy in the targeted country. Put into contemporary terms for 2022, these differences roughly amount to those between Norway and the UK (0.899 and 0.843), Ghana and Sri Lanka (0.633 and 0.575), Uzbekistan and Vietnam (0.221 and 0.157), or the year before and after Viktor Orbán was elected president of Hungary (2009: 0.854, 2010: 0.805).[21]

Second, the imposition of democratic sanctions is also estimated to have a negative effect on *human rights* (Figure 3). The immediate effect in the year of imposition (t_0) is –0.2

[18] Note that parallel trends (see 5.2) can also be inspected through these plots.

[19] See Cunningham (2021): 425–433 regarding this interpretation for this type of plot for difference-in-differences estimation.

[20] I thank the `PanelMatch` team, especially Adam Rauh, for answering my query about extracting standard errors and confidence intervals from `PanelMatch`; https://github.com/insongkim/PanelMatch/issues/132.

[21] The V-Dem polyarchy index for Hungary has since dropped to 0.455 in 2021.

on a –3.5–5.5 scale. The effects in subsequent years are also estimated to be negative. For 2021, a difference in the range of –0.2 roughly amounts to the differences between Germany and Taiwan (3.61 and 3.39), Romania and Hungary (1.60 and 1.40), or China and Mali (–1.71 and –1.93). However, pre-treatment trends are not entirely parallel, so these results are less reliable than those for the democracy outcome.

Substantively and in general, these negative estimates are more similar to the "corrosive" effects Peksen and Drury find for sanctions from 1972–2000 (2010: 255–256)[22] than the positive effects Wahman and von Soest find for democracy-related sanctions from 1990–2010 (2015b). Even when "signing the bias" (E. Bueno de Mesquita and Fowler 2021: 176–179), the effects are normatively concerning: While the treated group still experiences slightly more trigger events despite the weighting procedure and therefore the effects may be slightly overestimated, better balance would likely not fully remove the negative effects. That said, it is again worth highlighting the effect estimated here. This analysis examines a certain type of sanctioned country: States faced with 'democratic sanctions,' but not so extreme and long-term as to have been sanctioned for virtually the entire post-Cold War era. Appendix A.9 discusses this sample and type of country.

[22] A 6% drop in democracy in the year after the imposition of any type of limited/non-comprehensive sanctions.

Table 2: Weighted/refined/balanced control groups: Covariate balance from t–4 to t+0, both outcomes.

Time	Trigger Events	Instrastate war increase	Pro-dem. protest	Pol. proximity
t–4	0.00	0.09	0.03	-0.07
t–3	0.08	0.02	-0.01	-0.06
t–2	0.00	0.04	-0.02	-0.06
t–1	0.00	0.08	0.03	-0.05
t+0	0.14	0.06	0.37	-0.05

Trigger events and conflict increases used for weighting at t–4 to t+0, all others at t–4 to t–1.

Standardized mean differences, in SDs.

Rule of thumb: <|0.25|, <|0.2|, or <|0.1| is sufficiently balanced (Stuart 2010; Stuart et al. 2013; Imai et al. 2021, Greifer 2021), but the closer to 0, the better.

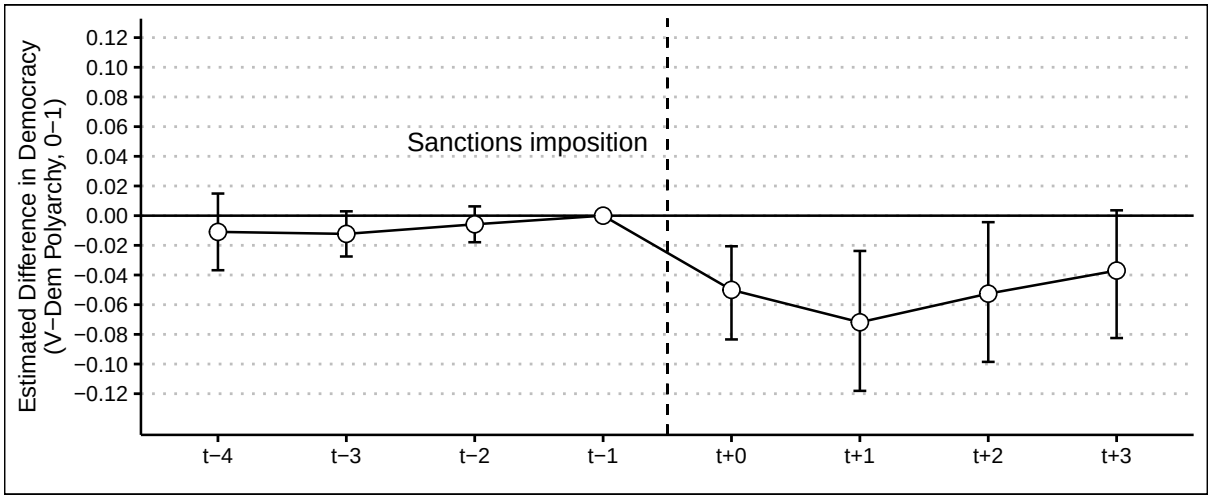

Figure 2: Estimated effect of democratic sanctions on democracy, 1990–2021 (95% CIs).

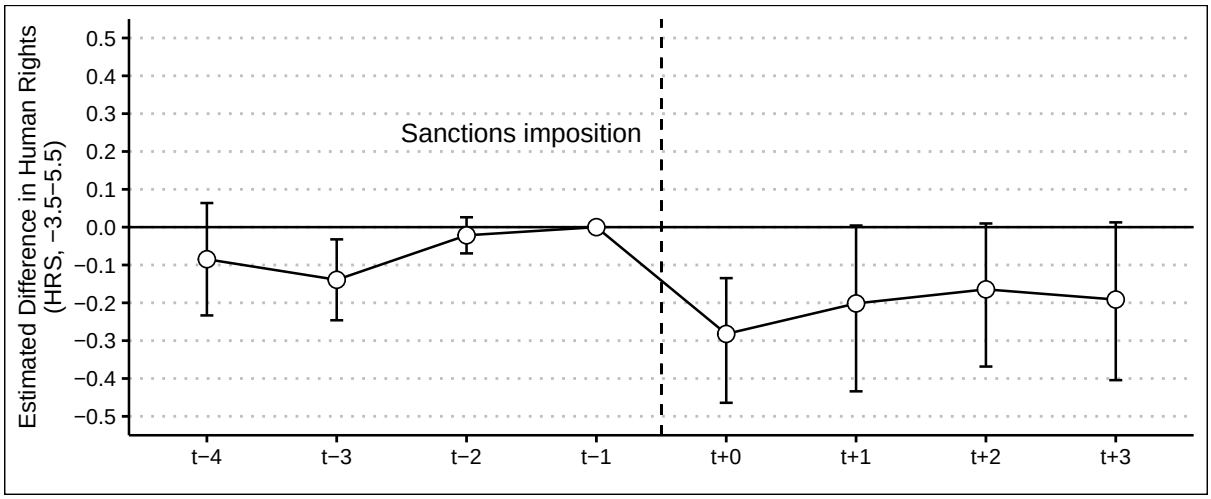

Figure 3: Estimated effect of democratic sanctions on human rights, 1990–2021 (95% CIs).

5.4 Robustness checks and sub-sample analyses

I probe these main results – which concerningly suggest negative effects of sanctions on the outcomes they seek to improve – with a number of supplementary analyses and robustness checks.

