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The Political Economy of International Sanctions

Dissertation

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Our knowledge can only be finite,

while our ignorance must necessarily be

infinite.

– Karl R. Popper

Preface

The present doctoral thesis follows a cumulative format and consists of an introductory chapter that provides a brief *Overview*, followed by five research articles. Chapter 2, 'International Sanctions and Emigration', is a peer-reviewed article published in the *Journal of Economic Behavior & Organization*. Chapter 4, 'Leader Similarity and International Sanctions', is likewise peer-reviewed and published in the *Journal of Conflict Resolution*. Chapter 5, 'International Sanctions and Corruption', has been submitted to *World Development*. My single-authored contribution is Chapter 3, 'International Sanctions and Coups d'État'. Chapter 3 and the final Chapter 6, 'International Sanctions and Constitutional Compliance', are expected to be submitted to academic journals soon.

Except for Chapter 3, all articles were written jointly with my doctoral supervisor, Prof. Dr. Matthias Neuenkirch, and Dr. Jerg Gutmann. Dr. Gutmann contributed primarily to the theoretical development of the respective papers, while Prof. Dr. Neuenkirch was mainly responsible for revising the manuscripts and describing the empirical findings. All empirical analyses reported in the articles were carried out using the statistical software packages *R* and *Stata*, relying on both self-written functions and established libraries.

The main progress on this work was achieved between 2023 and 2025 at Trier University, while being employed as a research and teaching assistant at the Professorship of Empirical Economics (Prof. Dr. Matthias Neuenkirch), and during my final year, from 2025 to 2026, at the University of Oxford, Jesus College.

All contributions included in this dissertation examine the causes and consequences of international sanctions. The underlying ideas were mostly developed jointly, while the data collection and empirical implementation were initially carried out independently. The results were subsequently discussed with my supervisor and Dr. Jerg Gutmann. Overall, I pursued and completed my doctoral studies in an excellent working environment.

Oxford, April 2026

Acknowledgements

After nearly 12 years of studying Economics, Political Science, and Applied Statistics, this dissertation marks the official end of my time as a student. For more than four of those years, I served as a doctoral candidate and research assistant at the Professorship of Empirical Economics at Trier University. Throughout this time, I was always given the opportunity to pursue my own interests. During my doctoral studies, I was able to continue running our small business with one of my best friends, spent two years as part of the fantastic Big Data team at the Leibniz Institute for Psychology in Trier, and, as I finish this thesis, I find myself at one of the best – perhaps the best – universities in the world, the University of Oxford. Attending conferences in places like Münster, Vienna, Riga, Madrid, and most recently Glasgow has given me unforgettable memories of fascinating conversations with exceptional researchers, fun evenings after a long day of work, and simply beautiful places all across Europe. I began my academic journey in the birthplace of Karl Marx, Trier, and attended my final academic conference as a student during the 250^{th} anniversary of 'The Wealth of Nations' by Adam Smith in Scotland. The circle is complete – or at least that is the tidy story I tell myself.

It was an intense but also unforgettable period of my life, and I would make every one of those decisions exactly the same way again. As a young adult after graduating from school, Trier University opened an entirely new world to me. Apart from my school teachers, I did not know anyone who had attended university. It gave me, among many other things, the opportunity to study in Turkey and Belgium, providing me with new experiences, perspectives, and lasting friendships. I am deeply grateful and will always remain closely connected to both the university and the city of Trier.

I would like to thank all my (former) colleagues and friends at Trier University, at the Leibniz Institute for Psychology and at the IAAEU, where I took my first steps in academia during my master's studies through a position as a student assistant. Moreover, I am also grateful to the University of Luxembourg, where I attended courses as a visiting student during my doctorate, and especially to Jesus College at the University of Oxford, which provided me with a truly unique environment during perhaps the

most exciting year of my life. My sincere thanks to everyone I was fortunate enough to meet along this long journey. You made these years unforgettable. To avoid doing anyone a disservice by leaving them out, I have chosen not to mention names here, but as I write these words, I am thinking of each and every one of you.

I would especially like to acknowledge my supervisor, Prof. Dr. Matthias Neuenkirch. He has supported and guided me throughout my work. Without his support, this dissertation would not have taken its present form; indeed, I likely would never have pursued a doctorate at all. The close guidance and at the same time flexibility that he offered are not something to be taken for granted. He is not only my supervisor and co-author of numerous research papers, but has also become a friend with whom I share both serious and joyful moments. I could not have imagined a better supervisor than you.

Furthermore, I would like to acknowledge Dr. Jerg Gutmann of the University of Hamburg, who contributed substantially to the success of my doctoral years. Four of the five research papers included in this dissertation were written in collaboration with him, and he always had an open ear and insightful remarks regarding my single-authored work as well. I am very grateful to you and deeply appreciative of our collaboration.

I would also like to thank Prof. Dr. Mohammad Reza Farzanegan of the University of Marburg, who kindly agreed to serve as the second supervisor of this doctoral project. At various conferences, he consistently provided helpful comments on my research, and I also look back fondly on our private conversations. Thank you very much!

In the following, I would like to add a few personal words in German.

An erster Stelle danke ich meinen Eltern für all die Liebe und Mühen, die sie für meine Geschwister und mich auf sich genommen haben, und dafür, dass sie bis heute immer für uns da sind. Ich danke meiner Familie und meinen Freunden, ohne die ich heute nicht der Mensch wäre, zu dem ich mit meinen Stärken und Schwächen geworden bin.

Manche Freunde begleiten mein Leben bereits seit der frühen Kindheit, andere sind später dazugekommen; manchmal verliert man sich auf dem Weg aus den Augen und findet sich doch an anderer Stelle wieder. Und manchmal werden Freunde sogar ein Teil der Familie.

Jeder Einzelne von Euch war und ist ein wichtiger Bestandteil meines Lebens, und ich hoffe, dass auch ich überwiegend einen positiven Einfluss auf Euer Leben hatte und weiterhin habe. Ich möchte abschließend nicht verschweigen, dass ich auch stolz auf mich selbst bin. Aber ohne jeden Einzelnen von Euch und die notwendigen Fügungen des Lebens wäre das nie möglich gewesen.

Duke Humfrey's Library, Oxford, 28. April 2026

Notational Remarks

1. Chapter 1 is the *Overview*. Therefore, Chapter 2 is the first research article in this dissertation.

2. Since each article is considered as an independent contribution, the notation varies from article to article. Therefore, we define the symbols in each article individually.

3. Acronyms, especially for variables, are defined and explained in each article. Acronyms of particular importance are also explained in the 'List of Acronyms'.

4. On a few occasions, defined acronyms are spelled out to emphasize the importance at this point or for the sake of stylistic variety.

5. In the German summary, the standard German conventions for punctuation and numerical notation are used.

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1 Overview

International sanctions have become one of the most prominent geopolitical and economic instruments of foreign policy, reaching a level of use unmatched since the end of the Second World War. Their expansion has been particularly pronounced in the post-Cold War era. Governments employ sanctions for a variety of reasons, including the promotion of democratization, as in the case of Venezuela, the prevention or termination of armed conflict, as in Russia's war against Ukraine, or the inducement of policy or regime change, as in Iran and North Korea. Yet, such prominent cases represent only a fraction of the broader sanction landscape. In practice, both the causes and the consequences of sanctions are considerably more complex than simplified narratives suggest and are often associated with unintended, or at times consciously ignored and politically tolerated, side effects. A systematic analysis of the motivations, objectives, and outcomes of international sanctions is therefore crucial for social scientists and policymakers alike.

This doctoral thesis contributes to the literature on international sanctions by examining their effects across different contexts. The five papers that constitute the thesis make use of the Global Sanctions Data Base (GSDB, see Felbermayr et al. 2020; Kirilakha et al. 2021; Syropoulos et al. 2024) and focus on a global perspective covering the period from the 1960s to the 2020s.

The initial section after the *Overview* analyzes how international sanctions affect global migration patterns, specifically emigration from sanctioned countries (Chapter 2). It highlights the inverse relationship between exit (emigration) and voice as central accountability mechanisms in political systems in the context of sanctions. Chapter 3 further explores political accountability in sanctioned countries by developing a nuanced understanding on the relationship between sanctions and regime collapse, with particular attention to coups d'état in both autocratic and democratic regimes. Chapter 4 bridges international economics and international relations investigating the role of personal relationships between political leaders in the context of sanctions. The final two chapters examine the effects of sanctions on state officials, focusing

on public sector corruption (Chapter 5) and governments' compliance with their national constitutions (Chapter 6). The following passage provides summaries of the motivation, empirical approach, and key findings of each chapter.

Chapter 2: International Sanctions and Emigration

In 'International Sanctions and Emigration', co-authored with Jerg Gutmann and Matthias Neuenkirch, we provide the first large-scale quantitative assessment of the impact of international sanctions on cross-border migration from sanctioned countries. While the existing literature has primarily examined whether sanctions trigger political reactions within target countries, such as increased opposition or rally-around-the-flag effects, we shift the focus to a behavioral response: the decision of citizens to leave their country of origin altogether (Grauvogel et al. 2017; Liou et al. 2021, 2023; Eichenberger and Stadelmann 2022; Gold et al. 2025; Grauvogel and Soest 2014; Seitz and Zazzaro 2020). To date, systematic quantitative evidence on this mechanism has been scarce and largely confined to single-country cases, such as Iraq or Haiti (Bossuyt 2000; Connell et al. 2021), or to studies analyzing emigration in the context of violent conflicts (Davenport et al. 2003; Dreher et al. 2011; Moore and Shellman 2004).

Empirically, we employ a panel difference-in-differences (DiD) framework complemented by an event study design. The analysis draws on a dyad-year dataset that merges our information on sanctions with bilateral migration data from the Organisation for Economic Co-operation and Development (OECD 2020) and the Determinants of International Migration (DEMIG 2015) project.

The results indicate that Western multilateral and UN sanctions increase emigration from targeted countries. Event study estimates reveal that migration flows rise progressively over the duration of a sanction episode and deviate markedly from pre-sanction trends, before returning to baseline levels once sanctions are lifted. The effects, particularly those associated with Western multilateral sanctions, are most pronounced in countries with restricted freedom of expression, suggesting that

outward migration may function as an alternative channel of political response when domestic dissent is constrained (Hirschman 1970). In contrast, we find no evidence for heterogeneous effects across gender.

Chapter 3: International Sanctions and Coups d'État

This chapter presents the single-authored study 'International Sanctions and Coups d'État' which constitutes the first systematic investigation of how international sanctions influence the occurrence of coups d'état in sanctioned countries. The chapter develops a theoretical argument and tests it empirically to assess whether sanctions alter the incentives and constraints faced by potential coup perpetrators. Building on foundational contributions in the separate research areas of regime dynamics by Tullock (1974, 1987, 2005) and Wintrobe (1990, 1998, 2012) and coups d'état by Finer (1988), Galetovic and Sanhueza (2000), Gassebner et al. (2016), Leon (2014), and Aidt and Leon (2019), the analysis explores the mechanisms through which sanctions influence coup behavior and accounts for differences between democratic systems and various forms of authoritarian rule. In contrast to earlier research that examined sanctions broadly through the lens of regime survival (Marinov 2005) and so-called 'irregular exits' (Escribà-Folch and Wright 2010), this study directly focuses on coup dynamics and regime-type heterogeneity.

The empirical strategy relies on panel logit estimations with region and year fixed effects, which are conceptually comparable to a DiD approach given the binary nature of the sanction (treatment) variable. To enhance causal inference, the baseline models are complemented by an event-study design and propensity-score weighting. Additional methods are employed to account for the rare occurrence of coups d'état. The analysis is based mainly on target-year data from the GSDB and the Coup Leader Dataset (Eschenauer-Engler and Herre 2023).

It is found that Western sanctions raise the risk of coups, with stronger effects for successful coups, in non-democratic regimes, and during the post-Cold War era. Event study estimates indicate a sharp increase in coup probability in the first year

after the introduction of Western sanctions, highlighting the short-term destabilizing effect of economic coercion.

Chapter 4: Leader Similarity and International Sanctions

The third article 'Leader Similarity and International Sanctions', co-authored with Jerg Gutmann and Matthias Neuenkirch, integrates research on economic sanctions with leader-centered approaches from international relations. The study departs from state-centric explanations of sanction behavior by emphasizing that the personal characteristics of political leaders, such as ideology, educational background, and professional experience, shape foreign policy choices not only domestically but also through interactions between leaders across state (DiLorenzo and Rooney 2025; Foster and Keller 2023; Levy 2023; Zurcher and Murphy 2025). Despite the growing prevalence of sanctions relative to other forms of interstate conflict, and their dramatic expansion since the end of the Cold War, existing sanctions research has largely overlooked the role of individual leaders in the initiation and design of sanction regimes.

We employ panel probit estimations with fixed effects on dyad-year observations. The dataset combines the data on sanctions with measures of leader similarity developed by DiLorenzo and Rooney (2025) based on the LEAD dataset by Ellis et al. (2015).

We find that greater similarity between political leaders substantially lowers the probability that sanctions are imposed. As our theory suggests, this relationship is particularly strong for sanctions decided unilaterally, that is, outside multilateral frameworks such as the United Nations or the European Union. Moreover, the relevance of leader similarity has increased in the Post-Cold War era and is most pronounced for sanctions pursuing political or human rights objectives. The findings further suggest that leader characteristics play a central role in the use of non-trade sanctions, such as arms and military restrictions, as well as in sanction interactions involving autocratic regimes.

Chapter 5: International Sanctions and Corruption

The article, 'International Sanctions and Corruption', also co-authored with Jerg Gutmann and Matthias Neuenkirch, mainly deals with the persistence of behavioral and institutional consequences of international sanctions for a sanctioned country, specifically focusing on public-sector corruption. Given that corrupt practices tend to become deeply entrenched once they are socially accepted, reversing such patterns is notoriously difficult. At the same time, sanctions have increasingly been deployed as an instrument to constrain or discipline corrupt political elites (Moiseienko 2024; Hamilton et al. 2024). Nevertheless, cross-national empirical research on the corruption effects of sanctions remained limited.

Our study contributes by offering such a quantitative assessment of how international sanctions influence public-sector corruption levels. Methodologically, the analysis combines a panel DiD framework with an event study approach. The dataset links sanction data and corruption indicators from the Varieties of Democracy (V-Dem) project (Coppedge et al. 2024).

The results show that sanctions imposed by Western actors and the UN are associated with reductions in corruption in democratic target countries. Event study estimates indicate stable corruption trends prior to sanction imposition and reveal measurable declines beginning three to four years into a sanction episode. These improvements are temporary and dissipate once sanctions are lifted, suggesting that the effects do not persist beyond the treatment period. Finally, the corruption-reducing impact is strongest when sanctions explicitly pursue objectives related to democratization or the protection of human rights.

Chapter 6: International Sanctions and Constitutional Compliance

In the final paper, 'International Sanctions and Constitutional Compliance', Jerg Gutmann, Matthias Neuenkirch, and I examine whether international sanctions induce governments to respect the commitments embedded in their own constitutions. Rather than evaluating sanctions through universal human rights standards alone, the paper

proposes constitutional compliance as an alternative benchmark, interpreting national constitutions as codified social contracts. This perspective is particularly relevant because sanctions are frequently criticized as imposing external or Western norms on sovereign states.

Our study provides an empirical analysis of the relationship between sanctions and constitutional compliance (CC). The empirical strategy combines modern Two-Stage Difference-in-Differences (2SDiD) estimators with a state-of-the-art Local Projection Difference-in-Differences (LP-DiD) event study framework to identify both average and dynamic treatment effects. The dataset merges sanction information from the GSDB with constitutional compliance indicators from the Comparative Constitutional Compliance Database (CCCD), covering 182 countries over the period 1962 to 2022.

The findings reveal strong heterogeneity depending on the political regime of the targeted state. Democracies exposed to sanctions tend to increase compliance with constitutional rules, while autocracies exhibit the opposite response and become less compliant. Event study estimates show no meaningful pre-treatment divergences, while positive effects for democracies emerge gradually after sanction onset and negative effects for autocracies deepen over time. These patterns suggest that sanctions can strengthen adherence to legal commitments only where governments rely on broad electoral support and face public accountability. Further results indicate that positive effects are mainly driven by UN sanctions against democratic countries, whereas Western and UN sanctions imposed on autocracies reduce constitutional compliance. Measures enacted by China or Russia show no systematic impact. In democratic targets, improvements are concentrated in areas such as basic human rights and political rights, whereas sanctioned autocracies experience deterioration across several constitutional domains. Overall, the paper concludes that sanctions are less effective where stronger legal discipline would be most needed, i.e. authoritarian regimes.

2 International Sanctions and Emigration

"International Sanctions and Emigration"

with Prof. Dr. Matthias Neuenkirch and Dr. Jerg Gutmann

Published in the *Journal of Economic Behavior & Organization*[1]

Abstract

In this first statistical analysis of how international sanctions affect international migration, we apply two estimation strategies, a panel difference-in-differences model and an event study approach. Our dataset covers 79,791 dyad-year observations, reflecting migration flows from 157 origin countries to 32 industrialized destination countries between 1961 and 2018. We find that UN and joint EU-US sanctions increase emigration from target countries by around 20 percent. Our event study results for joint EU-US sanctions imply a gradual increase in emigration throughout a sanction episode. The impact of UN sanctions on international migration is smaller and less persistent. Moreover, the effects are driven by target countries with limited freedom of political expression, where emigration substitutes for the costly voicing of dissent. Finally, there appear to be no systematic gender differences in the migration effect of sanctions.

1. We would like to thank Elsevier B.V. for publishing the article in this dissertation.

2.1 Introduction

Research on the effects of international sanctions, thus far, has focused on either macro-level outcomes for society[2] or on political leaders' policy choices and their survival in office.[3] At the same time, researchers have paid much less attention to how sanctions affect citizens' decision-making. Some research argues that sanction threats and imposed sanctions incentivize anti-government protest (Grauvogel et al. 2017; Liou et al. 2021), which in turn might increase the likelihood that the government complies with the sender's political demands (Attia et al. 2020) and with those of non-violent protest movements (Liou et al. 2023). Other studies, however, emphasize that sanctions would cause a rally-around-the-flag effect that strengthens the regime's popularity and, thus, its grip on the target country (Eichenberger and Stadelmann 2022; Gold et al. 2025; Grauvogel and Soest 2014; Seitz and Zazzaro 2020).[4]

Here, we conduct the first statistical analysis of how sanctions cause international migrant flows originating from target countries.[5] Whereas previous studies have focused on whether citizens react to sanctions by voicing criticism or support of the government, we ask whether some citizens respond by exiting the polity altogether. There is only limited qualitative evidence for an emigration-inducing effect of sanctions. Bossuyt (2000), for example, reports that emigration from Iraq skyrocketed under sanctions (see Connell et al. 2021, for a similar discussion of the case of Haiti).[6]

2. Sanctions, for example, have adverse effects on the economy (Gutmann, Neuenkirch, and Neumeier 2023a; Hatipoglu and Peksen 2018; Neuenkirch and Neumeier 2015; Peksen and Son 2015; Shchepeleva et al. 2024), increase economic inequality and poverty (Afesorgbor and Mahadevan 2016; Moteng et al. 2023; Neuenkirch and Neumeier 2016), harm the health of target populations – especially that of their most vulnerable members (Gutmann et al. 2021), and reduce international trade (Crozet and Hinz 2020; Felbermayr et al. 2020; Gutmann, Neuenkirch, and Neumeier 2024) and capital flows (Besedeš et al. 2017; Biglaiser and Lektzian 2011; Mirkina 2018).

3. When facing sanctions, political leaders are more likely to violate basic, political, and civil rights (Adam and Tsarsitalidou 2019; Gutmann, Neuenkirch, and Neumeier 2020; Peksen and Drury 2009; Wood 2008) as well as property rights (Lee et al. 2023), while sanctions reduce those leaders' likelihood of staying in office (Marinov 2005).

4. Frye (2019), in contrast, does not find a rally-around-the-flag effect in a survey experiment conducted in the context of sanctions imposed after Russia's illegal annexation of Crimea.

5. As it is common in the social science literature, we use the terms origin and destination country to describe migration flows, and target and sender country to describe the imposition of sanctions.

6. Connell et al. (2021) study the effect of a potential sender of sanctions being host to a large diaspora from a potential target country. They demonstrate that diaspora presence is associated with political opposition to imposing sanctions and argue that this is due to policymakers' fear of migration pressure from the target state.

Özdamar and Shahin (2021) identify the possible migration effect of sanctions as a central open research question in the literature on the effects of sanctions and, so far, no empirical study has addressed this question. Our study also contributes to a literature that has identified conflict events as a major driver of emigration (Davenport et al. 2003; Dreher et al. 2011; Moore and Shellman 2004). This literature has ignored international sanctions as a potentially important political shock responsible for emigration decisions.