First, I conduct exploratory placebo tests to probe the mechanism driving these negative effects. This uses states sanctioned for reasons *un*related to democracy and human rights as the treatment group (e.g., sanctions imposed due to corruption, drug trafficking, weapons proliferation, etc.). Appendix A.11 further describes and presents these placebo tests.[23] In these exploratory models, there seems to be no strong effect of sanctions unrelated to democracy and human rights on democracy and human rights outcomes. Thus, the trajectory we find among states facing 'democratic sanctions' can likely be attributed to these 'democratic sanctions' and not to any underlying similarities between countries facing sanctions onset in general. In other words, there seems to be something particular about 'democratic sanctions' that causes negative effects on democracy and human rights. This cannot be examined in greater quantitative detail given the sample sizes under examination here, but may be attributed to the type of domestic pressure 'democratic sanctions' entail (Early and Peksen 2022: 4–5). These types of sanctions often play out under precarious domestic circumstances, with autocrats clinging to power, civil society aiming to weaken that hold on power, and autocrats in turn cracking down on these dissidents (see, e.g., Grauvogel, Licht, and Soest 2017; Liou, Murdie, and Peksen 2020 on mechanisms).

Next, I ran a replication of the main analysis using temporal sub-samples splitting the 1990–2021 timeframe into pre- and post-2005 eras (1990–2004 and 2005–2021, respectively). Appendix A.12 shows these subset analyses. This takes up the turn from fairly unsophisticated, broad sanctions to more targeted measures including asset freezes, which most of the literature dates to around 2005 (see Section 2.2). The results suggest that, if anything, the negative effects of 'democratic sanctions' may have worsened post-2005. Although targeted sanctions may have decreased broader humanitarian fallout as intended

[23] I thank Constantin Ruhe and Neil T.N. Ferguson for this suggestion.

by the reforms of the 2000s, autocrats may nowadays react more harshly to 'democratic sanctions' than they previously did, with democracy and human rights deteriorating as a result (see, e.g., Liou, Murdie, and Peksen 2020). While it is not possible to closely probe the reasons for this dynamic here, it is nonetheless normatively concerning that the negative effects of this type of sanctions do not seem to have markedly lessened.

Finally, these estimates are robust to different lead and lag specifications (see Sections 5.1 and 5.3).

6 Conclusion

Previous research has argued and shown that economic sanctions have historically had largely negative or mixed results on human rights and democracy outcomes in sanctioned countries. However, this research mainly covered the Cold War and 1990s. Since then, numerous policy innovations to replace indiscriminate embargoes have taken hold in sanctions practice, most prominently 'targeted' sanctions such as asset freezes and travel bans. Given these significant changes, it stands to reason that democracy- and human rights-related sanctions may now positively affect human rights and democracy, or at least be less damaging. Based on these policy innovations, this study tested two empirical expectations: That sanctions intended to improve democracy (*H1a/H1b*) and human rights (*H2a/H2b*) today in fact do so.

This article has presented a thorough re-analysis of this research question for the 1990–2021 timeframe. It applies novel matching, weighting, and difference-in-differences analysis to new treatment and outcome data. In particular, this analysis uses past insights into dramatic trigger events as predictors of sanctions imposition [i.e., the key treatment assignment mechanism; Soest and Wahman (2015b); Licht (2017)]. The results show that much of the pessimism surrounding sanctions likely continues to be warranted: Human rights- and democracy-related sanctions are estimated to have a marked negative effect on democracy outcomes for at least 3–4 years post-imposition, and largely negative but less reliably estimated effects on human rights outcomes. The type of analysis presented here cannot estimate the longer-term effects of sanctions due to timeframe and data limitations.

However, these are worth exploring given the constraining goals sanctions often pursue (Giumelli 2011, 2016).

This study therefore finds empirical support for *H1a*, which proposed that 'democratic sanctions' would in fact cause a deterioration of democracy in target states. The results on human rights are less conclusive, though they also lean towards a negative effect (*H2a*). The mid-2000s reforms towards 'targeted sanctions' have therefore seemingly not solved the major problem of humanitarian fallout caused by economic sanctions, particularly those intended to improve democracy and human rights.

Taking these empirical results seriously means further investing in ways to minimize civilian harm, especially for democracy- and human rights-related sanctions. Sanctions of this sort must be carefully considered and crafted, must not be a knee-jerk reaction, and must be thoroughly implemented and monitored if decided upon.

That said, the mechanisms through which economic sanctions – even modern targeted sanctions – elicit negative fallout remain underexplored. They may increase domestic protest (Grauvogel, Licht, and Soest 2017) and decrease government revenues (Liou, Murdie, and Peksen 2020), in turn increasing repression and leading to poorer government services and greater corruption, respectively (Liou, Murdie, and Peksen 2020; Drezner 2021: 148–149). Exploratory analyses conducted here (Section 5.4 and Appendices A.11 and A.12) suggest that 'democratic sanctions' in particular elicit negative effects, while sanctions on other issues do not, and that these effects may have turned worse since 2005. Further probing these links lies beyond the scope of this paper, but would be worth exploring through case studies of the medium-N sample covered in the analysis presented here (see Appendix A.9). For instance, such work could compare the potential differences in underlying mechanisms of targeted elite sanctions (e.g., Portela and Van Laer 2022) and broader sectoral sanctions. A sender government cannot be made fully responsible for a target government deciding to repress its citizens following sanctions (Lopez 1999: 146). As sanctions become an increasingly popular Western policy tool, they should be considered prudently as a "least bad choice" at best (Peksen 2019a: 286–287; Early and Schulzke 2018; Early and Peksen 2022).

Beyond its substantive contribution, this study has laid out and used a framework for replicating, extending, and updating past country-year IR research given innovations in causal inference methods for panel data (Imai, Kim, and Wang 2023) that can be readily applied to most canonical country-year IR work. This answers the call for more replication studies in IR (B. Bueno de Mesquita et al. 2003; Gleditsch and Janz 2016), especially regarding highly policy-relevant research questions.

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A Online Appendix

A.1 Trends in global democracy, human rights, and 'democratic sanctions'

Figure A.1 illustrates the recent dual decline of democracy and human rights globally since the early 2010s, according to two central datasets on the topic, V-Dem and HRS. First, in 2022, mean electoral democracy across all countries stands at levels last seen in 2000 and 2001 (Coppedge et al. 2024). This trend is even more striking when considering the share of the world population experiencing this reversal. Particularly populous countries, such as India, Indonesia, and the US are notably declining in democracy measures (Alizada et al. 2022: 10–11; Boese et al. 2022). Second, similarly to democracy, average human rights practices – defined narrowly as physical integrity rights – improved consistently from 1989 onwards but have in 2021 reversed to their 2009 level (Fariss 2014; Fariss, Kenwick, and Reuning 2020).

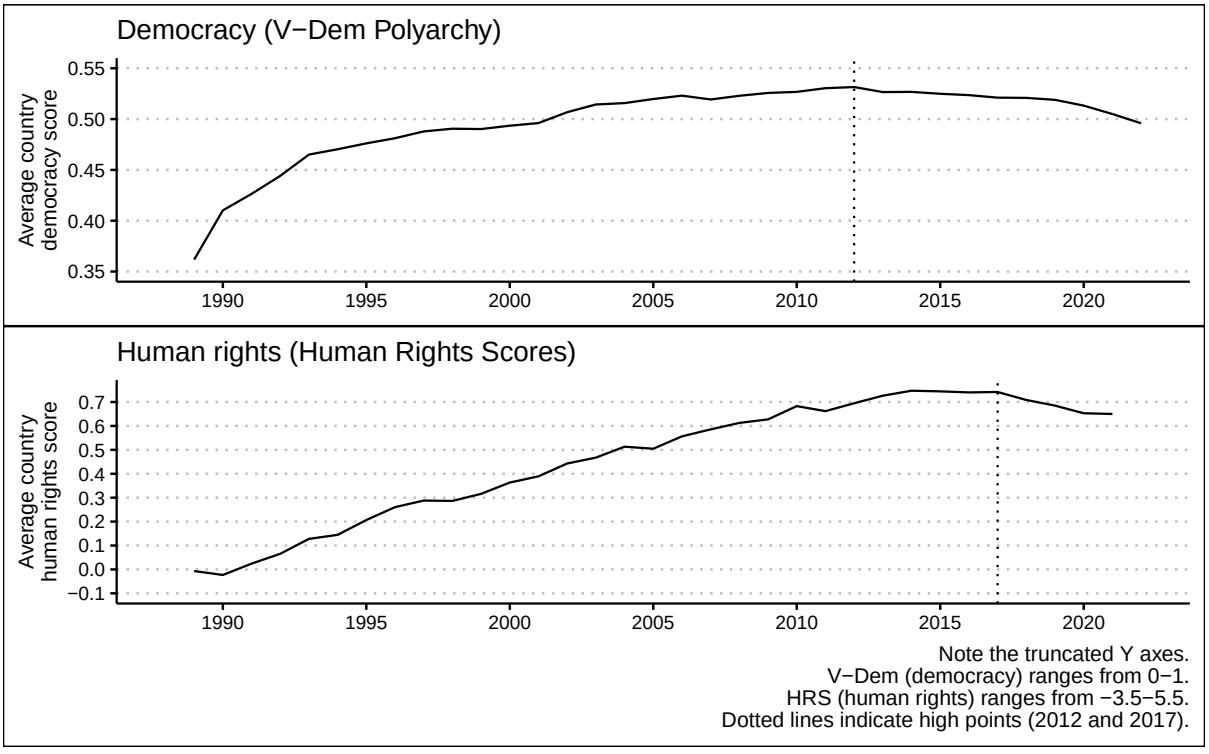

Figure A.1: Human rights and democracy, 1989–2022.

This study focuses on Western economic sanctions as a tool for democracy and human rights promotion. Domestic political dynamics and citizen movements and external democracy promotion and support are two key paths for strengthening democracy at home and abroad. However, international efforts may backfire by paving the way for "strategic manipulation" on the part of autocrats (Beaulieu and Hyde 2009), and by being used within an overarching hegemonic project on the part of powers such as the US (Wolff and Wurm 2011: 85–87).[1]

Figure A.2 shows the number of states under democracy- or human rights related UN, EU, or US sanctions from 1990–2021 (Attia and Grauvogel 2023). This shows that the pursuit of democracy and human rights through economic coercion has been a consistent goal of Western states in the post-Cold War era and has increased somewhat in recent years.

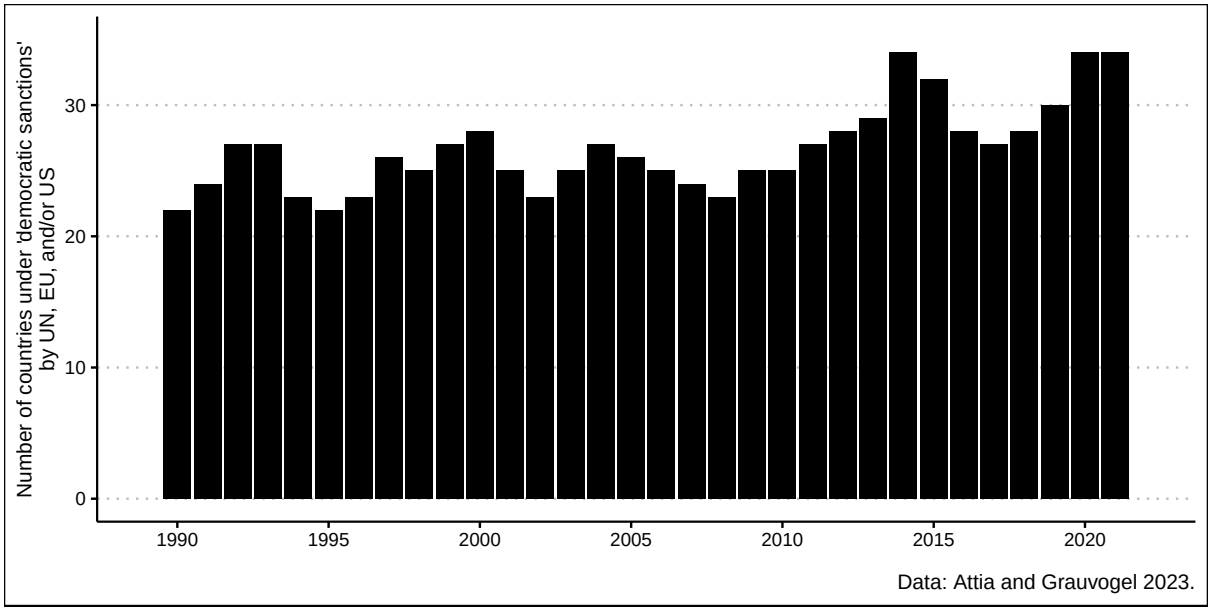

Figure A.2: Democracy- and human rights-related sanctions imposition, 1990–2021.

[1] See also Robinson (1996): 4; Kurki (2010); Poppe, Leininger, and Wolff (2019).

A.2 Literature: sanctions, democracy, and human rights

Table A.1 summarizes some of the canonical studies in the sanctions literature that examine democracy and human rights outcomes. It breaks 14 separate studies down by treatment (type of sanction, dataset), outcome (concept of human rights and/or democracy, dataset), timeframe under study, and the direction of the estimated effect. The majority of studies in this literature examine human rights outcomes. The most common datasets used to operationalize the two outcomes of interest are PTS and CIRI for human rights, and Freedom House and Polity for democracy measures (Wood and Gibney 2010; Cingranelli and Richards 2010; Freedom House 2022; Marshall et al. 2002). HSE and TIES, which cover 1914–2000 and 1945–2005, are the most commonly-used sanctions datasets (Hufbauer et al. 2009; T. C. Morgan, Bapat, and Kobayashi 2014). Most of the studies in the literature squarely focus on the 20$^{\text{th}}$ century, as the column 'range' indicates and as necessitated by these main datasets. These features underscore the main point made in Section 2: Examining the crucial question of sanctions, democracy, and human rights using newer data, newer statistical techniques for observational data, and the most recent timeframe available is a highly worthwhile and necessary endeavor for both the academic study of sanctions and for practical sanctions policy.

Table A.1: Selected past research findings on the effects of sanctions on democracy and human rights.

	Treatment		Outcome		Timeframe			
Study	Concept	Data	Concept	Data	Start	End	Range	Effect
Outcome: Human rights/physical integrity rights								
Wood 2008	Imp. sanctions, UN and US	HSE	Physical repression	PTS	1976	2001		–
Peksen 2009	Imp. sanctions, any sender	TIES, HSE	Physical integrity rights	CIRI, PTS	1981	2000		–
Escribà-Folch 2012	Imp. sanctions, any sender	HSE, Marinov	Physical integrity rights	PTS	1976	2001		–
Drury and Peksen 2014	Imp. sanctions, any sender	HSE	Women's rights	CIRI, World Bank	1971	2005		–/=/+
Carneiro and Apolinário 2016	Imp. sanctions, UN	TIES, TSC	Political repression	PTS	1992	2008		–/=
Peksen 2016	Imp. sanctions, any sender	HSE	Ethnic discrimination	MAR, Sorens 2010	1950	2003		–
Neuenkirch and Neumeier 2016	Imp. sanctions, US	Wood, HSE, authors	Poverty	–	1982	2011		–
Adam and Tsarsitalidou 2019	Imp. sanctions, US	TIES	Civil liberties	FH, CIRI	1972	2014		–
Gutmann et al. 2020	Imp. sanctions, US	Authors	Basic, econ., women's, pol. rights	CIRI, FH, others	1976	2012		=/=/+/–
Gutmann et al. 2021	Imp. sanctions, UN and US	Wood, HSE	Life expectancy, women's health	World Bank	1977	2012		–
Liou et al. 2021	Imp. sanctions, any sender	TIES	Physical integrity rights	CIRI, PTS, HRS	1990	2005		–
Early and Peksen 2022	Imp. sanctions, US	TIES, EUSANCT	HR, econ., public health	World Bank, V-Dem	1971	2015		–
Outcome: Democracy/political rights								
Peksen and Drury 2010	Imp. sanctions, any sender	HSE, TIES	Civil liberties, pol. freedoms	FH	1972	2000		–
von Soest and Wahman 2015	Imp. sanctions, US and EU	GIGA	Democracy	FH, Polity	1990	2010		+
Gutmann et al. 2020	Imp. sanctions, US	Authors	Basic, econ., women's, pol. rights	CIRI, FH, others	1976	2012		=/=/+/–
Present study – Outcome: Democracy and human rights								
Present study	Imp. sanctions, UN/US/EU	IST	Democracy, human rights	V-Dem, HRS	1989	2021		TBD

'Range' column: Dotted vertical line indicates 2005 as a rough demarcation of the increased use of targeted sanctions.
'Effect' column: '+' indicates an improvement; '=' indicates no change; '–' indicates a deterioration.