In our empirical analysis, we apply two estimation strategies: a panel difference-in-differences (DiD) model and an event study approach. Our dataset combines data from the Global Sanctions Data Base (GSDB, see Felbermayr et al. 2020) with migration data from the OECD (2020) and the Determinants of International Migration (DEMIG 2015) dataset. It covers 79,791 dyad-year observations, reflecting migration flows from 157 origin countries to 32 destination countries between 1961 and 2018. We distinguish the effects of (i) UN, (ii) joint EU-US (Western multilateral), (iii) EU unilateral, (iv) US unilateral, and (v) "non-Western" (i.e., imposed by China or Russia) sanctions.[7]

Our findings suggest that UN and Western multilateral sanctions have a significant positive effect on migration. Migration flows from the target country increase by 17–18% under UN sanctions and by 22–24% under Western multilateral sanctions. Our event study results for Western multilateral sanctions show a gradual increase in emigration throughout a sanction episode with a peak effect of 80–86% for long-lasting sanctions (relative to the final year before sanction imposition). The impact of UN sanctions on international migration is smaller than that of Western multilateral sanctions (peak effect: 30–31%) and less persistent. Our findings can be interpreted as causal because the measured increase in emigration marks a significant deviation from the pre-trend. In addition, migrant flows return to their pre-sanction level once sanctions are lifted. The results (in particular those for Western multilateral sanctions) are driven by target countries with limited freedom of political expression. This is consistent with emigration serving as a substitute for voicing dissent, especially where

7. Chinese and Russian sanctions are merged into a single dummy variable due to the low number of dyad-years with Chinese sanctions in place (653, less than 1% of all observations in the dataset).

the latter is costly. Finally, our results indicate no gender differences in the effects of sanctions on migration.

Section 2.2 outlines our theoretical arguments and derives testable hypotheses. Section 2.3 explains the estimation approach and the data used to test our hypotheses. Section 2.4 presents the results of our panel difference-in-differences and event study estimations and discusses their congruence with our theoretical predictions. Section 2.5 concludes and outlines the need for further research.

2.2 Theory

In his seminal book on "Exit, Voice, and Loyalty", Hirschman (1970) contrasts two central accountability mechanisms the members of an economic, political, or social organization can rely on vis-à-vis the organization's leadership. He argues that economists, with their trust in the virtues of competition, have disregarded the possible contribution of voice in ensuring the accountability of leaders, just as political scientists, with their focus on protest and voting, have neglected the role of exit. Another reason for economists' and political scientists' different analytical lenses is that exit is generally a private and often even a covert decision, whereas using voice is a contribution to a public good. Not surprisingly, political scientists are less interested in decisions that are typically not politically motivated and economists doubt citizens' ability to overcome the collective action problem of voice in most circumstances. Here, we argue that political scientists have also largely disregarded the possible role of exit in the discussion of how citizens deal with the economic and political pressure created by international sanctions.[8] Accordingly, emigration is one way for citizens to respond to the negative consequences of sanctions and possibly their government's inability or unwillingness to mitigate them. However, this does not mean that citizens migrate to punish the government. It is rather the individually rational choice of exit, which – if taken independently by many individuals – turns out to be collectively costly to the government and potentially threatens its survival in office.

8. Studies on how capital flows respond to sanctions (Besedeš et al. 2017; Biglaiser and Lektzian 2011; Mirkina 2018) are a noteworthy exception.

Other researchers who described the link between sanctions and migration have predicted the opposite of what follows from Hirschman's (1970) theory. Afesorgbor (2019) argues that sanctions can reduce the flow of migrants, specifically between senders and targets by adversely affecting these countries' relationships.[9] Weiner (1992) makes the related argument that sanctions are a key instrument to curtail unwanted migrant flows towards the sanction-sending country (see also Portela and Charron 2023). Accordingly, emigration can be limited by imposing sanctions on states from which major migrant flows originate, thereby, putting pressure on their governments to reduce these flows. Weiner (1992) even refers to the unusual case where Palestinians as a third party (i.e., neither the origin nor the destination country of migrants) threatened sanctions against carriers who brought Soviet Jews to Israel. A more recent example is US President Trump's threat to impose tariffs on all Mexican goods if Mexico did not curb the flow of migrants to the US southern border. Before sanctions were imposed, Mexico agreed to take adequate measures (Hufbauer and Jung 2020). Sanctions do not need to be intended to reduce emigration from the target country. If they successfully target some of the causes of migration, such as conflict and human rights violations, sanctions can curb migration flows as a side effect. The imposition of sanctions might lead to reduced emigration even before human rights violations and conflicts end, as long as citizens believe that sanctions will improve living conditions in the target country in the foreseeable future. Finally, sanctions may lead to less emigration by depriving citizens of the financial resources needed for international migration.

Summing up our theoretical considerations, we arrive at the following set of opposing hypotheses:

Hypothesis 1a. *Sanctions increase emigration from the target country.*

Hypothesis 1b. *Sanctions curtail emigration from the target country.*

9. However, Cucu and Panon (2023) provide empirical evidence that asylum recognition rates are higher between countries with worse diplomatic relations, which would incentivize migration.

If the adverse conditions created by sanctions lead to emigration, it can be expected that not all members of society and not all societies are equally affected. It can, therefore, not be assumed that the development of GDP per capita under sanctions is a precise metric of the hardship caused for the target population. The economic consequences of sanctions will, for example, be concentrated in particular sectors and occupations, where jobs might be lost and real wages might fall. And also only parts of society are vulnerable to a lack of access to critical imported consumption goods, such as medicine. Therefore, the hardship caused by sanctions, which may motivate emigration, cannot be broken down into a small set of well-defined and empirically operationalizable transmission channels.

Previous literature on the health and employment effects of sanctions has demonstrated disproportionate adverse effects of sanctions on women (Demir and Tabrizy 2022; Gutmann et al. 2021).[10] In addition, previous literature on gender differences in migration has demonstrated that differing migration decisions of men and women – both on the individual level and when aggregated to the country level – can be traced back to systematic gender-specific incentives (Gutmann, Marchal, et al. 2023; Neumayer and Plümper 2021; Ruyssen and Salomone 2018). This motivates our second hypothesis.

Hypothesis 2. *The positive migration effect of sanctions on women is larger than that on men.*

Not only are different societies and members of society differently affected by sanctions, but their responses to the hardship caused may also differ systematically. If the argument by Hirschman (1970) and others that voice and exit are substitutes is taken seriously, the attractiveness of exit should depend on the costliness of using voice. Exit would then be chosen primarily where the government's policy raises the cost of voice. Hence, our third hypothesis is the following:

10. The evidence on how sanctions affect women's rights, however, is ambiguous (Drury and Peksen 2014; Gutmann, Neuenkirch, and Neumeier 2020).

Hypothesis 3. *The positive migration effect of sanctions is larger in countries with less freedom of political expression.*

Of course, Hypotheses 2 and 3 are only plausible if Hypothesis 1a is supported by the data. They are, thus, formulated conditional on sanctions having a positive effect on emigration from the target country.

Hirschman (1978) surveys the historical discussion on and case evidence for the effect of exit by either capital or citizens on the remaining society. Exit from a state by dissatisfied citizens is not uncommon. But since it can be costly for society if too many citizens leave too quickly, it must be considered that not all polities allow for an unrestricted exit. The Berlin Wall, for example, was built in 1961 exactly for that reason – to stop the exodus from the German Democratic Republic to the West. To test our hypotheses, particularly Hypothesis 3, existing legal restrictions on citizens' freedom of international movement must be accounted for, as they may significantly impede exit in response to sanctions. Finally, we also evaluate empirically whether countries targeted with sanctions increase restrictions on foreign migration (see Section 2.5).

2.3 Estimation Strategy and Data

2.3.1 Estimation Strategy

As migration depends on a variety of economic, political, social, and individual push and pull factors, it is not trivial to estimate the effect of economic sanctions on emigration (e.g., Gibson and McKenzie 2011). Moreover, sanctions are typically imposed on politically unstable and economically vulnerable countries (Gutmann et al. 2021; Jing et al. 2003). Thus, causal effects have to be carefully identified.

Following Gutmann, Neuenkirch, and Neumeier (2023a), we apply two estimation strategies, a standard *panel DiD* model as well an *event study* approach. The panel DiD model can be expressed as follows:

$$y_{i,j,t} = \sum_{s=1}^{5} \beta^{s}_{sanc} D^{s}_{sanc,i,t} + \gamma_1 X^{pol}_{i,t} + \gamma_2 X^{econ}_{i,t-1} + \alpha_{i,j} + \tau_{j,t} + \epsilon_{i,j,t} \tag{1}$$

The dependent variable $y_{i,j,t}$ is the log-transformed absolute number of people who migrate from country i to country j in year t.[11] Our level of observation is the directed dyad- (or directed country pair-) year level. The vector of dummy variables $D^{s}_{sanc,i,t}$ distinguishes between (i) UN, (ii) joint EU-US (Western multilateral)[12], (iii) EU unilateral, (iv) US unilateral, and (v) non-Western sanctions (i.e., imposed by China or Russia). These are our key variables of interest and take the value 1 if sanctions are imposed against a country i in year t, and 0 otherwise. Sanctions enacted by the UNSC are not counted as EU, US, or non-Western sanctions.[13]

We implement two sets of fixed effects to account for various unobserved factors influencing international migrant flows. First, dyad (or country pair) fixed effects $\alpha_{i,j}$ absorb various standard control variables, such as distance, a common border, shared languages, and time-invariant cultural and genetic proximity. In addition, dyad fixed effects nest the less granular origin and destination fixed effects. Thus, they account for all time-invariant origin and destination country characteristics. Second, destination-year fixed effects $\tau_{j,t}$ capture the annual political, macroeconomic, and social conditions within destination countries, often referred to as "pull factors". This means that, for example, migration policy regimes in destination countries are fully accounted for, even if they change over time. These fixed effects also nest the less granular year fixed effects, which capture all global time trends in migration and the global political environment. The inclusion of origin-year fixed effects, however, is not feasible if one is interested in measuring the total emigration effect of sanctions, as all sanctions indicators would be absorbed.[14] Instead, we account for potentially confounding time-varying origin country characteristics – in line with the migration economics litera-

11. It is common in the migration literature to measure the dependent variable as a 'migration rate'. Log-transformation and dyad fixed effects imply that we also study the effect of sanctions on the relative deviation of migration flows from their dyad-specific conditional means.

12. We use the term "multilateral" here to refer to sanctions imposed by both the EU and the US, but this does not imply that these sanctions were coordinated between the EU and the US.

13. The three types of Western sanctions are by construction disjunctive. However, Western and non-Western sanctions can coincide.

14. Origin-year fixed effects can be included if one is only interested in sanction-induced migrant flows to specific countries, such as flows to sanction senders. However, such estimates must be interpreted carefully, as migration flows to countries other than the senders and caused by the same sanctions would be part of the counterfactual migration trend. We estimate such a model as an extension in Section 2.4.2.

ture and the literature on the effects of international sanctions (e.g., Beine et al. 2019; Giménez-Gómez et al. 2019; Gutmann, Neuenkirch, and Neumeier 2023a) – by including a set of economic and political control variables. $X_{i,t}^{pol}$ represents political and $X_{i,t-1}^{econ}$ represents one-year lagged economic control variables for the origin country, described in detail in Section 2.3.2. $\epsilon_{i,j,t}$ is an idiosyncratic error term.

Our second estimation strategy, the event study approach, is used to compare migration during the treatment period with the trends in migration before and after the imposition of sanctions (Dai et al. 2021; Gutmann, Neuenkirch, and Neumeier 2023a; Schmidheiny and Siegloch 2023). We examine the pre- and post-trend in migration in the three years before and after each sanction episode. By comparing these observations to non-sanctioned dyad-years, we can assess whether migration flows systematically increase before sanctions are imposed. This enables us to separate the impact of sanctions from the factors that led to their imposition. An additional benefit of the event study design is that it allows us to analyze how the treatment effect evolves throughout the sanction episode, rather than solely estimating an average treatment effect. The event study specification can be formalized as follows:

$$y_{i,j,t} = \beta_{no} D_{no,i,t} + \sum_{l=2}^{3} \beta_{pre,-l} D_{pre,i,t-l} + \sum_{l=1}^{11+} \beta_{sanc,l} D_{sanc,i,tl} + \sum_{l=1}^{3} \beta_{post,+l} D_{post,i,t+l} \quad (2)$$
$$+ \sum_{s=1}^{4} \beta_{sanc}^{s} D_{sanc,i,t}^{s} + \gamma_1 X_{i,t}^{pol} + \gamma_2 X_{i,t-1}^{econ} + \alpha_{i,j} + \tau_{j,t} + \epsilon_{i,j,t}$$

The control variables ($X_{i,t}^{pol}$ and $X_{i,t-1}^{econ}$), fixed effects ($\alpha_{i,j}$ and $\tau_{j,t}$), and the idiosyncratic error term ($\epsilon_{i,j,t}$) are defined as in Eq. (1). The event study indicators, denoted as $D_{sanc,i,tl}$, are binary variables that take the value 1 if either a UN or a Western multilateral (joint EU-US) sanction episode targeting country i was active during the l^{th} consecutive year. To capture the effects of sanctions over time, we employ individual dummy variables for each of the first ten years in a sanction episode ($t1$ to $t10$). Because longer-lasting sanctions are rare, we combine all sanction-years after the tenth year of an episode in one dummy variable ($t11+$).[15] $D_{pre,i,j,t-l}$ and $D_{post,i,j,t+l}$ are five

15. The effects of longer-lasting sanctions over time are disentangled further in Section 2.4.2.

dummy variables that identify the years three and two preceding and each of the three years following a sanction episode. These variables enable us to evaluate the trends in emigration from a sanctioned country before sanctions are imposed and after they are lifted. $D^{s}_{sanc,i,t}$ is a vector of controls for other types of sanctions. In event studies on the effect of UN sanctions, the vector contains joint EU-US, EU only, US only, and non-Western sanctions. In event studies on the effect of Western multilateral sanctions, the vector contains UN, EU only, US only, and non-Western sanctions. Finally, $D_{no,i,t}$ is a dummy variable identifying (i.e., coded 1 for) all observations where neither the sanction variables of interest (i.e., dummy variables for either UN sanctions or Western multilateral sanctions) nor the corresponding pre- and post-trends are coded 1. Hence, the estimated effects of sanctions (over time) and the pre-/post-trend coefficients are to be interpreted as deviations from the final year before the imposition of sanctions ($t-1$), that is, the year for which we have omitted the pre-trend dummy from Eq. (2) (cf., Callaway and Sant'Anna 2021).

Eqs. (1) and (2) are estimated using ordinary least squares and the standard errors are clustered at the dyad level.

2.3.2 Data

Our final dataset contains 79,791 observations for which we have complete data on migration, sanctions, and all control variables. It covers migration flows from 157 origin countries to 32 destination countries (31 of which are OECD members), corresponding to a total of 4,596 dyads, between 1961 and 2018 (see Table A.1 for a list of countries). Our dependent variables are based on the absolute number of migrants from country i to country j in year t. For our baseline specifications, we also distinguish male from female migrants to evaluate gender differences in the effects of sanctions.[16] Our baseline specification studies migration at the combined extensive and intensive margin

16. There is no information on other socio-demographic characteristics of interest, such as education or marital status.

and relies on a $log(y_{i,j,t}+1)$-transformation.[17] In addition, we assess the robustness of our results by estimating a model of only the intensive margin using a $log(y_{i,j,t})$-transformation.[18] Migration data is taken from the DEMIG (2015) and OECD (2020) databases.[19]

Sanctions data is obtained from the *Global Sanctions Data Base* (GSDB, see Felbermayr et al. 2020; Kirilakha et al. 2021; Syropoulos et al. 2024). Our empirical analysis focuses on the most active senders: the UN, the US, the EU, Russia, and China. In total, our dataset covers 3,932 dyad-years with UN sanctions in place, 4,480 with EU-US joint (or Western multilateral) sanctions, 2,034 with EU unilateral sanctions, 8,972 with US unilateral sanctions, and 5,561 with non-Western (i.e., Chinese or Russian) sanctions. UN and Western multilateral sanctions naturally occur less frequently, since more parties have to agree on their imposition – and for UN sanctions unanimous consent of five veto powers is required. Moreover, Wood (2008) and Hufbauer et al. (2009) document that UN sanctions are, on average, less comprehensive than US unilateral or multilateral sanctions.[20] Finally, the US is the most active sender of international sanctions. On average, UN sanctions (/joint EU-US sanctions) are newly introduced after US unilateral sanctions were already in place for 2.9 (1.3) years. However, only 50

17. We also test for the robustness of our results at the combined extensive and intensive margin by using an inverse hyperbolic sine transformation: $log(y_{i,j,t}+\sqrt{y_{i,j,t}^2+1})$ (see Aihounton and Henningsen 2020; Bartlett 1947; Chen and Roth 2024, for alternatives to log-transformation in regression analysis).

18. Depending on the dependent variable (total migration, male migration, or female migration), this robustness test is based on up to 11,000 observations less than the baseline specification.

19. Many migrants, of course, do not move to these 32 destination countries. However, nonindustrialized countries lacked the statistical capacity to record migrant inflows by origin country over many decades. Domestic migration is also not recorded and the data does not allow us to distinguish types of migration, such as legal vs illegal or skilled vs unskilled migration. Finally, this dataset does not cover immigration into sanctioned countries. Using the World Bank's net migration dataset, we explore this issue as an extension. The results for net migration (available on request) are consistent with our findings for emigration from sanctioned countries.

20. The GSDB does not measure the severity of sanctions, except for trade sanctions. We also explore the difference between types of sanctions. However, different types (arms, financial, military, trade, travel, and others) often coincide (i.e., sanction episodes are typically of more than one type), which can cause collinearity problems given that there is only a limited number of sanctions in each category. For instance, most sanctioned observations (UN: 2,981; multilateral: 3,825) entail financial sanctions. To reach conceptually clear and empirically robust results, we focus in an extension on one main subcategory of sanctions: those including trade or financial sanctions, which should be most prone to cause economic hardship. Most sanctions include one of the two types (UN: 2,981; multilateral: 3,992) and only a small fraction do not (UN: 951; multilateral: 488). Coefficient estimates for trade/financial sanctions are consistent with the results below for sanctions in general. Estimates for non-trade/non-financial sanctions have to be interpreted cautiously and are available on request.

(25.4) percent of newly imposed UN sanctions (/joint EU-US sanctions) were preceded by US sanctions.[21]

To account for the economic causes of migration in the origin countries, we control for real GDP per capita (in logs and lagged by one year due to potential reverse causality). Political and social causes of migration are incorporated through the *Polity2* democracy index (Marshall and Gurr 2020) and the *Human Rights Protection Score* by Fariss (2019). Considering these control variables should reduce omitted variable bias. But since sanctions also affect income, human rights, and democracy, we may at the same time introduce bias by controlling for mediating variables. We evaluate the robustness of our results to varying the model specification with regard to control variables in Section 2.4.2. Moreover, we include the binary variable *Freedom of Movement*, based on version 14 of the V-Dem dataset (Coppedge et al. 2024).[22] To test our third hypothesis, we add a variable measuring freedom of political expression (*Political Expression*) and interact it with the indicators for UN sanctions and Western multilateral sanctions.[23] Finally, we control for the occurrence of conflicts and wars based on data by Gleditsch et al. (2002) and Davies et al. (2022) and distinguish between two levels of intensity (minor and war) and three different scopes of conflict (interstate, intrastate without intervention, and intrastate with intervention).

Table A.2 provides definitions and data sources of all variables. Tables A.3 and A.4 show descriptive statistics. In general, migration flows are larger if an origin country is subject to sanctions. However, sanctioned countries, on average, have a lower GDP per capita, are less democratic, show more infringements of human rights, provide less freedom of political expression, and experience minor conflicts and wars at a much higher frequency. These descriptive statistics underscore the importance of separating

21. For UN sanctions, the corresponding figures for other senders are as follows. EU: 0.1 years/7.4 percent of cases, Russia: 0.1 years/1.1 percent, China: 0 years/0 percent. Joint EU-US sanctions are, on average, preceded by 1.6 years of EU unilateral sanctions, but only in 25.9 percent of the cases.

22. The variable takes the value 0 if there is no respect for freedom of foreign movement and 1 if freedom of foreign travel and emigration is at least weakly respected. Lagging all political control variables, all control variables, or none of the control variables by one year leaves our results virtually unchanged (estimates are available on request).

23. The freedom of political expression variable is based on the standardized first principal component of the following variables from the V-Dem dataset: freedom of discussion for men, freedom of discussion for women, freedom of academic & cultural expression, and freedom of peaceful assembly.

the treatment effect from the selection effect, as the political, social, and economic environment is considerably worse in sanctioned countries.

2.4 Empirical Results

2.4.1 Baseline Results

Average Treatment Effects. Table 2.1 shows the results of the DiD estimations for different dependent variables at the combined extensive and intensive margin, that is, after a $log + 1$-transformation. UN and Western multilateral sanctions have a significant positive effect on migration, confirming Hypothesis 1a. On average, UN sanctions increase migration flows from the target country by roughly 16.8–17.7%, whereas the effect of joint EU-US sanctions amounts to a 22.4–23.8% increase in migration. Concerning gender differences, we find a slightly, but not significantly, larger effect of Western multilateral sanctions on female migration (+23.8%) as compared to male migration (+22.4%).[24] In contrast, non-Western sanctions only significantly impact male migration (+7.6%), but not total or female migration. Hence, we find no empirical support for Hypothesis 2. Finally, unilateral EU or US sanctions do not significantly affect migration flows.

24. The baseline estimates for UN sanctions and joint EU-US sanctions remain virtually unchanged if we interact the sanction variables with the indicators for major conflicts (estimates are available on request). Hence, the effects of sanctions on migration are not driven by sanction episodes that coincide with large-scale conflicts.