A.3 Summary statistics

Table A.2 presents summary statistics for the main substantive variables covered in this study's analysis. The column on 'coverage' shows how complete the data on the variable in question is. The variables describing discrete events – sanctions imposition, coups, fraudulent elections, conflict onset – are assumed to be complete in their datasets, so that all country-years without an instance of the event in question are coded as '0.' The column shows that the data is sufficiently complete and coverage for the 1990–2021 timeframe is good. The righthand column visualizes the distribution of the variable in question.

Table A.2: Summary statistics for the main variables, 1990–2021.

Variable (N = 5.667)	N	Coverage	Min	Mean	Max	SD	Distribution
Sanctions (treatment)							
Democratic sanctions in place Y/N (IST)	5667	100.0%	0.00	0.15	1.00	0.36	
Only non-democratic sanctions in place Y/N (IST)	5667	100.0%	0.00	0.06	1.00	0.23	
Democracy and human rights (outcomes)							
Democracy (V-Dem Polyarchy)	5665	100.0%	0.01	0.50	0.92	0.27	
Human rights (HRS/Fariss)	5488	96.8%	-3.40	0.48	5.50	1.60	
Confounders: trigger events							
Successful coup (Colpus/Chin et al.)	5667	100.0%	0.00	0.01	1.00	0.11	
Controversial election Y/N (V-Dem)	5667	100.0%	0.00	0.04	1.00	0.21	
Trigger events count	5667	100.0%	0.00	0.06	2.00	0.23	
Confounders: other factors							
Increase in intrastate war (UCDP)	5644	99.6%	0.00	0.04	1.00	0.20	
Political proximity to US and EU	5319	93.9%	0.11	4.10	10.00	2.10	
Mobilization for democracy (V-Dem)	5626	99.3%	-3.20	-0.23	4.40	1.30	
GDP growth (World Bank)	5320	93.9%	-64.00	3.50	153.00	7.30	

A.4 Democracy and human rights as objectives of Western sanctions

Using IST data (Attia and Grauvogel 2023), Table A.3 shows the coincidence of democracy and human rights improvement as Western sanctions objectives from 1990–2021. These goals are frequently pursued in parallel by senders: 62.2% of country-years faced with either are faced with both types of sanctions (542 of 870). The table also illustrates the overall frequency of democracy- and human rights-related sanctions put into country-year terms: 870 of 6.005 (14.5%) country-years between 1990 and 2021 featured this type of sanctions.

Table A.3: Coincidence of democracy and human rights as sanctions objectives, 1990–2021.

	Human rights	
Democracy	N	Y
N	5135	195
Y	133	542

Observations are country-years.

A.5 Threats of democracy- and human rights-related sanctions imposition

Table A.4 examines how often threats of democracy- and human rights-related sanctions are successful. If *threats* of democracy- and human rights-related sanctions are frequently successful, this would pose a significant challenge to analyzing the effects of such sanctions, because merely examining *imposed* sanctions of this type would omit numerous cases of successful threats. This follows the approach laid out by von Soest and Wahman (2015b: 961–962). The table uses EUSANCT data (Weber and Schneider 2022) because this dataset includes sanctions threats from 1989–2015 (unlike IST, which I use for all other analyses). The table's rows show three different sanctions objectives: improving democracy, improving human rights, and all other sanctions objectives. The column 'Threat successful' shows the share of cases in which a sanctions threat was issued and the target state acquiesced to the sender's demands before sanctions imposition. 'Threat credible' calculates how often threats were issued, the target did not acquiesce, and the sender then indeed imposed the threatened sanctions, thus making the initial threat credible. Finally, 'Threat not credible' shows the share of sanctions threats that were ultimately not backed up by the sender.

These calculations support von Soest and Wahman's key point that the threat stage of human rights- and democracy-related sanctions is not in fact particularly important, in that targets quite rarely 'fold' at this stage. Only about one in nine such threats is successful (11 of 93; 11.7%), likely because concessions by authoritarian regimes to democracy- and human rights-related demands are particularly costly for the target and conceding early on these points makes dictators more likely to be held accountable and relinquish power (Soest and Wahman 2015b: 961–962). Conversely, 45% of all other sanctions threats are successful, an effect also known as the 'hidden hand of economic coercion' (Nooruddin 2002; Drezner 2003). Therefore, while important for analyzing other types of economic sanctions and their effectiveness, the threat stage can be bracketed here when analyzing democracy- and human rights-related sanctions.

Table A.4: The success of sanctions threats by objective, 1989–2015.

		Threat successful		Threat credible	Threat not credible
Objective	Threats	Sum	Share	Share	Share
Other issues	100	45	45.0%	63.6%	36.4%
Democracy	21	3	14.3%	83.3%	16.7%
Human rights	73	8	11.0%	89.2%	10.8%

US ICC-related sanctions removed.

A.6 Treatment variation: How common are 'democratic sanctions'?

How common are democracy- and human rights-related sanctions? Figure A.3 illustrates the treatment variation within the analyzed sample. Gray tiles show country-years for which sanctions data is complete and which did not face democracy- or human rights-related sanctions. Black tiles represent country-years with this type of sanctions imposed, i.e., treated units. A group of six countries (Myanmar, Cuba, Iran, China, North Korea, Somalia) was sanctioned for the entire timeframe, thus being dropped from the analysis because no change in treatment status can be observed and they cannot be used as control units. At least one treatment onset can be observed for a total of 63 countries (of 181; 34.8%) – ranging from Sudan, which was sanctioned in 31 of 32 observed years, to Malawi, which was sanctioned in one of 32 years. Finally, 110 countries (60.8% of the sample) were never sanctioned. Furthermore, there are no obvious temporal clusters in Western sanctions practice. This shows the range of treatment variation necessary to usefully apply `PanelMatch` (Imai, Kim, and Wang 2023: 591–592).