Table 2.1: Baseline Results (Log + 1)

	(1) Total Migration	(2) Male Migration	(3) Female Migration
Sanctions			
… UN	0.177***	0.169***	0.168***
	(0.052)	(0.053)	(0.050)
… Multilateral (EU-US)	0.228***	0.224***	0.238***
	(0.042)	(0.042)	(0.043)
… EU only	0.053	0.050	0.052
	(0.035)	(0.036)	(0.035)
… US only	–0.005	0.004	–0.009
	(0.025)	(0.025)	(0.024)
… Non-Western	0.034	0.076***	0.013
	(0.028)	(0.028)	(0.027)
lagged *log*(GDP pc)	0.304***	0.295***	0.318***
	(0.070)	(0.069)	(0.072)
Polity2	0.009***	0.006*	0.010***
	(0.003)	(0.003)	(0.003)
Human Rights	–0.123***	–0.116***	–0.109***
	(0.016)	(0.016)	(0.016)
Freedom of Movement	–0.031	–0.043	–0.031
	(0.055)	(0.056)	(0.053)
Interstate Conflicts			
… Minor	0.020	0.041	–0.010
	(0.063)	(0.063)	(0.066)
… Major	–0.203*	–0.209*	–0.243***
	(0.110)	(0.115)	(0.094)
Internal Conflicts w/o Intervention			
… Minor	0.024	0.027	0.037
	(0.023)	(0.024)	(0.023)
… Major	–0.010	0.028	–0.025
	(0.038)	(0.039)	(0.038)
Internal Conflicts w/ Intervention			
… Minor	0.007	0.036	0.001
	(0.045)	(0.043)	(0.043)
… Major	0.417***	0.460***	0.396***
	(0.077)	(0.078)	(0.076)
Observations	79,791	79,791	79,791
R^2	0.940	0.930	0.938
Within-R^2	0.020	0.018	0.020

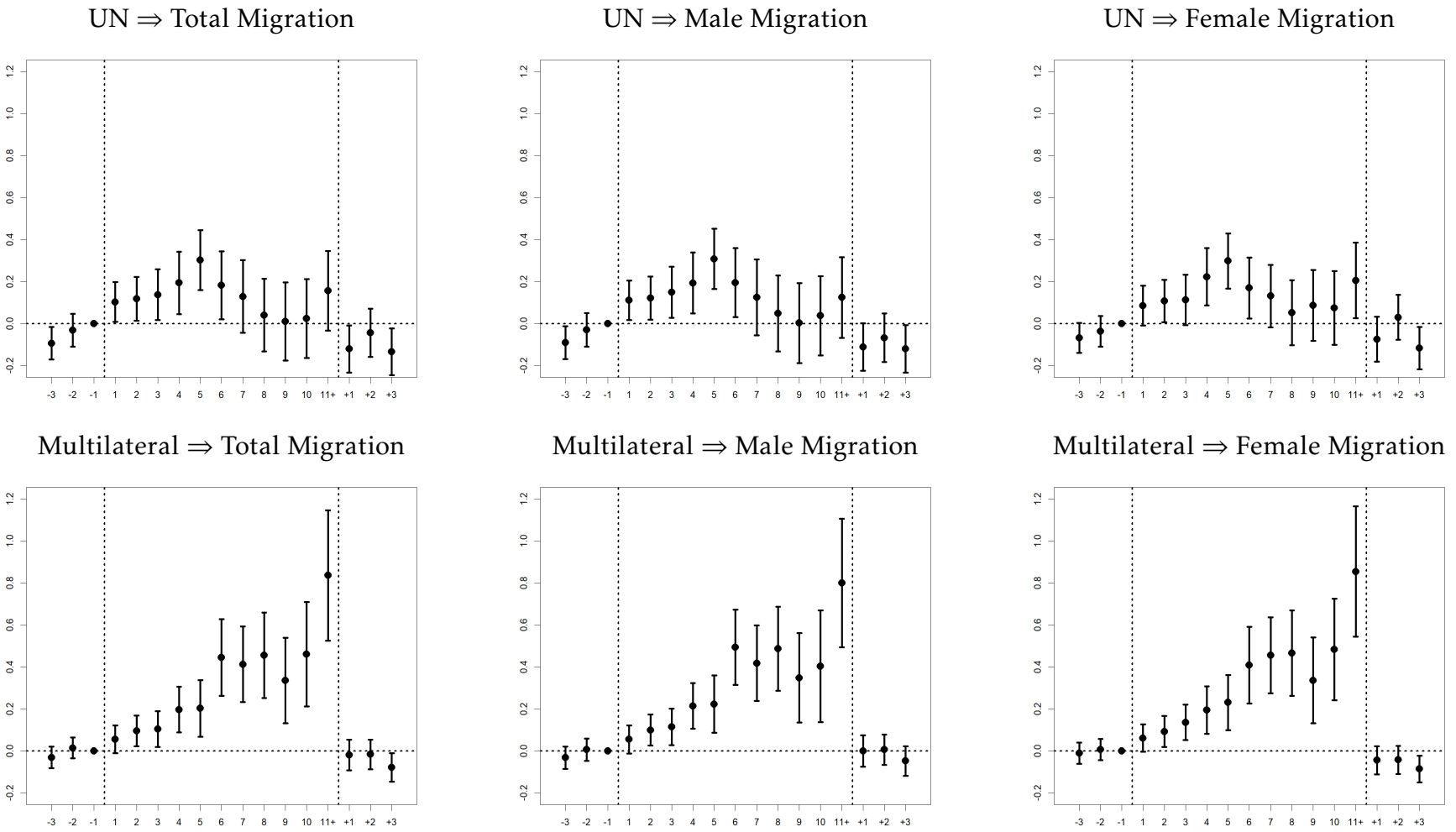
Notes: Coefficient estimates of Eq. (1) with different dependent variables. Standard errors in parentheses are clustered at the dyad level. Models include dyad fixed effects and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.

Turning to the control variables, we find positive coefficients for real GDP per capita and democracy, indicating more migration from economically well-developed democracies to industrialized countries. Moreover, human rights infringements (indicated by a negative coefficient) and major internal conflicts with international intervention lead to more emigration, whereas major interstate conflicts appear to curtail migration flows. Using the coefficient estimates for major internal conflicts with international intervention (39.6–46.0%) as a point of reference, we find that the effects of UN and Western multilateral sanctions are about half the size of large-scale civil wars. In conclusion, the migration effects of these sanctions are quantitatively relevant.

We explore the robustness of our findings in the Appendix by repeating the estimations at the combined extensive and intensive margin using an inverse hyperbolic sine transformation. The corresponding results in Table A.5 are virtually identical to the baseline estimates. Next, we study migration at the intensive margin and discard all observations without migration flows. The results in Table A.6 are qualitatively very similar to those in Table 2.1. If at all, we find larger point estimates at the intensive margin with 20.1–22.6% for UN sanctions and 24.3–26.2% for Western multilateral sanctions. In both cases, the effect on female migration is slightly (but not significantly) larger than that on male migration. The effect of non-Western sanctions on male migration is no longer significant. Hence, the intensive margin estimations further support Hypothesis 1a.

Effects of Sanctions over Time. Figure 2.1 shows the point estimates and the corresponding 95% confidence bands for UN sanctions (upper panel) and Western multilateral sanctions (lower panel) during the three years before a sanction episode (−3, −2, −1), throughout a sanction episode (1, 2, …, 11+), and for the three years after sanctions are lifted (+1, +2, +3). Following Callaway and Sant'Anna (2021), the effect in the year before the implementation of sanctions (−1) is normalized to 0. Hence, the estimated effects of sanctions over time and the pre-/post-trend have to be interpreted relative to the final year before sanctions are implemented.

Figure 2.1: Effects of UN and Western Multilateral Sanctions Over Time (Log + 1)



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 dyad level. Models include control variables (other sanction indicators, lagged *log*(GDP pc), Polity2, Human Rights, Freedom of Movement, and six conflict indicators), dyad fixed effects, and destination-year fixed effects. 95% confidence bands are indicated by whiskers.

There are no significant pre-trends observable before the imposition of Western multilateral sanctions. In addition, migration flows return to their pre-treatment (i.e., −1) levels once sanctions are lifted, with even a slight reduction in migration after three years. In the case of UN sanctions, we observe slow upward pre-trends and some evidence for a decline in migration relative to pre-treatment levels once sanctions are lifted. However, the effects estimated in the treatment period mark a clear deviation from any pre-trend, which supports a causal interpretation of the results.

Mirroring the results in Table 2.1, the impact of UN sanctions on international migration is smaller than that of Western multilateral sanctions. Nevertheless, the effect is statistically significant during the first six years of a sanction episode (for female migration also in the case of long-lasting sanctions, but not in year 1). The estimated effect reaches its peak of 29.8–30.9% in year 5 and declines thereafter. Western multilateral sanctions lead to a gradual increase in migrant flows throughout a sanction episode (with only the first year not being significant). The effects are particularly pronounced for very long-lasting sanctions (80.0–85.5%), but they also reach levels of 46.0–49.4% during the first ten years. While the economic effects of sanctions appear to be strongest in the first years of a sanctions episode (Gutmann, Neuenkirch, and Neumeier 2023a), it is plausible that the effect on migration builds up over the years. Emigration is a costly strategic decision and is often based more on expectations regarding future living conditions than on the status quo. Doxey (1996), for example, describes the increase in white Rhodesian emigration after their initial false expectation that sanctions against their country would not be long-lasting. Finally, we do not detect systematic gender differences in both panels of Figure 2.1.[25]

We explore the robustness of our results and repeat the event study estimations while excluding origin countries that were never subject to UN sanctions or Western multilateral sanctions. Arguably, the social, political, and economic situation in coun-

25. Figure A.1 shows the corresponding event study plots for up to 20 years of a UN or Western multilateral sanction episode. The effects of Western multilateral sanctions increase up until their peak in year 18/19 of a sanction episode. The impact of UN sanctions on migration peaks a second time in year 16 and is particularly pronounced for very long-lasting sanctions. However, the estimates beyond year 10 should be interpreted with caution, as they are based on few observations.

tries that were subject to sanctions at some point in time is more comparable to the situation of countries in the treatment group. This yields a more conservative counterfactual, but comes at the cost of reduced estimation efficiency. Figure A.2 shows the results. Using the restricted control group, we observe slightly larger peak effects of UN sanctions in year 5 (32.6–34.4%) compared to the baseline results in Figure 2.1. For Western multilateral sanctions, the peak effects (39.0%–43.8%) and the effects of long-lasting sanctions (67.1%–69.5%) are smaller in the robustness test. In general, our findings that Western multilateral sanctions trigger more emigration than UN sanctions and that there are no systematic gender differences in emigration due to sanctions are both supported by this robustness test.

2.4.2 Robustness Tests and Extensions

Robustness to Mediating Variables. Our baseline specification is conservative in that it controls for lagged GDP per capita, human rights, and democracy. Indirect effects of sanctions on migration via one of these channels are, therefore, not accounted for, while the risk of omitted variable bias from these migration determinants is reduced. We evaluate the robustness of our results towards considering the total (direct plus indirect) effects of sanctions on migration by re-estimating our baseline specification from Column (1) of Table 2.1 and leaving out one of the three control variables at a time.

The results in Table A.7 show that excluding the indicator for democracy (Column 4) leaves the results virtually unchanged. Excluding income per capita (Column 2) slightly reduces the point estimates for UN and multilateral sanctions. This is in line with the positive estimate for lagged GDP per capita in Column (1) and the detrimental effect of sanctions on income (Neuenkirch and Neumeier 2015; Gutmann, Neuenkirch, and Neumeier 2023a). Most striking is the change in coefficient estimates (also for conflicts) when excluding the indicator for human rights (Column 3). This suggests that the effects of sanctions on migration are possibly underestimated if we control for human rights. Nevertheless, this extension also underscores the robustness

of our results to different model specifications.

Robustness to Sanctions Objectives. The GSDB records nine types of objectives or reasons for imposing sanctions (democracy, human rights, destabilize regime, policy change, prevent war, end war, territorial conflict, terrorism, and others) based on information from official documents. Table A.8 lists the frequency of observations for each sanction objective and the different senders. It reveals some interesting patterns. Not surprisingly, ending wars is the most common objective of UN sanctions, followed by preventing war and human rights violations. Multilateral sanctions are particularly often justified with the target country's human rights situation and with supporting democracy. US and non-Western sanctions are more likely than other sanctions to aim at policy changes.

To test whether the measured sanction-induced migration is driven by sanctions imposed with a particular objective, we repeat the baseline estimations while excluding sanction cases with one objective at a time.[26] If excluding sanctions with a particular objective would fundamentally alter our estimates, this could hint at an omitted variable bias not accounted for by our fixed effects and control variables. Table A.9 shows the results of this jackknife-style robustness test.

A couple of findings seem worth highlighting. First, the effect of UN sanctions and Western multilateral sanctions remains significant throughout all specifications (and highly significant in most of them). The only exception is found when excluding sanctions that aim at ending war, as the significance of the estimates for UN sanctions is less pronounced in this case. Second, we do not find consistently significant results for other sanction senders.[27]

26. Note that sanction episodes can have multiple objectives.

27. Since this robustness test is based on nine regressions with partially overlapping samples, we refrain from interpreting estimates that are not consistently significant.

Effects of Sanctions Conditional on Income Groups. As another extension, we analyze the effects of sanctions across different country-income groups.[28] We rely on the World Bank classification and merge low and lower-middle-income countries into one group and upper-middle and high-income countries into another. Table A.10 in the Appendix shows the results of estimating Eq. (1) for each of the two resulting income groups.

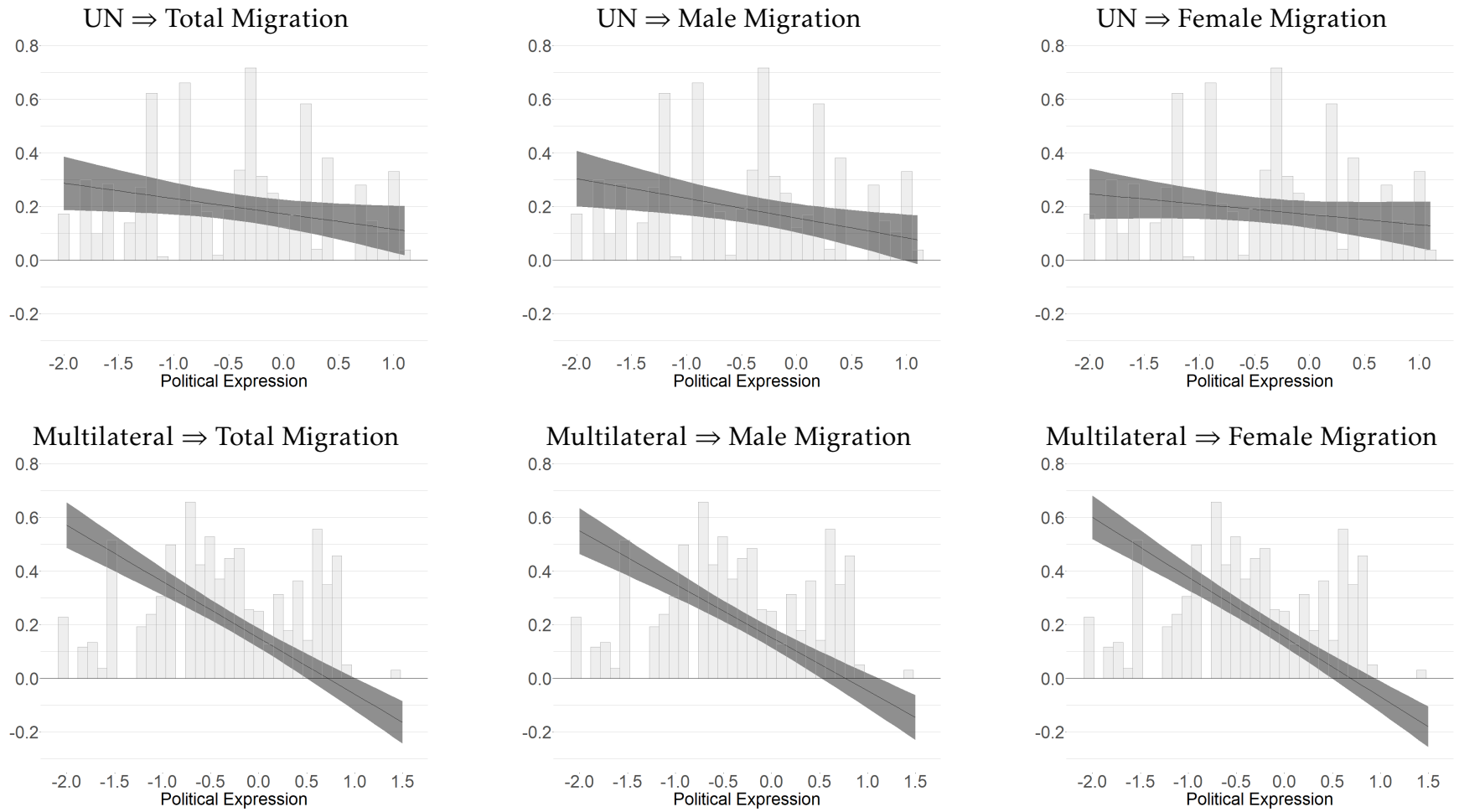
The effects of UN and Western multilateral sanctions are more or less the same across income groups. If at all, the point estimates are (slightly) larger for high-income countries, but they are also less precisely estimated. The difference in the coefficients for multilateral sanctions compared to the baseline estimates in Table 2.1 might be due to the loss of roughly 8,000 observations, as the World Bank classification is only available since 1987.[29] Finally, we detect some differences in the effects of unilateral US sanctions and non-Western sanctions on migrant flows from low-income countries (negative) and high-income countries (positive). There is no apparent theoretical justification for these subsample effects, which cancel each other out in the full sample. While it is plausible that sanctions suppress emigration only in low-income countries by depriving regular citizens of the resources needed for migration – especially to the Global North – it is unclear why that would not equally apply to multilateral and UN sanctions. Yet, these more powerful sanctions have virtually the same effect in low- and high-income countries.

Effects of Sanctions Conditional on Freedom of Political Expression. To test Hypothesis 3, we extend Eq. (1) with a variable measuring freedom of political expression and two interactions of this variable with the indicators for UN sanctions and Western multilateral sanctions.

28. We also considered splitting the sample into the Cold War period and the period thereafter. However, due to limited data availability before 1990, this would leave us with highly unbalanced subsamples of 10,993 (Cold War) and 68,798 (post-Cold War) observations.

29. When we restrict the sample for the baseline estimations in Table 2.1 to start in 1987, the effect of Western multilateral sanctions shrinks as well.

Figure 2.2: Effects of UN and Western Multilateral Sanctions Conditional on Freedom of Political Expression (Log + 1)



Notes: Effects of sanctions conditional on freedom of political expression according to an estimation of Eq. (1) for different dependent variables. Standard errors are clustered at the dyad level. Models include control variables (other sanction indicators, lagged *log*(GDP pc), Polity2, Human Rights, Freedom of Movement, Political Expression, and six conflict indicators), dyad fixed effects, and destination-year fixed effects. 95% confidence bands are indicated by dark-gray shaded areas. Light-gray vertical bars illustrate the full distribution of political expression under UN and Western multilateral sanctions. See also Table A.11 in the Online Appendix.

Table A.11 shows the results. Figure 2.2 provides a straightforward visualization of the marginal effects of sanctions on international migration, conditional on the level of freedom of political expression in the target country. For both interactions, the estimated effects are visualized over the whole range of observed freedom of political expression values of target countries, although the estimated effect is supported by very little data at the top end of the distribution.

The effect of UN sanctions on migration depends only weakly on the freedom of political expression, which is underlined by the non-significance of the negative interaction terms (see Table A.11). In contrast, the effect of Western multilateral sanctions is strongly moderated and shrinks by 20–22% with each additional standard deviation in the indicator for freedom of political expression.[30] Hence, we find, in line with Hypothesis 3, that increased migration (i.e., exit) in response to sanctions only occurs if freedom of political expression is limited (i.e., voice).

Target-Sender Migration Flows. As a final extension, we estimate Eq. (1) with added origin-year fixed effects and study migration flows specifically from target to sender countries. Our sanctions indicators are now recoded to take the value 1 only for dyad-years from a sanction target to a sanction sender.[31] In this research design, effects of UN sanctions cannot be estimated, as all countries are bound by UN sanctions and the corresponding sanctions indicator would be absorbed by the origin-year fixed effects. Chinese and Russian (i.e., non-Western) sanctions also cannot be studied here because our dataset does not cover bilateral migration flows to these countries.

We are now estimating whether sanctions lead to increased migration from the target to the sender country, which is not our research question but addresses an interesting complementary question. For the target country, the destination of their em-

30. A significant negative effect of Western multilateral sanctions on migration is estimated for freedom of political expression larger than 0.94–1.11, that is, for less than 0.5% of the observations under Western multilateral sanctions. Thus, it seems more plausible to assume a null effect on migration for sanction targets with high freedom of political expression.

31. Note that conventional sanctions indicators, as they are used throughout the sanctions literature and as they are used in the rest of the paper, would be absorbed by the origin-year fixed effects. In other words, once we include origin-year fixed effects, we can only compare migration flows from the target country to different destination countries with each other.

igrating citizens will generally not matter much. But it may be of great relevance to the sanction sender, if sanctioning a country may cause a wave of migrants from that country to arrive at its border. In other words, we are testing the argument referred to in footnote 6, but based on realized migration flows and not focused on the political decision to impose sanctions given expectations about migration flows. Finally, it is important to stress that our estimates do not indicate whether migrant flows from sanction target to sanction sender change, but whether they change relative to migrant flows to other (non-sender) destination countries. This is because flows from the target to other countries than the sender are now due to the use of "dyadic sanctions" part of the counterfactual.

The results in Table A.12 indicate that multilateral Western and unilateral EU sanctions lead to more migration to the respective sender countries than to other industrialized countries. This implies that particularly EU countries have to bear the costs of their sanctions in terms of increased immigration pressure, whereas the United States do not pay the same price.

2.5 Conclusion

We conduct the first statistical analysis of how sanctions affect international migrant flows originating from target countries. We apply two estimation strategies, a panel difference-in-differences model and an event study approach. Our dataset includes 79,791 dyad-year observations, reflecting migration flows from 157 origin countries to 32 destination countries between 1961 and 2018.

Our key findings suggest that UN and Western multilateral sanctions have a significant positive effect on migration. Emigration flows increase, on average, by 17–18% under UN sanctions and by 22–24% under Western multilateral sanctions. Our event study results of Western multilateral sanctions imply a gradual increase in emigration throughout a sanction episode with a peak effect of 80–86% for long-lasting sanctions (relative to the final year before sanctions are imposed). The impact of UN sanctions on international migration is smaller, with a peak effect of 30–31%, and less persis-

tent. Our findings can be interpreted as causal since the increase in emigration marks a significant deviation from the pre-trend. In addition, migration flows return to their pre-sanction level once sanctions are lifted. Our results are not indicative of gender differences in the effects of sanctions on migration. This finding contrasts with previous research demonstrating that women are disproportionately affected by sanctions (Demir and Tabrizy 2022; Gutmann et al. 2021) and migration is partially driven by gender-specific incentives (Gutmann, Marchal, et al. 2023; Neumayer and Plümper 2021; Ruyssen and Salomone 2018).

The positive migration effect, particularly that for Western multilateral sanctions, is driven by countries with limited freedom of political expression. This is in line with emigration serving as a substitute for voicing dissent, especially where the latter is costly (see also Hirschman 1970). The fact that sanctions lead dissatisfied citizens to emigrate may help to reconcile arguments in the literature claiming that sanctions can cause both protest (Grauvogel et al. 2017; Liou et al. 2021) and rally-around-the-flag effects (Eichenberger and Stadelmann 2022; Gold et al. 2025; Grauvogel and Soest 2014; Seitz and Zazzaro 2020). Especially in illiberal target countries, where protest and free speech are suppressed, those opposed to the regime may emigrate during sanctions, allowing for a consolidation of regime support among the remaining population.

Descriptive statistics support that countries under sanctions tend to reduce their citizens' freedom of foreign movement. Figure A.3 shows that during the second to fifth year of a sanctions episode roughly 7–8% of the countries impose restrictions on the freedom to emigrate. These governments must feel harmed or at least threatened by emigration. Cuba's requirement for a special permit before medical doctors can travel abroad, introduced in 2015, is an example of such a restriction. Cuba's government complained at the time that its health services were seriously affected by the emigration of doctors and it blamed the US for encouraging emigration from Cuba for political reasons.[32] Its medical sector earns Cuba much needed foreign currency after

32. https://www.bbc.com/news/world-latin-america-34979512.

it has endured decades of US economic sanctions. Nevertheless, omitting the migration regulation indicator from our model specification does not change our estimated emigration effect of sanctions, which suggests that while sanctioned governments might want to limit emigration, we do not find evidence that they are successful in this effort (see Table A.13).

For sanction senders, our results imply a potential unintended consequence of using sanctions to settle international disputes. Sanctions are not only potentially harmful to the sender country's economy, but they can also trigger international migration waves that may cause additional economic and political costs for the sender (see also Connell et al. 2021).

An important limitation of cross-country research on migration is the availability of bilateral migration data for only a limited number of destination countries. Therefore, our results are reflective of migration to industrialized countries (the only non-OECD member being South Africa), but they might not automatically generalize to South-South migration. Case studies of countries targeted by economic sanctions that would also consider emigration to non-OECD countries are, thus, necessary to evaluate the external validity of our results.

Appendix A

Appendix A1: Data Description

Table A.1: List of Countries

Origin Countries (number of observations; number of sanctioned observations)

Afghanistan (124; 124), Albania (505; 112), Algeria (623; 112), Angola (502; 156), Argentina (678; 95), Armenia (467; 0), Australia (698; 132), Austria (738; 148), Azerbaijan (457; 99), Bahrain (436; 0), Bangladesh (507; 0), Belarus (483; 414), Belgium (697; 134), Benin (471; 250), Bhutan (422; 0), Bolivia (542; 208), Botswana (464; 0), Brazil (675; 36), Bulgaria (599; 209), Burkina Faso (456; 23), Burundi (477; 154), Cambodia (433; 194), Cameroon (511; 45), Canada (447; 377), Cape Verde (439; 0), Central African Republic (433; 232), Chad (470; 46), Chile (622; 119), China (599; 569), Colombia (588; 421), Comoros (403; 0), Costa Rica (520; 377), Croatia (461; 240), Cuba (610; 610), Cyprus (506; 461), Czech Republic (429; 126), Denmark (657; 129), Dominican Republic (506; 197), Ecuador (534; 84), Egypt (617; 253), El Salvador (475; 23), Equatorial Guinea (415; 43), Eritrea (247; 143), Estonia (417; 152), Eswatini (438; 0), Ethiopia (549; 109), Fiji (430; 311), Finland (633; 129), France (817; 528), Gabon (435; 0), Gambia (451; 194), Georgia (465; 183), Germany (689; 148), Ghana (550; 25), Greece (741; 208, Guatemala (498; 364), Guinea (470; 349), Guinea-Bissau (445; 194), Guyana (434; 0), Haiti (471; 437), Honduras (490; 24), Hungary (501; 143), India (667; 371), Indonesia (570; 477), Iran (641; 584), Iraq (463; 421), Ireland (572; 420), Israel (444; 0), Italy (800; 139), Jamaica (470; 198), Japan (681; 0), Jordan (544; 28), Kazakhstan (465; 12), Kenya (552; 154), Kosovo (14; 0), Kuwait (442; 0), Kyrgyzstan (442; 0), Laos (373; 24), Latvia (432; 154), Lebanon (345; 345), Lesotho (443; 62), Liberia (371; 327), Libya (406; 406), Lithuania (448; 160), Luxembourg (520; 114), Madagascar (497; 202), Malawi (459; 48), Malaysia (525; 0), Mali (489; 93), Mauritania (424; 74), Mauritius (473; 0), Mexico (602; 0), Moldova (463; 376), Mongolia (488; 0), Montenegro (292; 97), Morocco (619; 0), Mozambique (468; 0), Myanmar (513; 467), Namibia (426; 0), Nepal (528; 17), Netherlands (754; 136), New Zealand (481; 0), Nicaragua (485; 86), Niger (464; 108), Nigeria (562; 433), North Macedonia (458; 0), Norway (625; 224), Oman (425; 0), Pakistan (585; 221), Panama (509; 217), Papua New Guinea (420; 0), Paraguay (492; 10), Peru (568; 63), Philippines (570; 386), Poland (518; 154), Portugal (657; 176), Qatar (350; 0), Romania (537; 207), Russia (555; 153), Rwanda (492; 246), Saudi Arabia (515; 25), Senegal (493; 0), Serbia (488; 377), Sierra Leone (486; 262), Singapore (482; 0), Slovakia (426; 126), Slovenia (421; 124), Solomon Islands (392; 0), Somalia (145; 145), South Africa (586; 150), South Korea (520; 10), Spain (706; 139), Sri Lanka (547; 5), Sudan (549; 471), Suriname (430; 14), Sweden (684; 149), Switzerland (602; 0), Syria (598; 416), Tajikistan (444; 0), Tanzania (458; 97), Thailand (551; 213), Timor-Leste (302; 0), Togo (474; 172), Tunisia (596; 214), Turkey (675; 89), Turkmenistan (431; 19), Uganda (480; 0), Ukraine (506; 215), United Arab Emirates (451; 0), United Kingdom (775; 149), United States (823; 139), Uruguay (553; 15), Uzbekistan (455; 208), Vietnam (548; 374), Yemen (481; 190), Zambia (500; 21), Zimbabwe (473; 379).

Destination Countries (number of observations)

Australia (3266), Austria (3182), Belgium (1998), Canada (3139), Chile (1831), Czech Republic (3682), Czechoslovakia (884), Denmark (4858), Estonia (2286), Finland (4440), France (1774), Germany (5897), Hungary (1685), Ireland (30), Israel (723), Italy (2866), Latvia (462), Luxembourg (2127), Mexico (1144), Netherlands (1923), New Zealand (5216), Norway (3241), Poland (1065), Portugal (282), Slovakia (3206), Slovenia (1974), South Africa (1750), South Korea (2890), Spain (4100), Sweden (3559), Switzerland (1367), Turkey (382), United States (2562).

Table A.2: Definitions of Variables and Data Sources

Variable	**Definition & Source**
Total Migration / Male Migration / Female Migration	Natural logarithm [$log(y+1)$ & $log(y)$] and inverse hyperbolic sine [$log(y+\sqrt{y^2+1})$] of total / male / female bilateral migration. *Source:* OECD International Migration Database (OECD 2020), DEMIG C2C dataset (DEMIG 2015).
Sanctions	Binary indicators for country-years with sanctions in place. *Source:* GSDB (Felbermayr et al. 2020; Kirilakha et al. 2021; Syropoulos et al. 2024).
lagged log(GDP pc)	Natural logarithm of real GDP per capita in 2015 USD, lagged by one year. *Source:* World Development Indicators (World Bank 2025).
Polity2	Democracy indicator that ranges from strongly democratic (+10) to strongly autocratic (–10). *Source:* Polity5 dataset (Marshall and Gurr 2020).
Human Rights	Latent human rights variable with higher values indicating a better protection of human rights. *Source:* Human Rights Protection Scores (Fariss 2019).
Freedom of Movement	Binary variable that takes the value 0 in the case of no respect for freedom of movement and 1 if freedom of freedom of foreign travel and emigration is at least weakly respected. *Source:* V-Dem, version 14 (Coppedge et al. 2024).
Political Expression	Standardized first principal component of Freedom of Discussion for Men, Freedom of Discussion for Women, Freedom of Academic & Cultural Expression, and Freedom of Peaceful Assembly. *Source:* V-Dem, version 14 (Coppedge et al. 2024).
Minor Conflict / Major Conflict	Armed conflicts resulting in 25 to 999 / at least 1,000 battle-related deaths in a given year. *Source:* UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002; Davies et al. 2022).
Interstate Conflict	Conflicts between two states. *Source:* UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002; Davies et al. 2022).
Intrastate Conflict w/ Intervention / w/o Intervention	Conflicts between a government and rebel groups with / without military intervention by foreign governments. *Source:* UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002; Davies et al. 2022).

Table A.3: Descriptive Statistics

	All Observations		**No Sanctions**		**Sanctions**	
	Mean	N	Mean	N	Mean	N
Total Migration						
$\ldots log(\ldots + 1)$	4.06	79,791	3.87	55,701	4.49	24,090
$\ldots log(\ldots)$	4.39	72,267	4.24	49,719	4.72	22,548
$\ldots ihs(\ldots)$	4.65	79,791	4.45	55,701	5.11	24,090
Male Migration						
$\ldots log(\ldots + 1)$	3.49	79,791	3.31	55,701	3.90	24,090
$\ldots log(\ldots)$	3.92	69,106	3.78	47,249	4.21	21,857
$\ldots ihs(\ldots)$	4.05	79,791	3.85	55,701	4.49	24,090
Female Migration						
$\ldots log(\ldots + 1)$	3.38	79,791	3.22	55,701	3.76	24,090
$\ldots log(\ldots)$	3.81	68,527	3.68	47,015	4.10	21,512
$\ldots ihs(\ldots)$	3.93	79,791	3.75	55,701	4.33	24,090
lagged log(GDP pc)	8.44	79,791	8.51	55,701	8.29	24,090
Polity2	3.67	79,791	3.95	55,701	3.04	24,090
Human Rights	0.45	79,791	0.59	55,701	0.12	24,090
Political Expression	0.39	79,791	0.47	55,701	0.18	24,090
	$X = 1$	N	$X = 1$	N	$X = 1$	N
Freedom of Movement	74,719	79,791	53,564	55,701	21,155	24,090
Minor Conflict	11,190	79,791	6,370	55,701	4,820	24,090
… Interstate	492	79,791	283	55,701	209	24,090
… Internal w/ Intervention	1,717	79,791	1,111	55,701	606	24,090
… Internal w/o Intervention	8,981	79,791	4,976	55,701	4,005	24,090
Major Conflict	2,653	79,791	1,093	55,701	1,560	24,090
… Interstate	176	79,791	89	55,701	87	24,090
… Internal w/ Intervention	807	79,791	107	55,701	700	24,090
… Internal w/o Intervention	1,670	79,791	897	55,701	773	24,090

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. The smaller number of observations for the 'log transformation' is due to dyad-years with zero migration flows.

Table A.4: Frequency of Sanctions

Panel A: Panel DiD Estimations			
Sanctions in General			
… No Sanctions	55,701		
… Sanctions	24,090		
Sanction Senders		Sanction Targets	
… UN	3,932	… Low Income	12,536
… EU-US joint	4,480	… High Income	10,265
… EU only	2,034		
… US only	8,972		
… Non-Western	5,561		
Panel B: Event Study Approach			
UN Sanctions		Western Multilateral Sanctions	
… Pre-Trend –3 Years	354	… Pre-Trend –3 Years	714
… Pre-Trend –2 Years	406	… Pre-Trend –2 Years	764
… Pre-Trend –1 Year	435	… Pre-Trend –1 Year	843
… Year 1	353	… Year 1	794
… Year 2	366	… Year 2	670
… Year 3	331	… Year 3	479
… Year 4	265	… Year 4	372
… Year 5	262	… Year 5	304
… Year 6	227	… Year 6	188
… Year 7	188	… Year 7	186
… Year 8	180	… Year 8	152
… Year 9	171	… Year 9	109
… Year 10	178	… Year 10	99
… Year 11+	1,411	… Year 11+	1,127
… Post-Trend +1 Year	425	… Post-Trend +1 Year	853
… Post-Trend +2 Years	394	… Post-Trend +2 Years	776
… Post-Trend +3 Years	337	… Post-Trend +3 Years	731

Notes: Frequency of observations of the different sanction indicators for which all control variables (see Table A.3) are available. Total number of observations in the dataset: 79,791. Sanctions enacted by the UNSC are not counted as US, EU, or non-Western sanctions. The three types of Western sanctions are by construction disjunctive. However, Western and non-Western sanctions can coincide.

Appendix A2: Additional Results

Table A.5: Baseline Results (Inverse Hyperbolic Sine Transformation)

	(1) Total Migration	(2) Male Migration	(3) Female Migration
Sanctions			
… UN	0.191***	0.185***	0.175***
	(0.053)	(0.055)	(0.052)
… Multilateral (EU-US)	0.228***	0.227***	0.240***
	(0.043)	(0.044)	(0.044)
… EU only	0.056	0.052	0.054
	(0.037)	(0.038)	(0.037)
… US only	–0.006	0.005	–0.009
	(0.026)	(0.026)	(0.025)
… Non-Western	0.023	0.070**	0.001
	(0.029)	(0.029)	(0.029)
Observations	79,791	79,791	79,791
R^2	0.937	0.926	0.934
Within-R^2	0.018	0.016	0.018

Notes: Coefficient estimates of Eq. (1) and different dependent variables. Standard errors in parentheses are clustered at the dyad level. Models include control variables (lagged *log*(GDP pc), Polity2, Human Rights, Freedom of Movement, and six conflict indicators; coefficient estimates are available on request), dyad fixed effects, and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.

Table A.6: Baseline Results (Log)

	(1) Total Migration	(2) Male Migration	(3) Female Migration
Sanctions			
… UN	0.201***	0.203***	0.226***
	(0.057)	(0.060)	(0.058)
… Multilateral (EU-US)	0.243***	0.247***	0.262***
	(0.046)	(0.048)	(0.050)
… EU only	0.049	0.045	0.059
	(0.039)	(0.040)	(0.040)
… US only	–0.004	0.006	–0.012
	(0.026)	(0.028)	(0.026)
… Non-Western	0.008	0.040	–0.022
	(0.027)	(0.029)	(0.028)
Observations	72,267	69,106	68,527
R^2	0.931	0.918	0.926
Within-R^2	0.022	0.020	0.024

Notes: Coefficient estimates of Eq. (1) and different dependent variables. Standard errors in parentheses are clustered at the dyad level. Models include control variables (lagged *log*(GDP pc), Polity2, Human Rights, Freedom of Movement, and six conflict indicators; coefficient estimates are available on request), dyad fixed effects, and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.

Table A.7: Results without Mediating Variables (Log + 1)

	(1) Baseline Results	(2) Excl. GDP	(3) Excl. Human Rights	(4) Excl. Democracy
Sanctions				
… UN	0.177***	0.135***	0.222***	0.176***
	(0.052)	(0.050)	(0.053)	(0.052)
… Multilateral (EU-US)	0.228***	0.216***	0.259***	0.214***
	(0.042)	(0.043)	(0.042)	(0.042)
… EU only	0.053	0.053	0.076**	0.064*
	(0.035)	(0.035)	(0.035)	(0.035)
… US only	–0.005	–0.017	0.012	–0.010
	(0.025)	(0.026)	(0.025)	(0.025)
… Non-Western	0.034	0.008	0.018	0.024
	(0.028)	(0.030)	(0.028)	(0.027)
lagged *log*(GDP pc)	0.304***		0.274***	0.293***
	(0.070)		(0.070)	(0.070)
Polity2	0.009***	0.007**	0.005	
	(0.003)	(0.003)	(0.003)	
Human Rights	–0.123***	–0.111***		–0.114***
	(0.016)	(0.016)		(0.016)
Freedom of Movement	–0.031	0.007	–0.053	–0.004
	(0.055)	(0.060)	(0.055)	(0.056)
Interstate Conflicts				
… Minor	0.020	–0.014	0.043	0.016
	(0.063)	(0.071)	(0.064)	(0.063)
… Major	–0.203*	–0.204*	–0.181	–0.196*
	(0.110)	(0.110)	(0.110)	(0.110)
Internal Conflicts w/o Intervention				
… Minor	0.024	0.015	0.085***	0.028
	(0.023)	(0.024)	(0.023)	(0.023)
… Major	–0.010	–0.023	0.097***	–0.006
	(0.038)	(0.037)	(0.036)	(0.038)
Internal Conflicts w/ Intervention				
… Minor	0.007	0.008	0.109**	0.015
	(0.045)	(0.045)	(0.044)	(0.045)
… Major	0.417***	0.395***	0.519***	0.429***
	(0.077)	(0.076)	(0.076)	(0.077)
Observations	79,791	79,791	79,791	79,791
R^2	0.940	0.940	0.940	0.942
Within-R^2	0.020	0.014	0.014	0.019

Notes: Coefficient estimates of Eq. (1) for total migration and different sets of control variables. Standard errors in parentheses are clustered at the dyad level. Models include dyad fixed effects and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level. Column (1) replicates the baseline results from Table 1.

Table A.8: Frequency of Sanction Objectives

Objective	**UN**	**Multilateral (EU-US)**	**EU only**	**US only**	**Non-Western**
Democracy	218	1,635	456	1,568	326
Human Rights	1,117	2,363	486	1,862	370
Destabilizing Regime	0	14	0	853	30
Policy Change	302	332	376	1,169	1,007
Prevent War	1,009	206	0	306	0
End War	1,980	330	133	331	0
Territorial Conflict	129	0	0	461	154
Terrorism	501	845	196	918	0
Other	62	270	593	2,775	765

Notes: Frequency of observation of the different sanction objectives by sender in the full sample with all control variables (see Table A.3). Total number of observations in the dataset: 79,791. Sanction episodes can have multiple objectives.

Table A.9: Results excluding Sanction Objectives (Log + 1)

	Excl. "Democracy"	**Excl. "Human Rights"**	**Excl. "Destab. Regime"**	**Excl. "Policy Change"**	**Excl. "Prevent War"**
… UN	0.159***	0.160***	0.165***	0.222***	0.207***
	(0.057)	(0.061)	(0.0518)	(0.052)	(0.057)
… Multilateral (EU-US)	0.441***	0.187***	0.193***	0.262***	0.180***
	(0.063)	(0.046)	(0.036)	(0.046)	(0.040)
… EU only	0.153***	0.036	0.044	0.058	0.061*
	(0.039)	(0.042)	(0.035)	(0.036)	(0.035)
… US only	–0.053*	–0.026	0.007	–0.018	0.012
	(0.029)	(0.027)	(0.024)	(0.026)	(0.026)
… Non-Western	0.049*	0.053**	0.049*	0.065**	0.046*
	(0.028)	(0.027)	(0.026)	(0.031)	(0.027)
Observations	75,716	73,721	78,924	76,938	78,270
R^2	0.941	0.943	0.941	0.941	0.941
Within-R^2	0.023	0.015	0.018	0.021	0.018

	Excl. "End War"	**Excl. "Territ. Conflict"**	**Excl. "Terrorism"**	**Excl. "Other Object."**
… UN	0.134**	0.188***	0.296***	0.190***
	(0.063)	(0.054)	(0.067)	(0.053)
… Multilateral (EU-US)	0.185***	0.227***	0.147***	0.253***
	(0.040)	(0.042)	(0.041)	(0.046)
… EU only	0.086**	0.054	0.044	0.050
	(0.034)	(0.035)	(0.037)	(0.047)
… US only	0.011	0.005	–0.012	0.010
	(0.026)	(0.025)	(0.026)	(0.031)
… Non-Western	0.046*	0.037	0.050*	0.029
	(0.027)	(0.028)	(0.028)	(0.030)
Observations	77,017	79,170	77,331	75,536
R^2	0.943	0.941	0.941	0.939
Within-R^2	0.018	0.020	0.019	0.021

Notes: Coefficient estimates of Eq. (1) for total migration. Standard errors in parentheses are clustered at the dyad level. Models include control variables (lagged *log*(GDP pc), Polity2, Human Rights, Freedom of Movement, and six conflict indicators; coefficient estimates are available on request), dyad fixed effects, and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.