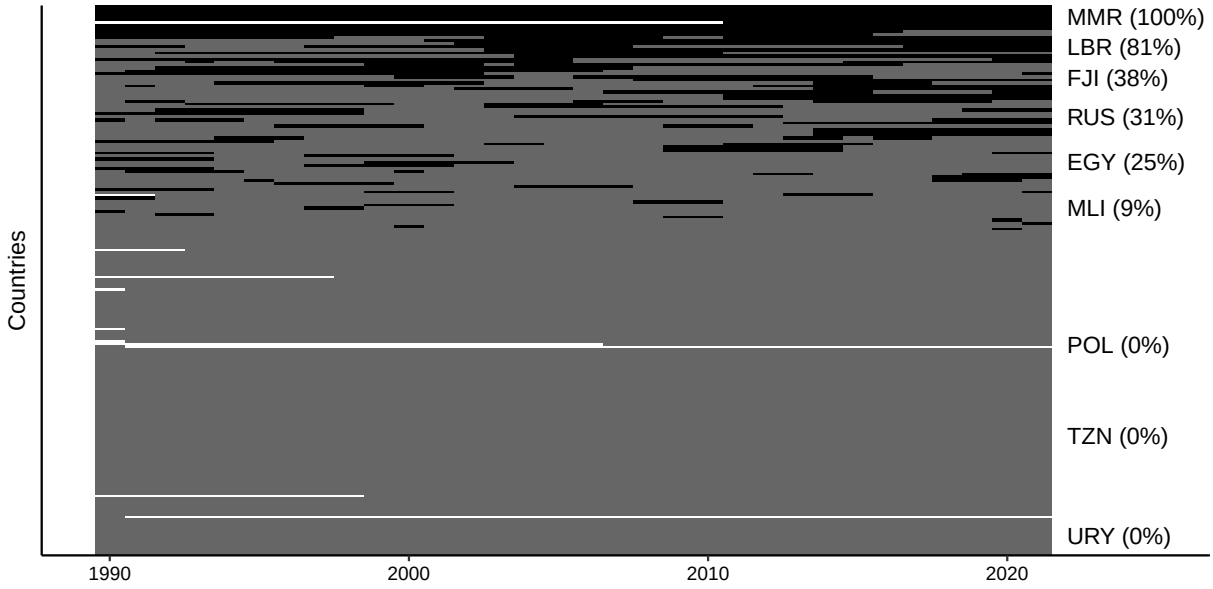

Figure A.3: Treatment variation plot: democracy- and human rights-related sanctions by the UN, EU, or US, 1990–2021.

A.7 Coups, fraudulent elections, and sanctions imposition

How commonly are coups and fraudulent elections met with sanctions imposition? Figure A.4 plots all country-year observations of coups and fraudulent elections and whether sanctions were newly imposed in the same year (Chin, Carter, and Wright 2021; Chin and Kirkpatrick 2023; Hyde and Marinov 2012; Coppedge et al. 2024).[2] The lefthand panel shows that sanctions remain a common reaction to coups. The righthand panel illustrates that while from 1990–2004, one in eleven controversial elections was met with 'democratic sanctions,' only one in fifty has since 2005.[3] Descriptively, while somewhat widely used as reactions to controversial elections in the early 1990s, sanctions are today more rarely used in this manner. This perhaps suggests an increasing sensitivity of international election monitors to fraud, but a decreasing Western willingness to meet such fraud with sanctions. This descriptive result is likely also driven by the fact that – as Western sanctions have continuously expanded (see Section 2.2) – more countries in which fraudulent elections are held today are already under under Western sanctions in the first place, omitting them from this figure. All that said, country-years with fraudulent elections remain more likely to be sanctioned than country-years without such elections (Section 4.3).

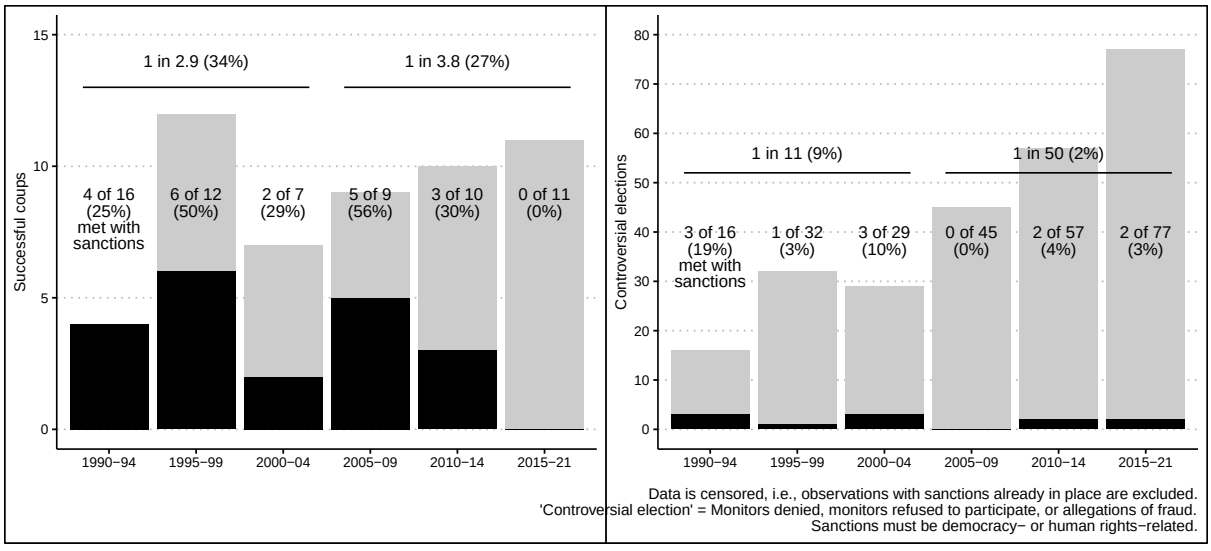

Figure A.4: Coups, controversial elections, and the imposition of democracy- and human right-related sanctions, 1990–2021.

[2]The data censors out observations that already have such sanctions imposed on them. For example, although Egypt held fraudulent elections in 2018, the country was already under US human rights- and democracy-related since 2012 and 2014. Therefore, new sanctions onset is not possible and the observation is dropped.

[3]7 of 76 and 3 of 150, respectively.

A.8 Replication and extension of von Soest and Wahman (2015a)

Table A.5 replicates and extends von Soest and Wahman's analysis of when 'democratic sanctions' are imposed (2015a). Specifically, this replicates Table 3 in their study in two steps (2015a: 27). First, it presents logit and rare events logit models for the 1990–2010 timeframe they analyze in their study. Second, this timeframe is extended to the 1990–2021 period analyzed in this study. Their results hold for 1990–2010 and with the reconstructed dataset used in the present study; coups retain their strong predictive power, as do controversial elections and political proximity to Western states. GDP per capita and FDI have similar effects as in the original study. These results also largely hold for 1990–2021, giving confidence in both the original analysis and the dataset constructed for the purpose of the present study.

Table A.5: Replications and extensions of von Soest and Wahman 2015a; Table 3.

	DV: 'democratic sanctions' onset			
	1990–2010 (replication)		1990–2021 (extension)	
	Logit	RE logit	Logit	RE Logit
Constant	0.914	0.604	−0.965	−1.516†
	(1.178)	(1.095)	(0.851)	(0.821)
Coup	3.806***	3.726***	3.371***	3.425***
	(0.445)	(0.423)	(0.366)	(0.361)
Controversial election	1.284**	1.268*	0.897*	0.870*
	(0.492)	(0.450)	(0.374)	(0.357)
Any other sanction (t-1)	0.235	0.118	0.692†	0.616
	(0.701)	(0.569)	(0.408)	(0.375)
Pro-democracy protests	0.198	0.181	0.147	0.120
	(0.132)	(0.122)	(0.101)	(0.096)
GDP growth (t-1)	−0.026	−0.019	−0.008	−0.014
	(0.028)	(0.023)	(0.022)	(0.020)
GDP inflation (t–1)	−0.001	0.000		0.000
	(0.002)	(0.000)		(0.000)
GDP per capita (t-1)	−0.111	−0.082	−0.099†	−0.059†
	(0.083)	(0.058)	(0.052)	(0.036)
Political closeness (t-1)	−0.295*	−0.248*	−0.188*	−0.165†
	(0.123)	(0.111)	(0.094)	(0.088)
Oil revenue (t-1)	0.000		0.000	
	(0.000)		(0.000)	
FDI as share of GDP (t–1)	−0.070	−0.056	−0.062†	−0.068†
	(0.054)	(0.047)	(0.036)	(0.032)
Num.Obs.	1374	1388	2015	2029
R2		0.181		0.137
AIC	332.1		524.6	
BIC	405.2		597.5	
Log.Lik.	−152.034		−249.279	
F	7.165		9.298	
RMSE	0.17	0.17	0.17	0.17

† p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Cubic polynomials not displayed. Robust SEs.

GDP per capita in thousands, Oil revenue in millions.

A.9 Included cases of sanctions onset

Table A.6 lists all instances of 'democratic sanctions' onset included in the study's main analysis, a total of $n = 45$ observations. Note that observations with onset in 1990–93 and 2019–21 are mainly dropped due to data availability or insufficient lag times for matching and weighting (t–4 through t–1/t0), and lead times for matching and effect estimation (t0 through t+3), respectively. Some other instances of sanctions onset – largely in the 1990s – are dropped due to missing data (see Appendix A.3). Finally, as discussed in Appendix A.6, states who have been sanctioned continuously or near-continuously do not feature in the analysis, as either sanctions onset cannot be observed, or because there are no exact matches on treatment history. Note that there are concerns around samples of roughly this size in staggered difference-in-differences analysis (Egerod and Hollenbach 2024). However, Imai et al. (2023) themselves present two analyses with similar sample sizes, at $n = 26$ and $n = 103$ matched sets. See also footnote 14 in the main text.

These data constraints explain the difference between the overall total of $N = 86$ instances of democracy- and human rights-related sanctions onset in the timeframe and this final number of $n = 45$ cases covered in the analysis. The table gives the reader an impression of the 'type' of country under examination in this study: The shown countries are largely autocracies, though not autocracies that have been sanctioned continuously. This temporal distribution also shows that the majority of cases are drawn from the 21st century, in line with the study's research interest and title.

Table A.6: Included cases of democracy and human rights-related sanctions onset, 1990–2021.

#	Country	Year
1	Russia	1999
2	Pakistan	1999
3	Ethiopia	1999
4	Côte d'Ivoire	1999
5	Comoros	1999
6	Peru	2000
7	Austria	2000
8	Fiji	2000
9	Zimbabwe	2001
10	Ukraine	2002
11	Belarus	2002
12	Nigeria	2003
13	DR Congo	2003
14	Moldova	2003
15	Nepal	2004
16	Côte d'Ivoire	2004
17	Eritrea	2004
18	Thailand	2006
19	Fiji	2006
20	Sri Lanka	2007
21	Mauritania	2008
22	Equatorial Guinea	2008
23	Honduras	2009
24	Guinea	2009
25	Madagascar	2009
26	Syria	2011
27	DR Congo	2011
28	Guinea-Bissau	2011
29	Libya	2011
30	Russia	2012
31	Mali	2012
32	Egypt	2013
33	Rwanda	2013
34	Bosnia and Herzegovina	2013
35	Yemen	2014
36	Thailand	2014
37	Uganda	2014
38	Venezuela	2014
39	Ukraine	2014
40	Gambia	2014
41	Burundi	2015
42	Cambodia	2017
43	Nicaragua	2018
44	Turkey	2018
45	Saudi Arabia	2018

Observations with onset in 1990–93 and 2019–21 mainly dropped due to data availability or the required lag and lead times.

A.10 `PanelMatch` illustration: Burundi 2015

Table A.7 illustrates the matching and weighting procedure used in `PanelMatch` (Imai, Kim, and Wang 2023) for a single example case of sanctions imposition: the imposition of sanctions against Burundi in 2015. The table shows that the procedure acts as expected: The countries assigned the greatest weight are similar to Burundi on the four key confounders identified in Section 4 (i.e., trigger events, civil war onset, protest, and political proximity to the West). For the sake of simplicity, the table shows the averages of the two years before sanctions imposition on Burundi (2013 and 2014 for protest and political proximity) and the averages of the year of imposition and one year before for the events variables (i.e., trigger events and civil war onset; 2014 and 2015).

The countries most similar to Burundi on these confounders are Azerbaijan, Ethiopia, and Nigeria, all at about 13% of the total weight assigned. Conversely, very different countries on these factors – Norway, Slovakia, Georgia – are assigned the least weight. The similarity on pre-treatment outcomes shown in the righthand columns (which are not used for weighting) also supports the parallel trends assumption (see Section 5).

Figure A.5 illustrates these three closest matches and important parts of `PanelMatch` (Imai, Kim, and Wang 2023) as applied to the data at hand. It shows the three countries making up about 40% of the weight within the control group constructed for the treated (i.e., sanctioned) case of Burundi in 2015: Nigeria, Ethiopia, and Azerbaijan (Table A.7). The Y axis shows these four countries' level of democracy from 1989–2021. Each country's line also indicates whether the country faced democracy- or human rights- related sanctions at any given year in this timeframe, which all four indeed did at some point. The lag and lead windows for the case of Burundi in 2015 is highlighted in dark grey; this is the timeframe that is relevant for the exact matching, weighting, and difference-in-differences estimation used in `PanelMatch` with this study's specifications. The lag period is used for matching on treatment history and weighting on relevant covariates. The lead period is used to estimate the ATT.

Table A.7: Illustration for democratic sanctions onset: Treated group and control group with weights for 2015.

							Outcomes (not used for weighting)	
Rank	Country	Weight	Trigger Events/Year (Y/N)	Increase in Civil War/Year (Y/N)	Pro-Dem. Protest	Political Proximity US/EU	Democracy	Human Rights
Treated: Burundi, democratic sanctions onset in 2015								
–	Burundi	–	0.5	0.5	-0.36	5.03	0.28	-0.90
Control group: Countries most and least likely to be sanctioned in 2015, and not in fact sanctioned								
1	Azerbaijan	13.5%	0.5	0.5	-1.18	4.90	0.20	-0.17
2	Ethiopia	13.0%	0.5	0.0	1.19	4.94	0.23	-1.08
3	Nigeria	12.3%	0.5	0.0	1.52	5.19	0.54	-1.65
4	Kazakhstan	5.8%	0.5	0.0	-0.18	4.58	0.23	-0.07
5	Tajikistan	4.7%	0.5	0.0	-0.64	5.24	0.20	-0.11
6	Haiti	4.2%	0.5	0.0	1.43	4.21	0.42	0.47
7	Afghanistan	3.5%	0.5	0.0	0.17	5.54	0.38	-1.44
8	Pakistan	2.3%	0.0	0.0	-0.26	5.40	0.46	-1.44
9	India	1.8%	0.0	0.5	0.67	5.28	0.64	-1.07
10	Maldives	1.5%	0.5	0.0	0.72	4.91	0.43	1.29
128	Belgium	0.1%	0.0	0.0	-1.70	1.04	0.90	2.77
129	Portugal	0.1%	0.0	0.0	-1.31	1.13	0.89	1.76
130	Slovakia	0.1%	0.0	0.0	-0.72	1.00	0.84	2.12
131	Georgia	0.1%	0.0	0.0	-0.96	1.38	0.65	1.08
132	Norway	0.1%	0.0	0.0	-2.11	1.12	0.89	4.38

Note:

Values are averages for the following timeframes, as indicated in the body text:

Trigger events: 2014–15, all others: 2013–14

Outcome variables are only displayed descriptively and are not used for refinement/weighting.