Table A.10: Results for Low- and High-Income Countries (Log + 1)

	Low-Income Countries	**High-Income Countries**
Sanctions		
… UN	0.182***	0.198*
	(0.043)	(0.114)
… Multilateral (EU-US)	0.112***	0.151***
	(0.033)	(0.048)
… EU only	0.037	0.067
	(0.039)	(0.053)
… US only	–0.041*	0.112**
	(0.024)	(0.046)
… Non-Western	–0.132**	0.104***
	(0.055)	(0.028)
Observations	37,526	34,290
R^2	0.949	0.956
Within-R^2	0.022	0.008

Notes: Coefficient estimates of Eq. (1) for total migration. Standard errors in parentheses are clustered at the dyad level. Models include control variables (lagged *log*(GDP pc), Polity2, Human Rights, Freedom of Movement, and six conflict indicators; coefficient estimates are available on request), dyad fixed effects, and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.

Table A.11: Results including Political Expression (Log + 1)

	(1) Total Migration	(2) Male Migration	(3) Female Migration
Sanctions			
… UN	0.173***	0.157***	0.170***
	(0.053)	(0.054)	(0.052)
… Multilateral (EU-US)	0.151***	0.152***	0.154***
	(0.040)	(0.039)	(0.041)
… EU only	0.055	0.051	0.055
	(0.035)	(0.036)	(0.035)
… US only	0.002	0.011	–0.003
	(0.025)	(0.025)	(0.023)
… Non-Western	0.039	0.081***	0.017
	(0.028)	(0.028)	(0.027)
Political Expression	0.003	0.005	–0.013
	(0.034)	(0.034)	(0.033)
UN × Political Expression	–0.057	–0.074	–0.038
	(0.048)	(0.049)	(0.047)
Multilateral × Political Expression	–0.210***	–0.199***	–0.223***
	(0.057)	(0.059)	(0.056)
Observations	79,791	79,791	79,791
R^2	0.940	0.930	0.939
Within-R^2	0.021	0.020	0.023

Notes: Coefficient estimates of Eq. (1) and different dependent variables. Standard errors in parentheses are clustered at the dyad level. Models include control variables (lagged *log*(GDP pc), Polity2, Human Rights, Freedom of Movement, and six conflict indicators; coefficient estimates are available on request), dyad fixed effects, and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.

Table A.12: Results using Dyadic Sanctions Measures (Log +1)

	Total Migration
Sanctions	
… Multilateral (EU-US) – Dyadic Measure	0.138**
	(0.054)
… EU only – Dyadic Measure	0.133***
	(0.042)
… US only – Dyadic Measure	–0.110
	(0.071)
Observations	79,791
R^2	0.944
Within-R^2	0.003

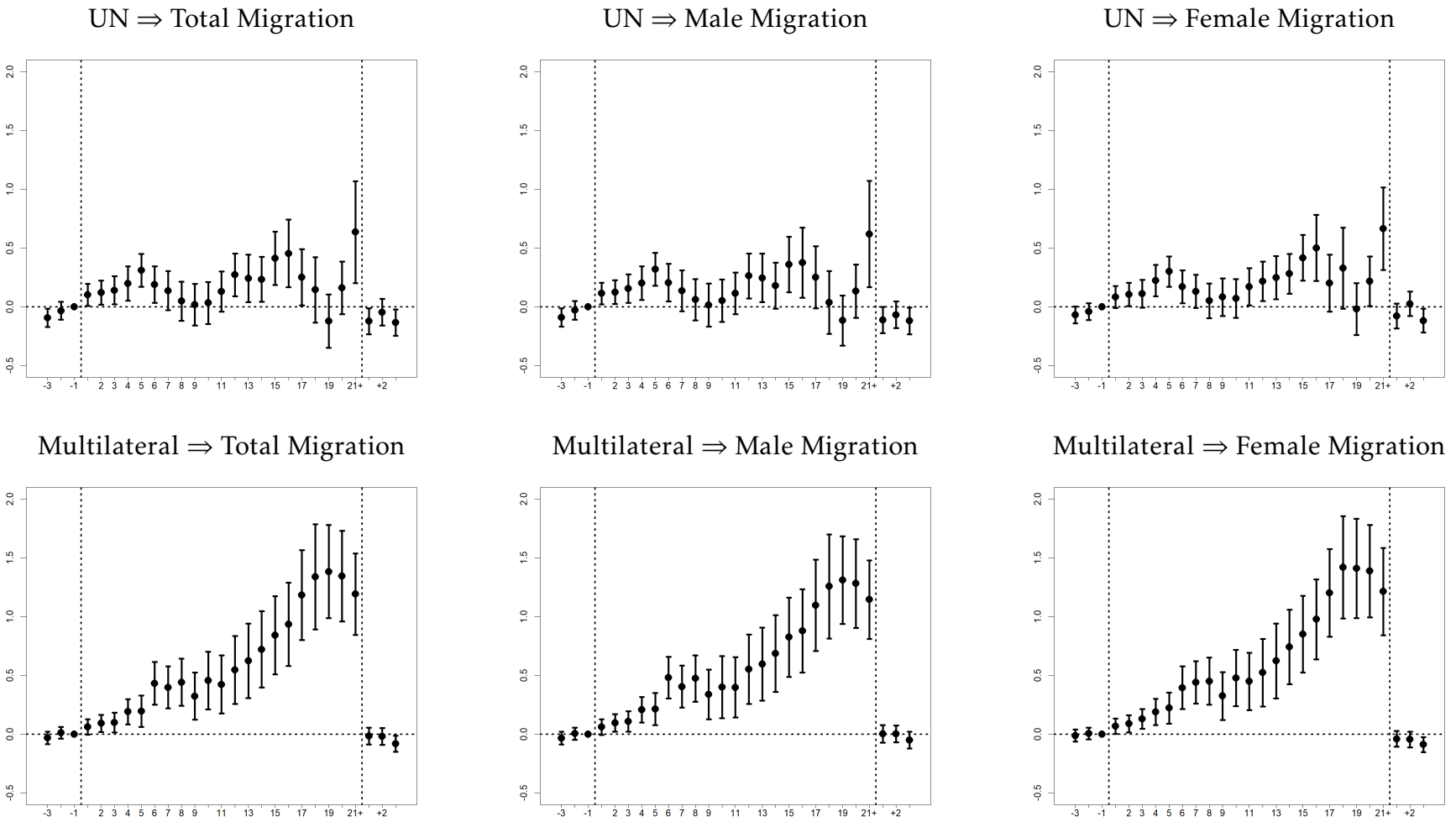
Notes: Coefficient estimates of a modified version of Eq. (1). Standard errors in parentheses are clustered at the dyad level. Models include dyad fixed effects, destination-year fixed effects, and origin-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.

Table A.13: Results excluding Freedom of Movement (Log + 1)

	(1) Baseline Results	(2) Excl. Freedom of Movement
Sanctions		
... UN	0.177***	0.179***
	(0.052)	(0.052)
... Multilateral (EU-US)	0.228***	0.231***
	(0.042)	(0.041)
... EU only	0.053	0.055
	(0.035)	(0.035)
... US only	–0.005	-0.004
	(0.025)	(0.025)
... Non-Western	0.034	0.034
	(0.028)	(0.028)
Observations	79,791	79,791
R^2	0.940	0.940
Within-R^2	0.020	0.020

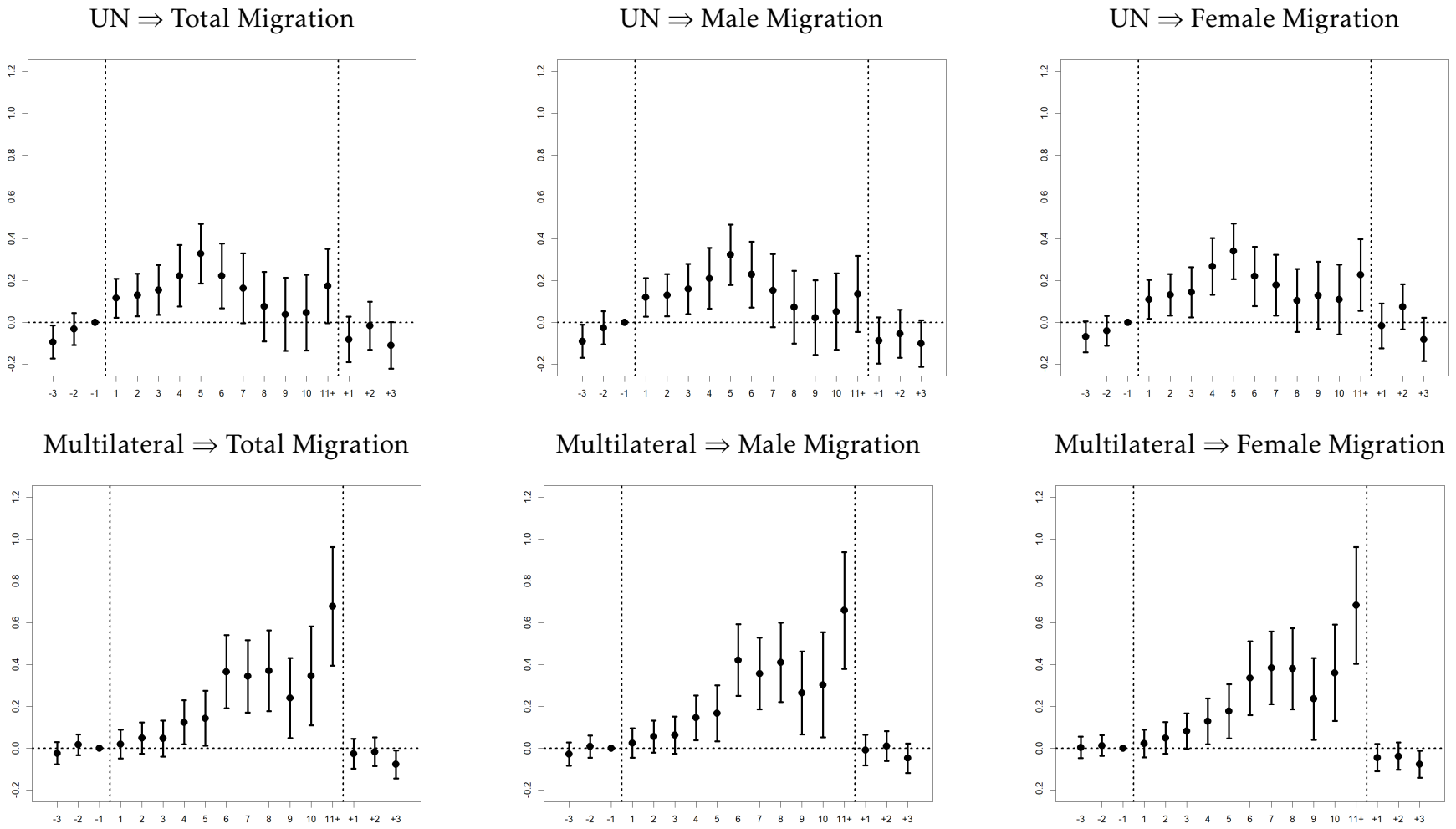
Notes: Coefficient estimates of Eq. (1) for total migration and different sets of control variables. Standard errors in parentheses are clustered at the dyad level. Models include control variables (lagged *log*(GDP pc), Polity2, Human Rights, Freedom of Movement, and six conflict indicators; coefficient estimates are available on request), dyad fixed effects, and destination-year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level. Column (1) replicates the baseline results from Table 1 in the paper.

Figure A.1: Effects of UN and Western Multilateral Sanctions Over Time (Log + 1): Extension 20 Years



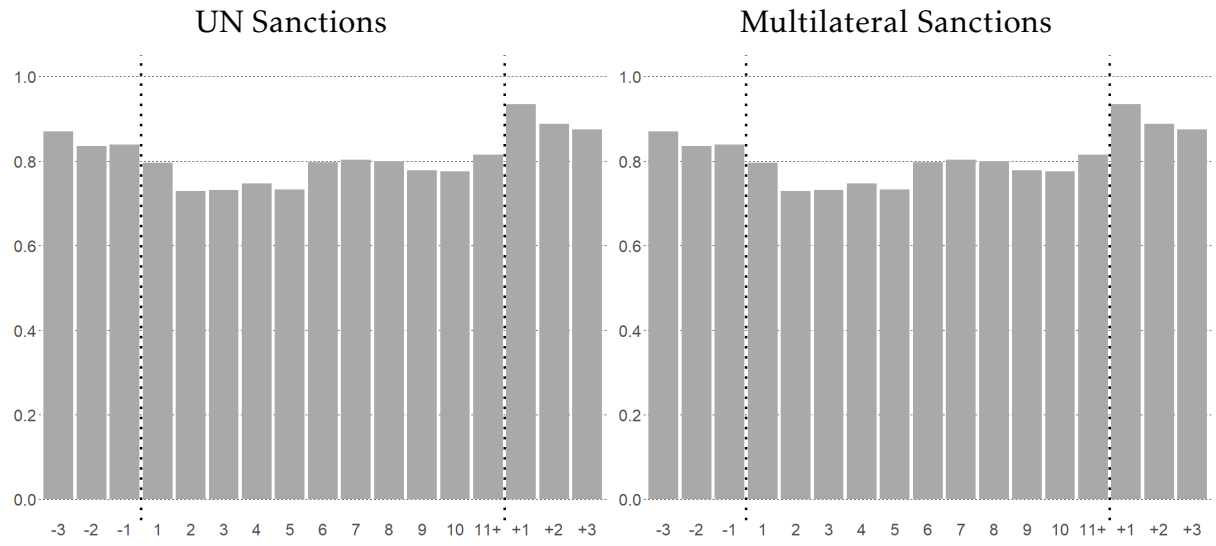
Notes: Effects of sanctions over time (1, 2, …, 21+) alongside pre-trend (−3, −2, −1) and post-trend (+1, +2, +3) according to an estimation of a modified version of Eq. (2) for different dependent variables. Standard errors are clustered at the dyad level. Models include control variables (other sanction indicators, lagged *log*(GDP pc), Polity2, Human Rights, Freedom of Movement, and six conflict indicators), dyad fixed effects, and destination-year fixed effects. 95% confidence bands are indicated by whiskers.

Figure A.2: Effects of UN and Western Multilateral Sanctions Over Time (Log + 1): Excluding Never-Sanctioned Origin Countries



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, excluding origin countries that were never subject to UN sanctions or Western multilateral sanctions. Standard errors are clustered at the dyad level. Models include control variables (other sanction indicators, lagged *log*(GDP pc), Polity2, Human Rights, Freedom of Movement, and six conflict indicators), dyad fixed effects, and destination-year fixed effects. 95% confidence bands are indicated by whiskers.

Figure A.3: Frequency of Freedom of Movement



Notes: Bar charts show the distribution of the freedom of foreign movement variable throughout a sanctions episode (1, 2, …, 11+) alongside pre-trend (−3, −2, −1) and post-trend (+1, +2, +3).

3 International Sanctions and Coups d'État

"International Sanctions and Coups d'État"

Abstract

Sanctions are intended to pressure governments, but they may also destabilize regimes from within. When sanctions weaken state capacity and patronage networks, coups d'état may become rationally attractive to internal elites. This paper analyzes how international sanctions influence the likelihood of coups d'état in targeted countries. Using panel logit models, an event-study design, and propensity-score weighting on data for 158 countries from 1971 to 2019, I find that Western sanctions significantly increase coup risk, with an initial effect of about 7.8 pp in the first year after imposition. On average, Western sanctions raise the probability of coups by 1.7–1.9 pp, and successful coups by 2.1–2.3 pp, with particularly strong effects in the post-Cold War era. The impact is largely driven by authoritarian regimes, especially personalist and military systems.

3.1 Introduction

The effects of sanctions on target countries have been widely studied, revealing significant economic, social, and political impacts. Economically, sanctions lead to GDP decline, reduced foreign investment and disrupted trade (Gutmann, Neuenkirch, and Neumeier 2023a; Neuenkirch and Neumeier 2015; Crozet and Hinz 2020; Dai et al. 2021; Gutmann, Neuenkirch, and Neumeier 2024; Afesorgbor 2019). Socially, sanctions can intensify poverty, strain healthcare systems, reduce life expectancy, increase emigration, and heighten inequality (Gutmann et al. 2021; Afesorgbor and Mahadevan 2016; Moteng et al. 2023; Gutmann, Langer, et al. 2024; Neuenkirch and Neumeier 2016). Although sanctions are often imposed with the hope of achieving regime transition or policy change in the target country, their political effects tend to be far more ambiguous.

While sanctions sometimes destabilize governments or drive policy changes, they may also consolidate authoritarian power by invoking nationalism or justifying increased repression. Some studies contend that the threat or imposition of sanctions motivates anti-government protests (Grauvogel et al. 2017; Liou et al. 2021), potentially pressuring the targeted government to meet the political demands of both sanctioning countries (Attia et al. 2020) and nonviolent domestic movements (Liou et al. 2023). Regarding government stability, Marinov (2005) finds that sanctions diminish target country leaders' prospects of maintaining their position office. Escribà-Folch and Wright (2010) also conclude that economic sanctions increase the likelihood of a change of a target country's autocratic ruler. In contrast, other research suggests that sanctions may trigger a rally-around-the-flag effect, which bolsters the regime's popularity and solidify its control within the country (Eichenberger and Stadelmann 2022; Gold et al. 2025; Grauvogel and Soest 2014; Seitz and Zazzaro 2020). Despite extensive research on the effects of sanctions on government support and survival, no study has explicitly examined the relationship between sanctions and coups d'état.

While Marinov (2005) explored the impact of sanctions on general "survival in office", his approach did not explicitly address the dynamics of coups and different

regime types. Additionally, although Escribà-Folch and Wright (2010) incorporated the concept of coups through their specification of irregular exits, their focus remained confined to authoritarian regimes, without accounting for distinctions between the sanction sender or different types of sanctions. Furthermore, the concept of irregular exit includes further events, such as revolts or assassinations.

To fully understand the dynamics behind regime collapse, it is crucial to distinguish among different types of leader exits. This applies not only to the distinction between regular exits (primarily elections or other constitutional processes) and irregular exits (such as coups, revolts, or assassinations), but also to the variation within irregular exits themselves, as each is driven by fundamentally different mechanisms. Although coups, revolts, and assassinations may all stem from political dissatisfaction, the actors involved in coups, are internal elite dissidents rather than ordinary citizens. Consequently, their strategies, intentions, motivations, perceptions of success, anticipated benefits and costs, and their relationship to the incumbent leader differ markedly from those of popular revolt participants.

It is equally important to differentiate among various types of regimes, particularly within the broad category of authoritarianism. Authoritarian regimes vary significantly in their institutional structures. Some rely primarily on military strength and coercive capacity (e.g., the military junta in Argentina in 1976), others revolve around a dominant personalist leader (e.g., Belarus under Lukashenko), and still others are governed by a powerful single-party apparatus, such as the Communist Party in China.[1] As a result, patronage networks, institutional foundations, and instruments of repression vary across authoritarian regime types, shaping both their overall stability and their relationship to potential coup perpetrators.

Therefore, this paper offers both a theoretical framework for understanding the relationship between sanctions and coups, and a statistical analysis of how sanctions affect the likelihood of coups in target countries. It provides a nuanced understanding

1. There may, of course, be overlaps. For instance, many personalist regimes are supported by a dominant ruling party or a strong military. Nevertheless, identifying the primary locus of power within an autocratic system remains crucial. The classification used here follows Eschenauer-Engler and Herre (2023), based on Geddes et al. (2014).

of the role of sanctions in facilitating or deterring coups, across both democratic and several types of authoritarian contexts.[2]

I estimate the impact of sanctions on the likelihood of coups using panel logit models with region and year fixed effects. Because sanctions constitute a binary treatment variable, the specification is analogous to a difference-in-differences design. To strengthen causal identification, I additionally implement an event-study framework and propensity-score weighting to account for temporal effects and systematic differences between treated and untreated countries.

One reason why the relationship between sanctions and coups d'état has not been systematically examined may be the small-n problem inherent in quantitative coup research. Although coups tend to have dramatic consequences, they occur infrequently, which makes reliable estimation challenging. To explicitly address this issue, I employ Firth's logit estimator, alongside a range of robustness checks designed to mitigate common estimation problems in this context and to strengthen the causal interpretation of the results.