Trigger events: binary variable for t and t–1

Increase in civil war: binary variable for t and t–1

Pro-democracy protest: roughly -4 (none) to 4 (widespread)

Political proximity: 0 (very close) to 10 (very different)

Democracy: 0 (very undemocratic) to 1 (very democratic)

Human rights: -3 (poor) to +6 (respected)

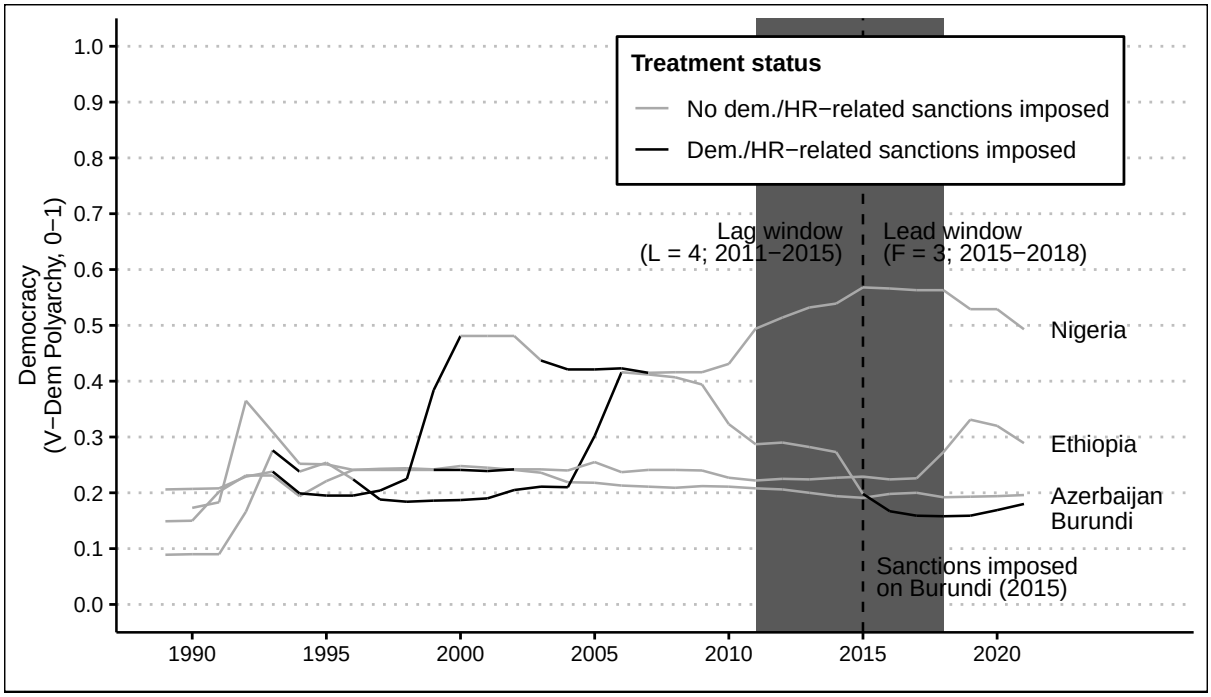

Figure A.5: Illustration: Burundi 2015 and its three closest matches.

A.11 Placebo tests: sanctions not related to democracy or human rights

These analyses start with two of the four confounders used in the main analyses: increases in intrastate conflict and political proximity. They drop the pro-democracy protest and trigger event variables, because these are not theoretically expected to affect the imposition of sanctions unrelated to democracy and human rights. Among the $n = 28$ matched cases of this type of sanctions imposition, there are no cases of increases in civil war, so this variable is ultimately also not used. Thus, the types of countries under examination are sanctioned and do not face increases in domestic conflict. They include cases of sanctions imposition such as Guyana in 2001 (US sanctions over the repatriation of foreign nationals), Bulgaria in 2008 (EU sanctions surrounding corruption and pre-accession EU funding), and Azerbaijan in 2017 (US sanctions over Azerbaijan's relations with Armenia, known as "Section 907"). See the IST dataset for more complete descriptions of these cases (Attia and Grauvogel 2023). Otherwise, the `PanelMatch` specifications are identical (e.g., on leads and lags).

Table A.8 shows the balance of treated and control cases once weights are applied. It shows that treated and control units are sufficiently balanced on political proximity, and that the pre-treatment trends on the outcome variables (which are not used for weighting) also run in parallel. Using this matching and weighting, Figure A.6 shows the effects of sanctions unrelated to democracy and human rights on democracy. Figure A.7 shows the effects of sanctions unrelated to democracy and human rights on human rights. Both results indicate that these types of sanctions do not negatively affect democracy and human rights outcomes in sanctioned countries. This suggests that it is not sanctions as such that are driving this study's main estimates, but rather 'democratic sanctions' specifically.

Table A.8: Placebo tests: Weighted/refined/balanced control groups, covariate balance from t–4 to t+0, both outcomes.

		Outcomes (not used for weighting)	
Time	Pol. proximity	Polyarchy (V-Dem)	Human rights (HRS)
t–4	-0.15	0.02	-0.10
t–3	-0.12	-0.01	-0.15
t–2	-0.11	0.00	-0.11
t–1	-0.12	0.03	-0.17
t	-0.15	0.03	-0.17

Political proximity used at t–4 to t–1; democracy and human rights not used for weighting.
Standardized mean differences, in SDs.
Rule of thumb: <|0.25|, <|0.2|, or <|0.1| is sufficiently balanced (Stuart 2010; Stuart et al. 2013; Imai et al. 2021, Greifer 2021), but the closer to 0, the better.

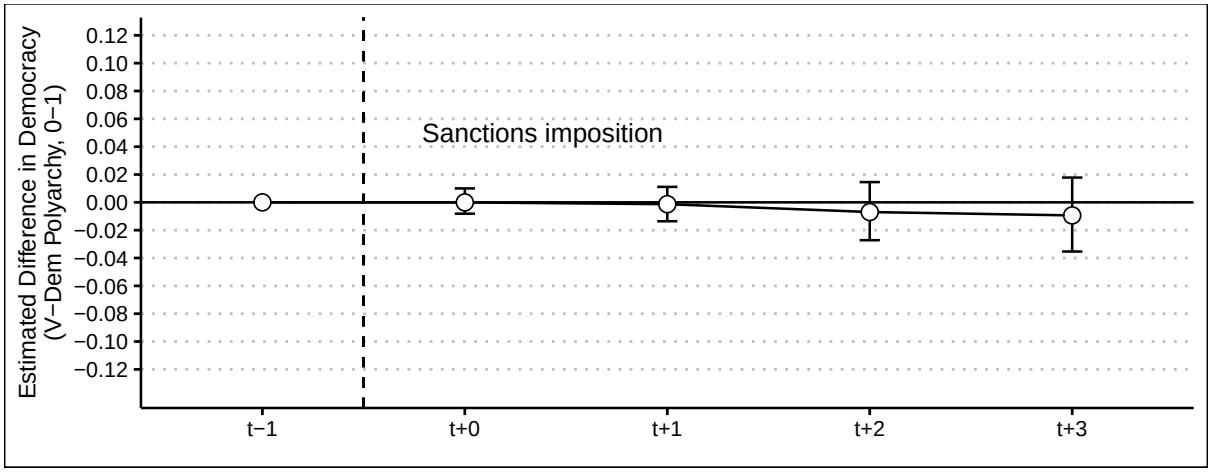

Figure A.6: Placebo test: the effects of sanctions unrelated to democracy and human rights on democracy.

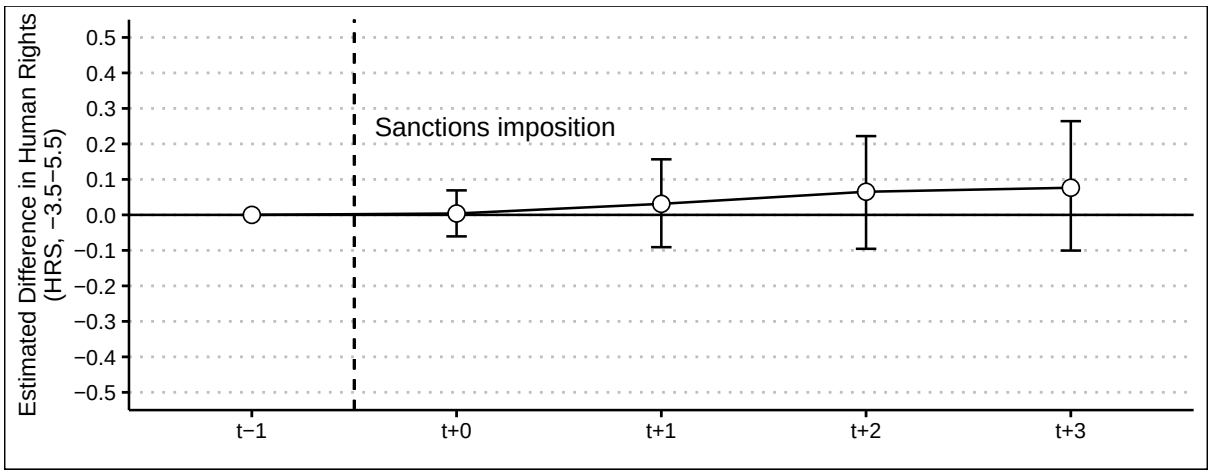

Figure A.7: Placebo test: the effects of sanctions unrelated to democracy and human rights on human rights.