Finally, this paper contributes by not only offering the first quantitative analysis of the relationship between sanctions and coups, but also by extending the temporal scope of existing work in related areas, which typically relies on data that do not extend beyond the 1990s. As sanctions have become increasingly common tools of international politics since the end of the Cold War, more recent data capture contemporary global dynamics and allow me to investigate the relationship between sanctions and coups both during and after the Cold War.

The data is mainly derived from the Global Sanctions Data Base (GSDB, see Felbermayr et al. 2020) and the novel Coup Leader Dataset (Eschenauer-Engler and Herre 2023). 6,070 observations for 158 countries for the period from 1971 to 2019 are cov-

2. von Soest and Wahman (2014) also examine the relationship between coups and sanctions, but from a different perspective. They conceptualize coups as "trigger events" that increase the likelihood of democratic sanctions being imposed on the country in which the coup occurs. In their analysis, coups enter contemporaneously; they do not lag the coup variable or apply additional strategies to address potential endogeneity. In contrast, in this paper sanctions are lagged by one year to mitigate concerns about reverse causality, and several complementary approaches are employed to support a causal interpretation of the results. Further details are provided in the following sections.

ered by the dataset. To accurately assess the impact of international sanctions, I distinguish between (i) Western sanctions (i.e. imposed by the US and/or EU countries) and (ii) Non-Western sanctions (i.e. imposed by the UN or Russia and/or China).[3]

On average, Western sanctions increase the probability of coups by about 1.7 to 1.9 percentage points (pp). Even under conservative model specifications, including a range of confounders potentially leading to an underestimation of the real effect, the results remain statistically significant for the subset of successful coups, that is, coup attempts in which the perpetrators successfully replace the incumbent leader. Propensity score weighting estimations suggest an even slightly stronger impact of Western sanctions. Given that the baseline probability of coups is generally only around 1.7%, this represents a substantively large effect.

The average effects are found to be strongest in non-democratic regimes. However, there is substantial heterogeneity across different types of authoritarian systems: the effects are more pronounced when single-party regimes are excluded from the sample. Further analysis of effect heterogeneity suggests that sanctions motivated by the objective of democratization are a key driver of the overall results.

The findings further indicate that Western sanctions significantly increase the likelihood of coups in the first year following their imposition, with a peak effect of approximately 7.8 percentage points. After the first year of a sanctions episode, the effect remains positive but is no longer statistically significant. Moreover, the effect of Western sanctions on coup likelihood is significantly stronger in the post-Cold War period across all model specifications. Western sanctions more than double the probability of a coup. By contrast, non-Western sanctions do not exhibit a statistically significant effect on coup likelihood in target countries.

The remainder of this paper is structured as follows. Section 3.2 explains the theoretical understanding of the relationship between sanctions and coups. Section 3.3 describes the empirical approach and the data used. Section 3.4 interprets and discusses the results, while section 3.5 concludes.

3. Given the limited number of observations (especially of Russian and Chinese sanctions), a more granular differentiation of sanctions senders was not feasible.

3.2 Theory

According to the most common definition of coups d'état in academic research, coups are defined as "illegal and overt attempts by the military or other elites within the state apparatus to unseat the sitting executive" (Powell and Thyne 2011, p. 252).[4] Following Gassebner et al. (2016), the decision for a coup attempt is assumed to be based on a rational cost-benefit analysis in which the expected utility of the coup perpetrators after the coup $E(U_{coup})$ must be higher than the current utility $U_{status\ quo}$ ($E(U_{Coup}) > U_{status\ quo}$).

$$E(U_{coup}) = p \times B + (1 - p) \times C, \tag{1}$$

where p represents the probability of coup success, B is the net benefit or payoff of a successful coup and C symbolizes costs or the negative payoff of a non-successful coup.[5]

International sanctions aim to pressure targeted leaders, typically in the hope of inducing policy change or regime transition. Such measures impose economic costs that weaken a government's financial and political stability, thereby affecting the utility of maintaining the status quo (Gutmann, Neuenkirch, and Neumeier 2023a; Neuenkirch and Neumeier 2015; Crozet and Hinz 2020; Dai et al. 2021; Gutmann, Neuenkirch, and Neumeier 2024; Afesorgbor 2019). The resulting economic hardships erode a leader's legitimacy and the state's capacity to sustain patronage networks and governance functions, increasing the likelihood of coups, as it lowers the elites current utility, and hence, increasing B. It draws on the influential works of Gordon Tullock (1974, 1987, 2005) and Ronald Wintrobe (1990, 1998, 2012) on autocracy and coups d'état. Tullock argues that autocrats are primarily concerned with regime survival and

4. Due to the complex nature and causes of coups, the definition can vary depending on whether the focus is on the actors involved, the methods used, or the legitimacy of such actions (Welch 1970; Janowitz 1977; O'Kane 1987; Finer 1988; McGowan 2003; Marshall and Marshall 2007; Edward 2016). However, most definitions share the idea that within the state apparatus, often from within the military, an (illegal) attempt is made to replace the ruling leader.

5. Comparable theoretical frameworks have been developed by Finer (1988), Galetovic and Sanhueza (2000), Leon (2014), and Aidt and Leon (2019).

must therefore overcompensate, or *overpay*, potential rivals to prevent defection. In other words, dictators must ensure that $E(U_{coup}) < U_{status\ quo}$ by keeping the expected benefits of coups, B, low. It implies that political leaders require substantial resources to maintain elite loyalty. Economic sanctions reduce these resources, thereby intensifying pressure on incumbents. Leaders deprived of economic rents may struggle to finance the military and to buy the allegiance of key elites, increasing the risk of a coup (Marinov 2005).

However, sanctions do not uniformly destabilize governments. When elites anticipate that the external threat posed by sanctioning countries could jeopardize their own future position, rather than solely that of the incumbent, the expected benefit of a coup, B, decreases. In such situations, it becomes fully rational for elites to close ranks behind the leadership. This dynamic triggers a rally-around-the-flag effect and ultimately strengthens the regime's legitimacy.

Moreover, sanctions operate as a form of enforced protectionism (Eichenberger and Stadelmann 2022, p. 112). While they generally reduce overall welfare, they also generate domestic winners who substitute for foreign suppliers in meeting local demand. In authoritarian contexts, where the regime maintains strong control over domestic producers, these actors often profit from the altered economic environment and thereby reinforce the government's elite support base, as the current utility, $U_{status\ quo}$, increases (Eichenberger and Stadelmann 2022; Grauvogel and Soest 2014; Gold et al. 2025).

Thus, the impact of sanctions on coup dynamics depends on whether elites perceive the external threat as directed solely at the incumbent or as one that, even after a successful coup, would also endanger their own future position, thereby decreasing B. Consequently, the following hypotheses capture these ambiguous theoretical expectations:

Hypothesis 1a. *Sanctions increase the likelihood of a coup attempt in the target country.*

Hypothesis 1b. *Sanctions reduce the likelihood of a coup attempt in the target country.*

With that in mind, sanctions are likely to have different effects on the stability of democracies compared to authoritarian regimes. In democracies, sanctions create economic discontent that translates into electoral pressure rather than coups. Democratic institutions provide avenues for peaceful leadership change, mitigating the risk of military intervention. So, if sanctions significantly harm the economy, public dissatisfaction leads to political upheaval through elections rather than through military coups (Bell 2016; Gassebner et al. 2016; Marinov 2005). Thus, assuming that evidence is found in favor of Hypothesis 1a, the second hypothesis states:

Hypothesis 2. *The positive effect of sanctions on coup attempts is larger in authoritarian regimes.*

Furthermore, Wintrobe (2012) highlights important differences in coup dynamics across types of authoritarian regimes. Authoritarian rulers must not only overpay potential rivals, a requirement common to all autocracies, but also establish internal governance structures that allow them to control elites, especially the military (top-down by keeping C high), while simultaneously providing incentives for these elites to remain loyal (bottom-up by keeping B low) (Wintrobe 2012; Acharya et al. 2011). Thus, an authoritarian regime's vulnerability to sanctions depends on its ability to adapt by creating new incentives, reallocating resources, and, if necessary, intensifying repression.

Escribà-Folch and Wright (2010) differentiate among regime types with regard to their institutional characteristics. According to their argument, personalist regimes, which rely on a narrow base of elite support and external revenues, are particularly susceptible to coups when sanctions disrupt their patronage networks. By contrast, more institutionalized regimes, such as military or single-party systems, are often better equipped to withstand sanctions through repression and cooptation. Yet even this differentiation requires further nuance. Military regimes are indeed typically institutionalized and possess a functioning apparatus of repression, which increases the cost of an unsuccessful coup, C. However, excessive control combined with insufficient incentives for loyalty simultaneously raises the expected payoff of a successful coup, B.

In such contexts, collaboration between internal elites and external actors becomes a plausible strategy for overcoming a repressive government, making sanctions a potentially effective instrument of external intervention.[6]

Although all coup attempts seek to remove the incumbent leader, not all attempts succeed. Failed and successful coups are thus subsets of the broader category of coup events. Because coup plotters cannot know ex ante whether their effort will succeed, it is expected that sanctions influence the likelihood of coup attempts in the same directional manner as they influence successful coups. Whether an attempt ultimately succeeds depends on the probability of success, p, which is largely shaped by the strength of domestic institutions and the number of elites willing to participate (Tullock 2005; Wintrobe 2012; Gassebner et al. 2016). Elite willingness itself is constrained by a fundamental coordination problem, as actors typically possess incomplete information about other elites' intentions. While sanctions modify individual incentives by altering the perceived costs and benefits of staging a coup, they do not directly resolve this coordination challenge. Thus, sanctions raise p only insofar as they erode the institutional capacity of the targeted regime. Consequently, the final hypothesis is proposed:

Hypothesis 3. *The positive effect of sanctions on coups is smaller in more institutionalized authoritarian regimes, whose enhanced repressive and cooptative capacities mitigate elite incentives to defect.*

6. A descriptive analysis indicates that respect for human rights, used here as a proxy for repression, is on average lower in military regimes (–0.84) than in personalist (–0.56) and single-party authoritarian regimes (–0.31). At the same time, the average log of GDP per capita, capturing institutional strength and a regime's capacity for co-optation, is lower in personalist regimes (7.05) than in military (7.33) and single-party regimes (7.58). As expected, democracies exhibit higher values on both human rights (0.92) and log GDP (8.84). All variables are discussed in greater detail later in the paper.

3.3 Data and Estimation Strategy

3.3.1 Data

The data for this study encompass variables that capture political, economic, and demographic characteristics of countries targeted by sanctions. A list of all countries can be found in Table B.1. Table B.2 provides an overview of the variables used, including definitions and data sources. The dependent variable, *Coups*, is a binary indicator representing whether a coup d'état occurred in a given country-year, sourced from the Coup Leaders Dataset (Eschenauer-Engler and Herre 2023).[7] Since not all coup events are successful, all models are re-estimated with the subset of successful coups.

The main independent variable, *Sanctions*, is also binary and indicates whether sanctions were in place for the target country in each year, with data derived from the GSDB (Felbermayr et al. 2020; Kirilakha et al. 2021; Syropoulos et al. 2024).

Figure B.1, Figure B.2, and Figure B.3 in the Appendix illustrate clear regional and temporal variations in the incidence of coups and sanctions. Notably, the data reflect a decline in coup activity after the Cold War, alongside a marked increase in the use of sanctions, particularly from the early 1990s onward. These patterns suggest shifting intervention strategies in the post-Cold War era, with sanctions emerging as a more prominent tool of foreign policy.

The selection of control variables for the probability of coup attempts is based on the theoretical considerations outlined in Section 3.2. Control variables are divided into political and socio-economic categories to account for factors that might confound the relationship between coups and sanctions. Political controls include the *Polity2* indicator of democratic status ranging from strongly democratic (+10) to strongly autocratic (–10), derived from the Polity5 dataset (Marshall and Gurr 2020). Conflicts are divided into *Intra-* and *Interstate conflicts*, as research shows that both types have different effects on the likelihood of coups. Intrastate conflicts typically increase coup risk

7. Alternatively, the coup datasets by Powell and Thyne (2011) or Bjørnskov and Rode (2020) may be used. The dataset by Eschenauer-Engler and Herre (2023) can be considered a novel extension of Powell and Thyne (2011). Furthermore, the baseline results are also estimated using coup data from Bjørnskov and Rode (2020). The results are presented in the Appendix and discussed later in the paper.

by destabilizing governance and empowering the military, while interstate conflicts may initially reduce risk through national unity but later increase it if the conflict is prolonged or poorly managed (Powell 2012; Powell and Thyne 2011, 2016; Belkin and Schofer 2003).

Other political controls are *Human Rights*, *Previous Coup* within the last 10 years, and *Years since the last coup* as detailed in Table B.2. Socio-economic controls include *Population Growth*, *log GDP per capita*, and *Resource Rents Share*. Together, these variables provide a comprehensive set of controls to account for political and socio-economic influences on coup dynamics.

Descriptive statistics for the dataset are presented in Table B.3. The data include 6,070 observations, with 1,354 cases in which sanctions are present (22.3%). Target countries under sanctions tend to have lower *Polity2* scores (indicating more autocratic regimes), lower average human rights scores, and slightly higher levels of resource rents compared to cases without sanctions. Coups occurred 197 times out of the 6,070 observations in the dataset (3.2%), 92 of which successfully replaced the incumbent leader (1.5%). During sanction periods, 35 coups and 19 successful coups occurred, corresponding to 2.6% and 1.4% of the 1,354 sanctioned observations, respectively.

3.3.2 Estimation Strategy

Baseline Estimation Method. The effect of sanctions on coups d'état is estimated following a panel logit approach.[8] This specification enables modeling binary outcomes while addressing unobserved heterogeneity and capturing non-linear trends over time.

$$y^*_{i,t} = \beta S_{i,t-1} + \gamma X^{pol}_{i,t-1} + \delta X^{econ}_{i,t-1} + \alpha_k + \tau_t + \epsilon_{i,t} \tag{2}$$

8. Greene (2004) demonstrates that the bias associated with the incidental parameter problem in panel logit models is minimal. Moreover, the estimation procedure calculates coefficients for fixed effects only when variation exists in the dependent variable within a given data spell. Spells lacking such variation are excluded from the estimation. Additionally, the inclusion of region fixed effects considerably reduces the number of fixed effects compared to employing country fixed effects. Nevertheless, bias corrected results are discussed later in the paper.

In the specified regression model, $y^*_{i,t}$ is the latent variable underlying the binary observed outcome $y_{i,t}$, where $y^*_{i,t}$ equal to one indicates that a coup occurs in country i in year t, and $y^*_{i,t} = 0$ otherwise. The variable $S_{i,t-1}$ represents the primary explanatory variable of interest, the presence of sanctions in country i in year t, which is also binary and hypothesized to influence the likelihood of coups. To address the issue of reverse causality, sanctions are lagged by one year.

Yearly and regional fixed effects, τ_t and α_k respectively, capture broader temporal patterns and region-specific unobserved characteristics.[9] The inclusion of year fixed effects τ_t adjusts for global, time-varying trends.[10] Regional fixed effects α_k help control for unobserved, time-invariant factors specific to geographic areas, such as shared cultural or political contexts that may affect both sanctioning behavior and susceptibility to coups. Regional fixed effects were included instead of country fixed effects because coups are rare events that did not occur in some countries during the research period (see Table B.1). Consequently, the variation in the dependent dummy variable is too low at the country level, and many countries would be excluded from the estimation process due to insufficient within-country variation. Aggregating to the regional level retains a broader sample while still accounting for shared regional characteristics.

Additionally, the model incorporates $X^{pol}_{i,t-1}$ and $X^{econ}_{i,t-1}$, representing lagged political and (socio-)economic control variables for the target country, capturing relevant time-varying conditions that may influence sanctions. These controls account for po-

9. The regional grouping follows the World Bank's classification, with additional subdivisions applied to East Asia and Pacific as well as Europe and Central Asia. Consequently, the fixed effects encompass the following regions: East Asia (14), Oceania/Pacific (5), Europe (5), Central Asia (4), Latin America and Caribbean (42), the Middle East and North Africa (11), South Asia (8), and Sub-Saharan Africa (108). The numbers in parentheses represent the overall number of coups in the particular region according to the dataset.

10. For robustness reasons, US-President fixed effects were included as an alternative to year fixed effects. This approach addresses the potential issue that coups did not occur in some years (2002, 2004, 2007), which might bias the estimation of time effects. US-President fixed effects provide a meaningful alternative temporal categorization, as presidential administrations can influence global foreign policy dynamics, including the imposition of sanctions, based on differing priorities and ideological orientations. This substitution allows for the capture of presidential-term-specific variations in international sanction behavior and its potential influence on coup outcomes. Since the results from models using US-President fixed effects do not substantially differ from those obtained with year fixed effects, I rely on the more conservative baseline models with year fixed effects. However, the model results incl. US-President fixed effects are available on request.

tential confounding factors, as discussed in Section 3.3.1. Finally, $\epsilon_{i,t}$ denotes the error term, accounting for random, idiosyncratic variation in coup occurrence that is not explained by the model's covariates.

In a logit model, the observed binary outcome $y_{i,t}$ is linked to the underlying latent variable $y^*_{i,t}$, where $y_{i,t} = 1$ if $y^*_{i,t} > 0$ and $y_{i,t} = 1$ if $y^*_{i,t} \leq 0$. The probability of observing $y_{i,t} = 1$ is given by the logistic cumulative distribution function (CDF) denoted by $\Lambda(\cdot)$. Specifically,

$$P\left(y_{i,t} = 1 \mid S_{i,t-1}, X^{pol}_{i,t-1}, X^{econ}_{i,t-1}, \tau_t, \alpha_k\right) = \Lambda\left(\beta S_{i,t-1} + \gamma X^{pol}_{i,t-1} + \delta X^{econ}_{i,t-1} + \tau_t + \alpha_k\right) \quad (3)$$

$\Lambda(z) = \frac{e^z}{1+e^z}$ represents the logistic function. In this logit framework, maximum likelihood estimation is used to estimate the parameters, allowing us to interpret the coefficient in terms of changes in the log-odds of observing $y_{i,t} = 1$.

As the coefficients represent changes in the log-odds of the binary outcome rather than direct changes in the probability, interpreting the substantive effect of each predictor on the probability of observing $y_{i,t} = 1$ requires calculating marginal effects. Marginal effects measure the instantaneous rate of change in the probability with respect to a given explanatory variable, holding other variables constant. For the binary variable $S_{i,t-1}$, which represents sanctions, the marginal effect captures the discrete change in the probability of observing $y_{i,j,t} = 1$ when $S_{i,t-1}$ changes from 0 to 1, holding all other variables constant. This discrete marginal effect is calculated as

$$P\left(y_{i,t} = 1 \mid S_{i,t-1} = 1, X^{pol}_{i,t-1}, X^{econ}_{i,t-1}, \tau_t, \alpha_k\right) - P\left(y_{i,t} = 1 \mid S_{i,t-1} = 0, X^{pol}_{i,t-1}, X^{econ}_{i,t-1}, \tau_t, \alpha_k\right). \quad (4)$$

This expression represents the change in probability associated with the occurrence of a coup, allowing us to interpret the impact of $S_{i,t}$ on its likelihood.[11] The standard

11. For a continuous variable captured by variable $X^{pol}_{i,t}$ (similarly for $X^{econ}_{i,t-1}$), the marginal effect is computed as $\frac{\partial P\left(y_{i,t}=1|S_{i,t-1}, X^{pol}_{i,t}, X^{econ}_{i,t-1}, \tau_t, \alpha_k\right)}{\partial X^{pol}_{i,t}} = \Lambda'\left(\beta S_{i,t-1} + \gamma X^{pol}_{i,t} + \delta X^{econ}_{i,t-1} + \tau_t + \alpha_k\right) \cdot \gamma$, where $\Lambda'(z) = \Lambda(z)(1 - \Lambda(z))$ is the derivative of the logistic function, which scales the effect of $X^{pol}_{i,t}$ on the probability of $y_{i,t} = 1$.

errors are clustered at the country level.

Event Study Approach. The second estimation strategy is an event-study design, which compares the risk of coups before, during, and after a period of sanctions. This approach helps assess whether coup risk was already trending upward prior to sanction imposition, thereby separating the effect of sanctions from the factors that triggered them, and traces how the effect evolves over the course of a sanction episode (Dai et al. 2021; Gutmann, Neuenkirch, and Neumeier 2023a; Gutmann, Langer, et al. 2024).

Because coups are rare events, I focus on the three years following the start of a sanction episode, while collapsing all subsequent sanction-years into a single category. The event-study specification is:

$$y^*_{i,t} = \beta_{no} S_{no,i,t} + \sum_{l=2}^{3} \beta_{pre,i,t-l} S_{pre,i,t-l} + \sum_{l=1}^{4+} \beta_{sanc,l} S_{sanc,i,tl} + \sum_{l=1}^{3} \beta_{post,+l} S_{post,i,t+l} \quad (5)$$
$$+ \beta^s_{sanc} S^s_{sanc,i,t} + \gamma X^{pol}_{i,t-1} + \delta X^{econ}_{i,t-1} + \alpha_k + \tau_t + \epsilon_{i,t}$$

The control variables $X^{pol}_{i,t-1}$ and $X^{econ}_{i,t-1}$, the fixed effects α_k and τ_t, and the error term $\epsilon_{i,t}$ are defined as in Eq. (1). The event-time indicators $S_{\text{pre},i,t-l}$, $S_{\text{sanc},i,t+l}$, and $S_{\text{post},i,t+l}$ equal one when country i is in the corresponding year l relative to the start of a sanction episode. Separate dummy variables identify the three years before and the three years after a sanction episode. This structure allows me to test for pre-treatment trends, post-treatment trends, and to trace how the effect of sanctions on coup risk evolves during the early stages of a sanction episode. All coefficients are interpreted relative to the omitted pre-treatment year, $l = -1$, the final year before sanctions begin (Callaway and Sant'Anna 2021). The term $\beta^s_{sanc} S^s_{sanc,i,t}$ captures the effect of sanctions imposed by other, concurrently active senders, ensuring that the estimated event-time coefficients isolate the dynamic effect of the focal sanction sender. $\beta_{no} S_{no,i,t}$ is a binary indicator equal to one for all observations in which neither the sanction variables of interest nor the pre- or post-trend indicators are coded as one. The es-

timation procedure and calculation of marginal effects follows the explanations before.

Propensity-Score Weighting. As Aidt et al. (2021) point out, establishing a causal relationship remains a significant challenge in the empirical study of foreign interventions, such as the effect of sanctions on coups d'état. To strengthen causal identification, I therefore employ propensity-score weighting, which reweights the sample so that treated and untreated observations become comparable on the basis of observed pre-treatment covariates. Formally, the method estimates each observation's propensity score, i.e. the probability that a country-year is a target of sanctions, as a function of pre-treatment political and economic covariates as well as region and year fixed effects.

$$e(X_{i,t-1}) = P(S_{i,t-1} = 1 | X^{pol}_{i,t-1}, X^{econ}_{i,t-1}, \alpha_k, \tau_t) = P(S_{i,t-1} = 1 | X_{i,t-1}), \tag{6}$$

where $S_{i,t-1}$ indicates whether a country–year is subject to sanctions and $X_{i,t-1}$ is the vector of all political, $X^{pol}_{i,t-1}$, and socio-economic, $X^{econ}_{i,t-1}$, covariates included in the treatment model. For constructing the average treatment effect on the treated (ATT), I set:

$$w_{i,t-1} = \begin{cases} 1, & \text{if } S_{i,t-1} = 1, \\ \dfrac{e(X_{i,t-1})}{1 - e(X_{i,t-1})}, & \text{if } S_{i,t-1} = 0. \end{cases} \tag{7}$$

These weights reweight the non-sanctioned observations so that their distribution of pre-treatment characteristics increasingly resembles that of the sanctioned observations. I assess covariate balance using standardized mean differences (SMDs) and visual diagnostic plots. Following conventional thresholds, balance is considered satisfactory when $|SMD| < 0.1$ across all covariates.

In the second stage, I estimate the effect of sanctions on coup occurrence using a weighted fixed-effects logit model with the specification in Eq. 1, where $X_{i,t-1}$ denotes the combined vector of political and economic controls:

$$y^*_{i,t} = \beta S_{i,t-1} + \theta X_{i,t-1} + \alpha_k + \tau_t + \epsilon_{i,t}, \tag{8}$$

The model is estimated on the weighted sample using cluster-robust standard errors at the country level.

This two-step procedure yields a doubly robust estimator: causal identification is valid if either the propensity-score model or the outcome model is correctly specified. By reweighting the control observations to approximate the covariate distribution of sanctioned observations and then estimating a fixed-effects logit on the weighted sample, this approach reduces bias stemming from the non-random assignment of sanctions and strengthens the credibility of the estimated impact of sanctions on coup risk.

Further Approaches. Because coups are rare events, standard maximum-likelihood logit estimation is susceptible to small-sample bias and may even fail to produce finite coefficient estimates when the data exhibit (quasi-)separation. To mitigate these issues, I additionally estimate a Firth penalized logit model, which is specifically designed for binary outcomes with low event frequencies by introducing a bias-reducing penalty to the log-likelihood function.

To summarize, the construction of appropriate counterfactuals, the event-study design, and the consistent one-year lag structure for sanction variables jointly support a causal interpretation of the estimated results. Moreover, the robustness analyses, including the use of bias-reducing estimation methods and alternative estimators such as probit and linear probability models, further strengthen the credibility of the empirical strategy.

3.4 Empirical Results

3.4.1 Full Research Period Results

The baseline models are estimated in a stepwise manner. First, only clearly exogenous variables are included, namely, the socio-economic variables lagged by one year, the previous coup dummy and the time elapsed since the last coup event. Columns (1) to (3) of Table 3.1 report the marginal effects from the panel logit estimations, using (1) and (2) all coups and (3) successful coups only as the dependent variable. When restricting the outcome to successful coups, a larger number of country–year spells are absorbed by the fixed effects because fewer observations exhibit sufficient within-unit variation. To demonstrate that differences between the models using all coups and those using only successful coups are not merely an artifact of the absorbed observations, I re-estimate the coup models using the identical restricted sample, reported in columns (2) and (5).[12]

Lagged Western sanctions are, on average, associated with a 1.7 to 1.9 pp increase in the probability of both coup attempts and successful coup occurrences. The effects are statistically significant at least at the 5% significance level in both specifications. Considering that the overall likelihood of a coup attempt in the dataset is 3.2% (i.e., 197 coups out of 6,070 observations), this represents a relatively strong impact. The effect is even relatively more pronounced for successful coups, where the overall likelihood is 1.5% (i.e., 92 successful coups out of 6,070 observations), implying that Western sanctions more than double the likelihood of a successful coup. By contrast, non-Western sanctions exhibit no statistically significant effect on coup likelihood.

Second, columns (4) to (6) in Table 3.1 present the marginal effects of the panel logit estimations that incorporate the full set of variables, including controls for the political situation. In these models, the estimates for sanctions decrease in magnitude and lose statistical significance. Given the reciprocal relationship between the imposition of sanctions and the political situation in the target country, these results are not unex-

12. For robustness, all baseline models were re-estimated using a panel probit approach. The marginal effects are nearly identical. Results are available upon request.

pected. However, the effect of lagged Western sanctions on successful coups remains highly significant with an effect size of 1.7 pp. The years elapsed since the most recent coup emerge as a significant predictor of coup occurrences, whereas the likelihood of a successful coup is more strongly influenced by whether a coup occurred within the past 10 years. Specifically, the probability of a coup decreases by 0.1 pp for each additional year since the last coup. Lagged GDP per capita significantly reduces the probability of both a coup and a successful coup. Whereas interstate conflicts do not significantly affect coups, intrastate conflicts increase the likelihood of a coup d'état by around 2.0 pp. However, the relationship between intrastate conflicts and successful coups is not statistically significant. Human rights appear to have only an effect on coup events but not successful coups. The level of democratization is not a significant predictor, but additional analysis on that is provided in the following subsection.

The models presented in (4) to (6) in Table 3.1, including all control variables, can be considered as conservative specification because it risks underestimating the effect of lagged sanctions by controlling for variables that may themselves be related to sanctions. Therefore, the true effect of lagged sanctions on coups lies between the marginal effects estimated by the small model (upper bound) and the full model (lower bound).

Table 3.1: Baseline Results

	Coups	Coups	Successful Coups	Coups	Coups	Successful Coups
	(1)	(2)	(3)	(4)	(5)	(6)
Sanctions						
… lag Western	0.0172**	0.0193***	0.0187***	0.0092	0.0112	0.0172***
	(0.0075)	(0.0082)	(0.0049)	(0.0081)	(0.0088)	(0.0055)
… lag Non-Western	0.0032	0.0042	0.0088	–0.0097	–0.0085	0.0063
	(0.0159)	(0.0177)	(0.0108)	(0.0169)	(0.0185)	(0.0111)
lag Polity2				0.0004	0.0003	0.0003
				(0.0006)	(0.0007)	(0.0005)
lag Human Rights				–0.0083**	–0.0078*	–0.0017
				(0.0039)	(0.0045)	(0.0031)
lag Intrastate Conflict				0.0198**	0.0222**	0.0091
				(0.0080)	(0.0090)	(0.0065)
lag Interstate Conflict				0.0273	0.0331*	0.0100
				(0.0171)	(0.0192)	(0.0131)
Previous Coup (10 years)	0.0094	0.0108	0.0121**	0.0060	0.0077	0.0116**
	(0.0069)	(0.0080)	(0.0048)	(0.0070)	(0.0082)	(0.0051)
Years since last coup	–0.0009**	–0.0012**	–0.0003	–0.0011**	–0.0013**	–0.0004
	(0.0004)	(0.0005)	(0.0003)	(0.0004)	(0.0005)	(0.0003)
lag Population Growth	0.0165	0.0179	–0.0144	0.0292	0.0325	–0.0111
	(0.0297)	(0.0344)	(0.0218)	(0.0271)	(0.0313)	(0.0205)
lag *log*(GDP pc)	–0.0142***	–0.0156***	–0.0082**	–0.0110**	–0.0125**	–0.0078**
	(0.0042)	(0.0049)	(0.0035)	(0.0043)	0.0050	(0.0037)
lag Resources Rents Share	–0.0003	-0.0009*	–0.0003	–0.0004	–0.0010	–0.0003
	(0.0004)	(0.0005)	(0.0003)	(0.0004)	(0.0005)	(0.0003)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,419	4,570	4,570	5,419	4,570	4,570

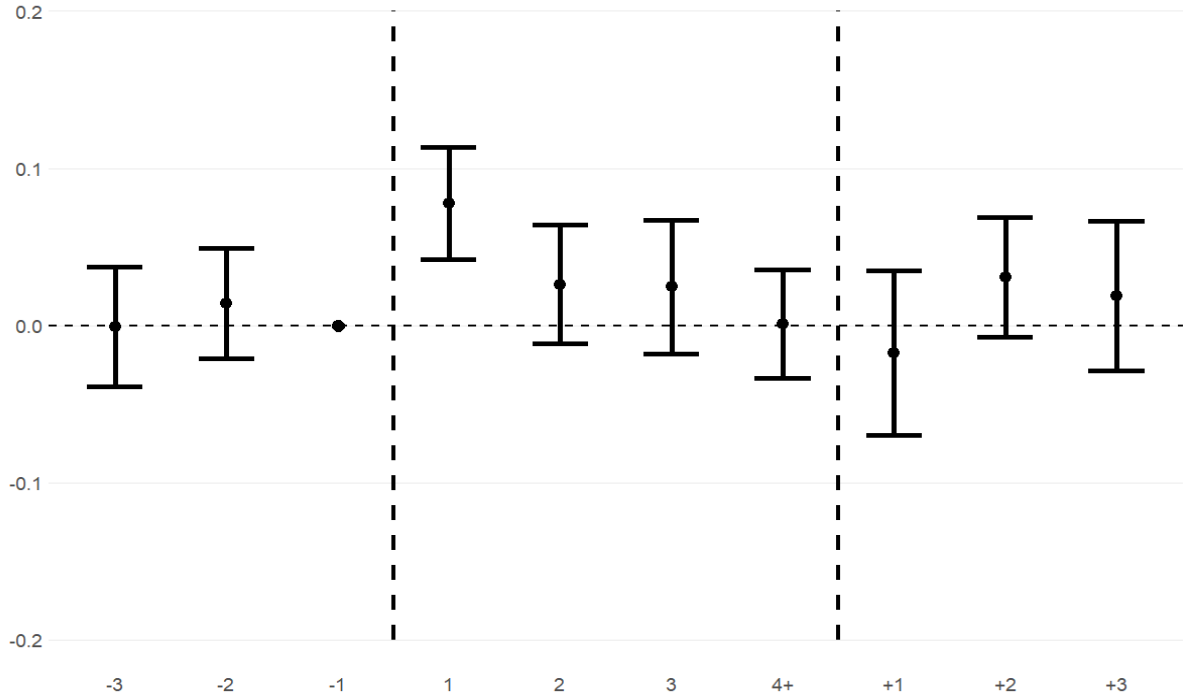
Notes: Marginal effects of panel logit models according to Eqs. (1)–(4). The LHS variable indicates whether a coup occurred in country i at year t. Standard errors are clustered at the country level. ***/**/* indicates significance at the 1%/5%/10% level. The differences in the number of observations are due to the absorption of data spells by the fixed effects.

Effects of Sanctions over Time. To support a causal interpretation of the results, an event study design is applied. Implementing such an approach in the context of coups is challenging due to the low number of coup events. As a consequence, several adjustments are necessary: (i) all sanction-years beyond the third year are collapsed into a single category, (ii) the analysis of Western sanctions focuses on all coup events, and (iii) the dynamic effects of non-Western sanctions cannot be estimated within the event-study framework. Nonetheless, non-Western sanctions are included as controls in all specifications, and their average effect is statistically insignificant (see Table 3.1).

The insignificant coefficients for the pre- and post-treatment periods displayed in Figure 3.1 indicate that coup risk does not rise prior to the imposition of Western sanctions nor immediately after sanctions are lifted. The results show that the initial imposition of Western sanctions has the strongest effect, increasing the probability of a coup by approximately 7.8 pp. Although coup risk remains elevated in the two subsequent periods, these estimates are not statistically significant. This temporal pattern is consistent with the findings of Gutmann, Neuenkirch, and Neumeier (2023a), who document that the economic effects of sanctions tend to be most pronounced in the first year and diminish over time.

Overall, the baseline results support hypotheses 1a, stating that sanctions, here Western sanctions, increase the likelihood of coups as well as successful coups. Hypotheses 2 and 3, addressing the heterogeneity related to the particular regime type, are tested in the following.

Figure 3.1: Coups and the Effect of Western Sanctions over Time.



Notes: Effects of sanctions over time (1, 2, 3, 4+) alongside pre-trend (-3, -2, -1) and post-trend (+1, +2, +3) according to an estimation of Eq. 5 for coups as dependent variable. Standard errors are clustered by country. Models include control variables (lag Non-Western sanctions, lag Polity2, lag Human Rights, lag Intrastate and Interstate Conflicts, Previous Coup (10 years), Years since last coup, lag Population Growth, lag log(GPD pc), and lag Resources Rents Share), region and year fixed effects. 95% confidence bands are indicated by whiskers.

Effect Heterogeneity regarding Regime Type. The extended analysis excludes one of the four regime types (Military, Personal, Party, and Democratic) at a time and re-estimates the models including all control variables, similar to columns (4) and (6) in Table 3.1.[13]

Table 3.2 shows that sanctions do not exert a significant effect on coup attempts in any specification. However, substantial heterogeneity emerges once the focus shifts to successful coups. When democratic regimes are excluded and the analysis is restricted to authoritarian systems, lagged Western sanctions markedly increase the probability of a successful coup, indicating that the positive effect identified in the baseline is largely driven by autocratic contexts. This finding provides support for Hypothesis 2.

13. Although interacting regime types with sanction indicators is the standard approach in the literature, the very low frequency of coup events makes such interaction models infeasible in this context. A jackknife-style strategy therefore provides the only practical way to meaningfully assess heterogeneity across regime types.

Digging deeper into the heterogeneity within autocratic regimes reveals an even more nuanced pattern. Excluding single-party regimes increases the estimated effect of Western sanctions on successful coups to 2.4 pp, significant at the 1% level. Excluding military regimes, by contrast, reduces the estimated effect, suggesting that both personalist and military autocracies are particularly susceptible to successful coups under sanction pressure. These divergent effects across authoritarian regime types highlight the importance of institutional configurations and elites' incentives in shaping regime vulnerability to sanctions. This heterogeneity is explored in greater detail in a subsequent subsection, drawing on a post-Cold War sample.

Effect Heterogeneity regarding Objective of Sanctions. Furthermore, the GSDB identifies nine non-exclusive categories of objectives or motivations for imposing sanctions – democracy promotion, human rights protection, regime destabilization, policy change, war prevention, conflict resolution, territorial disputes, counterterrorism, and other miscellaneous goals – based on data derived from official documents. To examine whether the influence of sanctions on coups d'état differs depending on the specific objectives, I re-estimate the models including all controls while sequentially excluding sanction cases associated with each objective category. Table B.4 in the Appendix reveals the results for the full research period. Again, there is no significant effect of sanctions, neither of Western nor of non-Western sanctions, on coups in any of the specifications. However, it is found that the effect of Western sanctions on successful coups is mainly driven by sanctions aiming for democratization in the target country, as the significantly positive relationship vanishes if this objective is removed.

Table 3.2: Effect Heterogeneity for Regime Types

		Coups	
	Excl. Military Regimes	**Excl. Personalist Regimes**	**Excl. Single-Party Regimes**
Lag Western	0.0006	0.0050	0.0163
	(0.0076)	(0.0083)	(0.0101)
Lag Non-Western	–0.0045	–0.0169	0.0052
	(0.0119)	(0.0174)	(0.0202)
Observations	4,727	4,554	4,107
	Excl. Democratic Regimes	**Full Dataset**	
Lag Western	0.0173	0.0092	
	(0.0147)	(0.0081)	
Lag Non-Western	–0.0251	–0.0097	
	(0.0374)	(0.0169)	
Observations	2,786	5,419	
		Successful Coups	
	Excl. Military Regimes	**Excl. Personalist Regimes**	**Excl. Single-Party Regimes**
Lag Western	0.0110*	0.0119*	0.0241***
	(0.0066)	(0.0061)	(0.0077)
Lag Non-Western	0.0120	0.0070	0.0125
	(0.0075)	(0.0117)	(0.0153)
Observations	2,897	3,851	3,301
	Excl. Democratic Regimes	**Full Dataset**	
Lag Western	0.0287***	0.0172***	
	(0.0107)	(0.0055)	
Lag Non-Western	–0.0075	0.0063	
	(0.0277)	(0.0111)	
Observations	2,407	4,570	

Notes: Marginal effects of panel logit models according to Eqs. (1)–(4). LHS variable: (successful) coup in country i at year t. Standard errors are clustered by country. ***/**/* indicates significance at the 1%/5%/10% level. The difference in the number of observations are due to (i) the absorption of data spells by the FEs and (ii) the exclusion of specific regime types. Models include all controls, region- and year-fixed effects.

Effect Heterogeneity regarding Coup Type. As outlined in Section 3.2, coups are predominantly carried out by the military. In the dataset, 183 out of 197 recorded coups were executed by military actors, while only 14 were led by civilian elites. Thus, civilian coups are removed from the dataset to test for potential effect heterogeneity. The results for the impact on military coups remain insignificant, while the estimate for successful military coups loses its significance (see Table B.6).

3.4.2 Post-Cold War Results

Cold War Sample Split. As sanctions have been increasingly used as a tool in international politics since the beginning of the 1990s, all models are re-estimated with a sample split in 1991 when the Soviet Union collapsed and the Cold War came to an end. Columns (1) and (3) in Table 3.3 present the marginal effects for the Cold War era (here 1971 to 1991), while (2) and (4) include the post-Cold War results (1992 to 2019).

The statistical results corroborate the growing relevance of sanctions since the 1990s. Prior to 1992, sanctions exhibit no significant effect on the probability of coups or successful coups. In the post–Cold War period, however, the estimated impact becomes both positive and statistically significant. Western sanctions increase the likelihood of coups by approximately 1.8 pp on average, and by more than 2.2 pp when focusing solely on successful coups, providing further support for Hypothesis 1a.

Moreover, the determinants of coups shift markedly when comparing the Cold War and post-Cold War eras. Internal conflict, which exerted a strong and highly significant positive influence on coup risk during the Cold War, no longer shows a statistically significant effect after 1992. By contrast, Western sanctions emerge as one of the most influential, if not the most influential, predictors of coup activity in the post-Cold War era.

Table 3.3: Cold War Sample Split

	Coups		Successful Coups	
	(1)	(2)	(3)	(4)
	Cold War	Post-1991	Cold War	Post-1991
Sanctions				
… lag Western	–0.0239	0.0182***	–0.0100	0.0227***
	(0.0245)	(0.0058)	(0.0172)	(0.0060)
… lag Non-Western	–0.0152	0.0015	0.0216	0.0105
	(0.0474)	(0.0123)	(0.0236)	(0.0118)
lag Polity2	–0.0017	0.0013**	–0.0010	0.0013**
	(0.0014)	(0.0006)	(0.0010)	(0.0005)
lag Human Rights	–0.0085	–0.0051	0.0034	–0.0036
	(0.0089)	(0.0031)	(0.0055)	(0.0035)
lag Intrastate Conflict	0.0489***	0.0056	0.0240**	0.0045
	(0.0160)	(0.0077)	(0.0119)	(0.0083)
lag Interstate Conflict	0.0612*	0.0029	0.0178	0.0149
	(0.0361)	(0.0232)	(0.0303)	(0.0135)
Previous Coup (10 years)	0.0080	0.0023	0.0097	0.0064
	(0.0135)	(0.0063)	(0.0082)	(0.0066)
Years since last coup	–0.0030***	–0.0004	–0.0005	–0.0003
	(0.0012)	(0.0003)	(0.0007)	(0.0003)
lag Population Growth	–0.0062	0.0140	–0.0960	0.0001
	(0.0899)	(0.0156)	(0.0725)	(0.0131)
lag *log*(GDP pc)	–0.0123	–0.0106***	–0.0131*	–0.0077**
	(0.0097)	(0.0034)	(0.0075)	(0.0034)
lag Resources Rents Share	–0.0021**	0.0002	–0.0008	–0.0002
	(0.0009)	(0.0003)	(0.0006)	(0.0003)
Region FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	2,103	3,316	1,988	2,417

Notes: Marginal effects of panel logit models according to Eqs. (1)–(4). The LHS variable indicates whether a coup occurred in country i at year t. Standard errors are clustered at the country level. ***/**/* indicates significance at the 1%/5%/10% level. The differences in the number of observations are due to (i) the absorption of data spells by the fixed effects and (ii) the Cold War sample split.