A.12 Subset analyses: 1990–2004 and 2005–2021

How might the estimated effects of democracy- and human rights-related sanctions on democracy and human rights have changed since the advent of targeted sanctions in the early 2000s (Section 2.2)? I apply the estimation strategy used in this paper to two temporal subsets of the full timeframe: 1990–2004 and 2005–2021. Given these shorter timeframes than in the main analysis, I also shorten the lag timeframe to $L = 1$ (i.e., the pre-treatment timeframe on which treatment history is matched and for which matches are weighted). For the sake of brevity, I only report results for the democracy outcome in the following.

Tables A.9 and A.10 show that treatment and control groups are well-balanced in both the 1990–2004 and 2005–2021 timeframe across all four confounders used in the analyses. Figures A.8 and A.9 show the ATT estimates for these two timeframes. These timeframes include $n = 25$ and $n = 30$ cases of sanctions imposition, respectively. If anything, these separate analyses indicate that the negative effects of 'democratic sanctions' have turned stronger in more recent years, after the turn to 'targeted sanctions' and the more sophisticated sanctions policies and implementation around 2005. Although targeted sanctions may have decreased humanitarian fallout as intended, autocrats may nowadays react more harshly to 'democratic sanctions' than they previously did (see, e.g., Liou, Murdie, and Peksen 2020 on mechanisms). These results are further discussed in the main text and especially in the conclusion (Section 6).

Table A.9: Weighted/refined/balanced control groups: Covariate balance from t–1 to t, outcome: democracy, 1990–2004.

Time	Trigger Events	Instrastate war increase	Pro-dem. protest	Pol. proximity
t–1	0.04	-0.16	-0.05	0.00
t	0.06	0.06	0.10	-0.04

Trigger events and conflict increases used for weighting at t–1 to t0, all others at t–1.
Standardized mean differences, in SDs.
Rule of thumb: $<|0.25|$, $<|0.2|$, or $<|0.1|$ is sufficiently balanced (Stuart 2010; Stuart et al. 2013; Imai et al. 2021, Greifer 2021), but the closer to 0, the better.

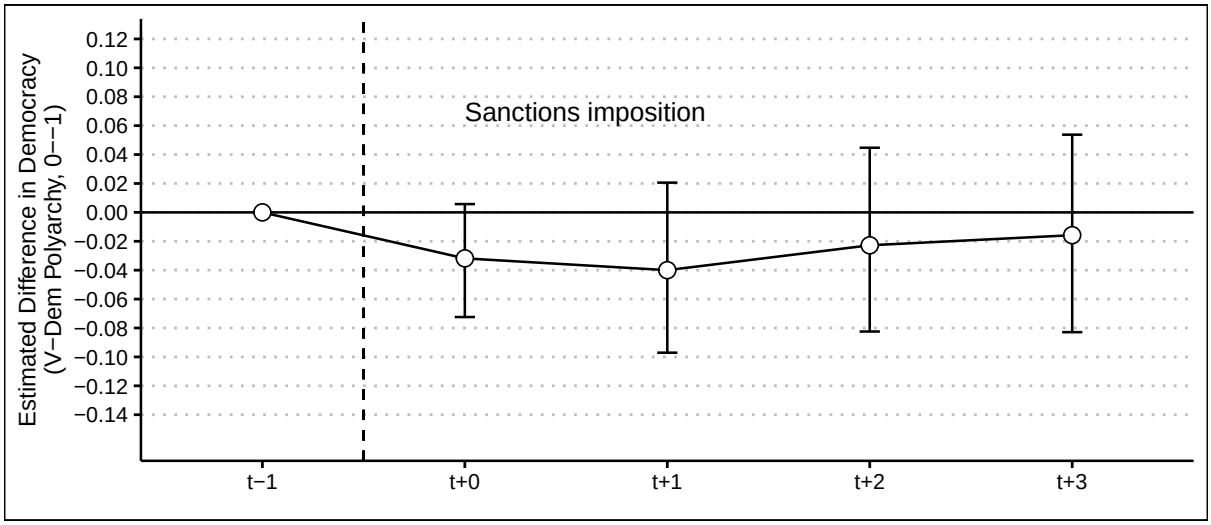

Figure A.8: Estimated effect of democratic sanctions on democracy, 1990–2004 (95% CIs).

Table A.10: Weighted/refined/balanced control groups: Covariate balance from t–1 to t, outcome: democracy, 2005–2021.

Time	Trigger Events	Instrastate war increase	Pro-dem. protest	Pol. proximity
t–1	-0.03	0.05	0.03	0.01
t	0.05	0.11	0.36	0.05

Trigger events and conflict increases used for weighting at t–1 to t0, all others at t–1.
Standardized mean differences, in SDs.
Rule of thumb: <|0.25|, <|0.2|, or <|0.1| is sufficiently balanced (Stuart 2010; Stuart et al. 2013; Imai et al. 2021, Greifer 2021), but the closer to 0, the better.

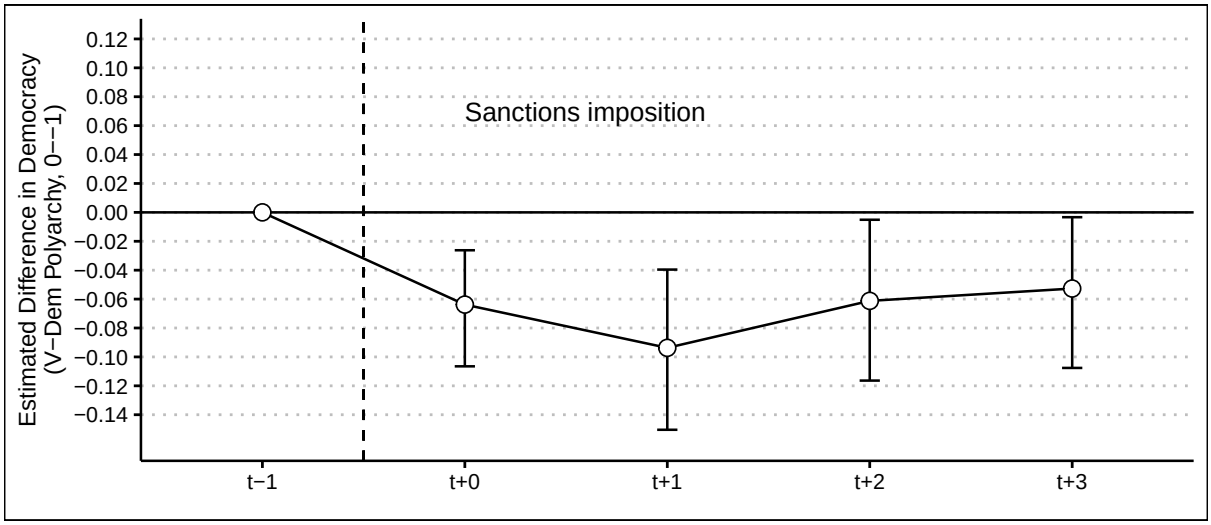

Figure A.9: Estimated effect of democratic sanctions on democracy, 2005–2021 (95% CIs).