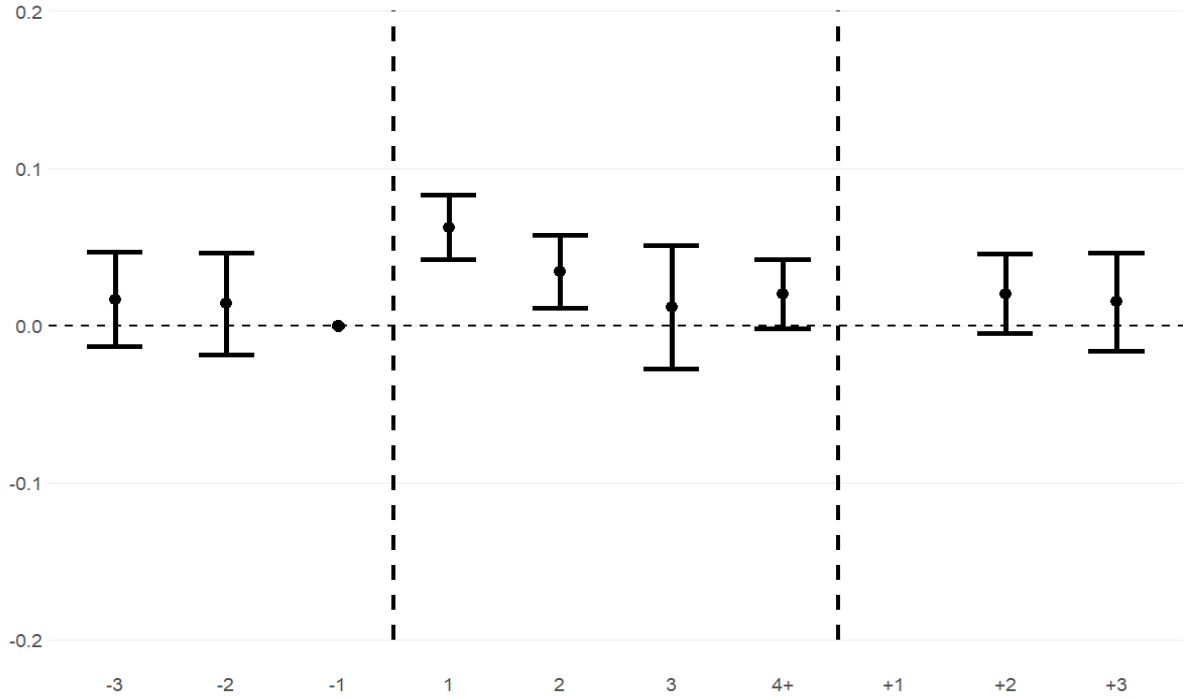
A likely explanation is that during the Cold War, major powers often relied on direct (covert or overt) interventions to influence the internal affairs of target countries. According to Aidt et al. (2021), an estimated 12% of the global population lived in countries targeted by CIA or KGB intervention in an average year during the Cold War, with this share rising to as much as 25% at its peak in the late 1960s. In contrast, since the 1990s, sanctions have become a more commonly employed political

instrument (Felbermayr et al. 2020; Berger et al. 2013). Interestingly, while the post-Cold War period is marked by an increase in the use of sanctions and a decline in coup frequency, trends that may appear antagonistic, the estimated effects of sanctions on coup likelihood are more pronounced. This contrast suggests that the relationship is not driven by concurrent trends, but may reflect a more robust underlying (causal) relationship, particularly in the post-Cold War context. The opposing trends are illustrated in Figure B.1 and Figure B.2 in the Appendix.

Effects of Sanctions over Time. The average post-Cold War findings are further supported by the event study results in Figure 3.2. These estimates indicate that lagged Western sanctions initially increase the probability of a coup by roughly 6.2 pp. The effect remains statistically significant in the second year, with an estimated increase of about 3.4 pp. Thereafter, the coefficients remain positive but lose statistical significance.[14]

14. The coefficient for the first period after sanctions are lifted is omitted because no coups are observed in that interval.

Figure 3.2: Post-Cold War: Coups and the Effect of Western Sanctions over Time.



Notes: Effects of sanctions over time (1, 2, 3, 4+) alongside pre-trend (-3, -2, -1) and post-trend (+1, +2, +3) according to an estimation of Eq. 5 for coups as dependent variable. Standard errors are clustered by country. Models include control variables (lag Non-Western sanctions, lag Polity2, lag Human Rights, lag Intrastate and Interstate Conflicts, Previous Coup (10 years), Years since last coup, lag Population Growth, lag log(GPD pc), and lag Resources Rents Share), region and year fixed effects. 95% confidence bands are indicated by whiskers.

Effect Heterogeneity Regarding Regime Type. The post-Cold War results excluding different regime types reveal marked heterogeneity in the impact of Western sanctions. As shown in Table 3.4, the estimated effects on both coups (4.6 pp) and successful coups (5.8 pp) are significantly stronger in authoritarian regimes. This confirms that the overall post-Cold War relationship is primarily driven by autocratic contexts.

Within authoritarian regimes, the findings further align with theoretical expectations. Single-party regimes appear substantially more resilient to Western sanctions. Once they are excluded from the sample, the estimated effects become even larger, reflecting the comparatively strong institutional structures and cooptative capacities of single-party systems. Differences also emerge between military and personalist regimes. Western sanctions exert a stronger effect on the probability of any coup in military regimes, but a weaker effect on the likelihood of successful coups compared to personalist regimes. This suggests that while military regimes may generate high

incentives for coup plotting under sanction pressure, they remain relatively more capable of resisting such attempts than personalist regimes during the post-Cold War period. Taken together, the post-Cold War results of Western sanctions provide strong support for all hypotheses:

(1a) Western sanctions increase the likelihood of coups; (2) this positive effect is amplified in authoritarian regimes; and (3) within authoritarianism, the effect is attenuated in regimes with stronger institutional and cooptative capacities, most notably single-party regimes.

Effect Heterogeneity regarding Objectives of Sanctions. Table B.5 in the Appendix shows that the marginal effects of Western sanctions on coups become stronger and more statistically significant when cases aimed at improving human rights or inducing policy change in the target country are excluded. In these restricted samples, the probability of a coup increases by 2.3 and 2.4 pp, respectively. By contrast, the estimated effects diminish when sanction episodes targeting democratization or regime destabilization are excluded, an intuitive result given that coups themselves may constitute the outcome of interest. For successful coups, the pattern is similar but more pronounced across all specifications.

Effect Heterogeneity regarding Coup Type. The post-Cold War results presented in Table B.6 indicate that the effect of Western sanctions on the likelihood of military coups, and successful military coups, is slightly lower than the average effect observed when all coup events are included. However, the patterns remain largely similar. The findings suggest that the impact of sanctions on the probability of (successful) civilian-led coups may be moderately higher. This inference cannot be further empirically tested due to the limited number of civilian coup observations.

Table 3.4: Post-Cold War Effect Heterogeneity for Regime Types

		Coups	
	Excl. Military Regimes	**Excl. Personalist Regimes**	**Excl. Single-Party Regimes**
Lag Western	0.0118***	0.0150**	0.0168***
	(0.0044)	(0.0063)	(0.0064)
Lag Non-Western	0.0024	–0.0065	–0.0007
	(0.0087)	(0.0162)	(0.0142)
Observations	3,060	2,757	2,606
	Excl. Democratic Regimes	**Full Dataset**	
Lag Western	0.0464***	0.0182***	
	(0.0161)	(0.0058)	
Lag Non-Western	0.0265	0.0015	
	(0.0279)	(0.0123)	
Observations	1,297	3,316	
		Successful Coups	
	Excl. Military Regimes	**Excl. Personalist Regimes**	**Excl. Single-Party Regimes**
Lag Western	0.0228***	0.0187***	0.0246***
	(0.0070)	(0.0064)	(0.0065)
Lag Non-Western	0.0131	0.0100	0.0082
	(0.0098)	(0.0133)	(0.0141)
Observations	1,303	2,176	1,771
	Excl. Democratic Regimes	**Full Dataset**	
Lag Western	0.0580***	0.0227***	
	(0.0176)	(0.0060)	
Lag Non-Western	0.0291	0.0105	
	(0.0349)	(0.0118)	
Observations	875	2,417	

Notes: Marginal effects of panel logit models according to Eqs. (1)–(4). LHS variable: (successful) coup in country i at year t. Standard errors are clustered by country. ***/**/* indicates significance at the 1%/5%/10% level. The difference in the number of observations are due to (i) the absorption of data spells by the FEs, and (ii) the exclusion of regime types. Models include all controls, region- and year-FEs.

3.4.3 Causality, Extensions and Robustness

Robustness to Heterogeneity between Treatment & Control. The robustness of the baseline results, as well as the causal interpretation of the underlying mechanisms, is further evaluated using additional estimation techniques. Propensity-score weighting yields results that are highly consistent with, and in some cases even stronger than, the baseline estimates. The effect of Western sanctions on coups remains statistically significant at the 10% level even when controlling for the full set of covariates. For successful coups, the marginal effect increases to 2.1 pp, and even non-Western sanctions exhibit a positive and mildly significant association. Among all predictors, Western sanctions continue to stand out as the most influential determinant of successful coups (see Table B.7).[15]

Robustness to alternative Estimation Approaches. As shown in Table B.8, the results obtained using Firth's penalized logit estimator, which addresses rare-event bias in the context of infrequent coup occurrences, are fully consistent with the baseline findings. In addition, correcting for potential incidental parameter bias in the logit framework using the method proposed by Fernández-Val (2009), as well as estimating linear probability models, further demonstrates that the main results are robust to a range of statistical concerns (see Table B.9 and Table B.10). All core findings remain stable across these alternative estimation strategies.

Robustness to alternative Coup Data. Additionally, to ensure robustness of my findings, Table B.11 presents the baseline estimation results using coup data from Bjørnskov and Rode (2020) instead of Eschenauer-Engler and Herre (2023). Under this specification, the marginal effects of lagged Western sanctions appear even more pronounced.

15. Re-estimating the baseline logit models after excluding Europe and North America, an alternative strategy to address systematic regional differences influencing the comparability between treated and control cases, produces nearly identical results, further reinforcing the robustness of the main findings. Results are available upon request.

3.5 Conclusion

In the present paper, a panel logit model is employed to estimate the impact of international sanctions on the likelihood of coups in the target country. The dataset covers 6,070 observations, with 1,354 cases in which sanctions are present and 197 coups occurred in the target countries between 1971 and 2019. 92 coups out of the 197 were successful.

The key findings indicate that Western sanctions exert a positive effect on the probability of coups d'état. In the post-Cold War era, Western sanctions emerge as one of the strongest predictors of coups. On average, they increase coup likelihood by 1.8 percentage points during this period, effectively doubling the probability of a coup, given that coups are rare events occurring in only 1.7% of observations in the post-Cold War subsample. Focusing exclusively on successful coups, Western sanctions raise their likelihood by 2.3 percentage points, corresponding to an almost tripling increase relative to the baseline probability of 0.8% after 1991. By contrast, non-Western sanctions do not exhibit a statistically significant impact. When considering the full sample period, the results display a similar but weaker and less significant pattern.

Furthermore, the results across different regime types and sanctions objectives are fully consistent with theoretical expectations. The positive effect of sanctions on coup likelihood is driven primarily by authoritarian regimes, where sanctions increase the probability of a coup by about 4.6 pp. Within the broader category of authoritarianism, the effect is particularly pronounced in personalist and military regimes. With respect to sanctions objectives, the estimated effect becomes weaker and less statistically significant when the objective of democratization is excluded, but it becomes stronger when the objectives of human rights improvements and policy change are removed. Similar, though slightly stronger, patterns emerge when focusing solely on successful coups as the dependent variable.

Regarding the temporal impact, the event study shows that Western sanctions initially raise the probability of a coup by 7.8 pp, followed by a diminishing positive,

though statistically insignificant, effect in subsequent years. Similar temporal patterns emerge for the post-Cold War period.

The causal interpretation and robustness of the overall findings are further reinforced by propensity-score weighting and additional bias-correction techniques, including Firth's penalization method and incidental parameter bias correction.

For sanction senders, the results suggest that sanctions can indeed destabilize authoritarian regimes by increasing the likelihood of a coup. However, the effect depends crucially on how elites within the target country perceive and experience the sanctions. Do elites interpret sanctions as a broad external threat directed at the entire ruling coalition, or as pressure focused specifically on the incumbent leadership? In this context, smart sanctions targeting particular individuals, entities, or sectors, may be a more effective tool for exerting selective pressure on the leading elites while minimizing harm to ordinary citizens.

At the same time, policymakers must recognize that a change in leadership following a successful coup does not necessarily produce better outcomes for the population or for international relations between the sanction sender and the target state. Coups can generate substantial political instability and unintended consequences that lie beyond the scope of this paper. Future research could delve more deeply into elite-level dynamics within a smaller set of countries to further empirically assess the costs and benefits of coups under sanction conditions. Such an approach would help illuminate the mechanisms at play within elite networks, information that is typically not publicly accessible and thus cannot be captured in the large-N panel analysis conducted here.

Appendix B

Appendix B1: Data Description

Table B.1: List of Countries

Target Countries (Number of total observations; sanctions; coups; successful coups)

Afghanistan (4; 3; 0; 0), Albania (34; 3; 0; 0), Algeria (47; 11; 1; 1), Angola (33; 14; 0; 0), Argentina (48; 13; 3; 1), Armenia (26; 0; 0; 0), Australia (48; 4; 0; 0), Austria (48; 5; 0; 0), Azerbaijan (24; 8; 1; 0), Bahrain (37; 0; 0; 0), Bangladesh (45; 0; 5; 4), Belarus (26; 18; 0; 0), Belgium (48; 4; 0; 0), Benin (48; 12; 2; 1), Bhutan (37; 0; 0; 0), Bolivia (48; 12; 7; 5), Botswana (48; 0; 0; 0), Brazil (48; 4; 0; 0), Bulgaria (37; 8; 0; 1), Burkina Faso (48; 0; 7; 4), Burundi (48; 10; 6; 4), Cambodia (24; 12; 1; 1), Cameroon (47; 7; 1; 0), Canada (20; 15; 0; 0), Cape Verde (31; 0; 0; 0), Central African Republic (48; 13; 4; 3), Chad (46; 1; 6; 3), Chile (48; 21; 1; 1), China (48; 38; 0; 0), Colombia (48; 19; 0; 0), Comoros (37; 0; 7; 2), Costa Rica (48; 17; 0; 0), Croatia (22; 10; 0; 0), Cuba (47; 46; 0; 0), Cyprus (42; 31; 0; 0), Czech Republic (24; 4; 0; 0), Denmark (48; 4; 0; 0), Djibouti (4; 0; 0; 0), Dominican Republic (48; 7; 0; 0), Ecuador (48; 8; 5; 3), Egypt (48; 16; 2; 1), El Salvador (48; 8; 2; 1), Equatorial Guinea (33; 9; 0; 0), Eritrea (16; 6; 0; 0), Estonia (22; 6; 0; 0), Ethiopia (32; 13; 1; 0), Fiji (47; 22; 3; 4), Finland (48; 4; 0; 0), France (48; 27; 0; 0), Gabon (48; 0; 0; 0), Gambia (48; 13; 2; 1), Georgia (26; 8; 0; 1), Germany (46; 5; 0; 0), Ghana (48; 0; 7; 4), Greece (48; 7; 1; 1), Guatemala (48; 33; 4; 2), Guinea (31; 15; 3; 1), Guinea-Bissau (43; 8; 8; 5), Guyana (48; 0; 0; 0), Haiti (48; 32; 4; 5), Honduras (48; 1; 6; 4), Hungary (26; 4; 0; 0), India (47; 37; 0; 0), Indonesia (44; 24; 0; 0), Iran (44; 33; 0; 0), Iraq (33; 24; 3; 0), Ireland (47; 20; 0; 0), Israel (21; 0; 0; 0), Italy (48; 4; 0; 0), Jamaica (48; 7; 0; 0), Japan (48; 0; 0; 0), Jordan (41; 4; 0; 0), Kazakhstan (26; 2; 0; 0), Kenya (48; 8; 1; 0), Kuwait (25; 0; 0; 0), Kyrgyzstan (26; 0; 0; 1), Laos (33; 10; 0; 0), Latvia (22; 6; 0; 0), Lebanon (13; 12; 0; 0), Lesotho (48; 4; 4; 2), Liberia (17; 15; 0; 0), Libya (16; 15; 0; 0), Lithuania (22; 5; 0; 0), Luxembourg (48; 4; 0; 0), Madagascar (48; 9; 4; 2), Malawi (48; 5; 0; 0), Malaysia (48; 0; 0; 0), Mali (45; 2; 3; 1), Mauritania (48; 4; 7; 5), Mauritius (41; 0; 0; 0), Mexico (48; 0; 0; 0), Moldova (22; 15; 0; 0), Mongolia (36; 0; 0; 0), Montenegro (11; 3; 0; 0), Morocco (48; 0; 2; 0), Mozambique (27; 0; 0; 0), Myanmar (44; 27; 2; 1), Namibia (27; 0; 0; 0), Nepal (48; 1; 0; 1), Netherlands (48; 4; 0; 0), New Zealand (40; 0; 0; 0), Nicaragua (48; 17; 0; 0), Niger (48; 8; 6; 4), Nigeria (47; 21; 6; 4), North Macedonia (26; 0; 0; 0), Norway (48; 8; 0; 0), Oman (47; 0; 0; 0), Pakistan (44; 19; 3; 2), Panama (47; 10; 2; 0), Papua New Guinea (42; 0; 2; 0), Paraguay (48; 5; 2; 1), Peru (47; 10; 3; 2), Philippines (45; 16; 4; 0), Poland (27; 4; 0; 0), Portugal (48; 8; 2; 1), Qatar (17; 0; 0; 0), Romania (27; 9; 0; 0), Russia (25; 3; 0; 0), Rwanda (48; 17; 2; 2), Saudi Arabia (48; 0; 0; 0), Senegal (48; 0; 0; 0), Serbia (21; 17; 0; 0), Sierra Leone (48; 15; 6; 4), Singapore (48; 0; 0; 0), Slovakia (24; 4; 0; 0), Slovenia (22; 4; 0; 0), Solomon Islands (35; 0; 0; 1), Somalia (4; 3; 0; 0), South Africa (46; 26; 0; 0), South Korea (48; 5; 0; 2), South Sudan (3; 2; 0; 0), Spain (48; 4; 1; 0), Sri Lanka (47; 0; 0; 0), Sudan (46; 28; 7; 2), Suriname (42; 7; 1; 2), Swaziland (47; 0; 1; 0), Sweden (48; 4; 0; 0), Switzerland (37; 0; 0; 0), Syria (48; 23; 1; 1), Tajikistan (26; 0; 0; 0), Tanzania (29; 3; 0; 0), Thailand (46; 9; 7; 6), Timor Leste (4; 0; 0; 0), Togo (48; 16; 2; 1), Tunisia (48; 7; 1; 1), Turkey (44; 11; 3; 2), Turkmenistan (26; 3; 0; 0), Uganda (35; 0; 1; 1), Ukraine (25; 9; 1; 0), United Arab Emirates (42; 0; 1; 0), United Kingdom (47; 4; 0; 0), Uruguay (48; 5; 2; 1), USA (46; 4; 0; 0), Uzbekistan (26; 10; 0; 0), Vietnam (33; 23; 0; 0), Yemen (27; 6; 0; 0), Zambia (48; 3; 3; 0), Zimbabwe (37; 21; 1; 1).

Table B.2: Definitions of Variables and Data Sources

Variable	Definition & Source
Coups	Binary indicator for country-years in which coups have occurred. *Source:* Coup Leaders Dataset (Eschenauer-Engler and Herre 2023).
Successful Coups	Binary indicator for country-years in which successful coups have occurred. *Source:* Coup Leaders Dataset (Eschenauer-Engler and Herre 2023).
lag Sanctions	Binary indicators for country-years with sanctions in place, lagged by one year. *Source:* GSDB (Felbermayr et al. 2020; Kirilakha et al. 2021; Syropoulos et al. 2024).
lag Polity2	Democracy indicator that ranges from strongly democratic (+10) to strongly autocratic (–10), lagged by one year. *Source:* Polity5 dataset (Marshall and Gurr 2020).
Human Rights	Latent human rights variable with higher values indicating a better protection of human rights, lagged by one year. *Source:* Human Rights Protection Scores (Fariss 2019).
Intrastate Conflict / Interstate Conflict	Armed conflict with at least 25 battle-related deaths between a government and one or more rebel groups / between two states, lagged by one year. *Source:* UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002; Davies et al. 2022).
Previous Coups (10 years)	Binary indicator for a country in which a coup has occurred in the last 10 years. *Source:* Coup Leader Data (Eschenauer-Engler and Herre 2023).
Years since last coup	Number of years elapsed since the most recent coup in the sanctioned country. *Source:* Coup Leader Data (Eschenauer-Engler and Herre 2023).
lag Population Growth	Annual growth of de facto population, regardless of legal status or citizenship, lagged by one year. *Source:* World Development Indicators (World Bank 2025).
lag *log*(GDP pc)	Natural logarithm of real GDP per capita in 2015 USD, lagged by one year. *Source:* 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. *Source:* World Development Indicators (World Bank 2025).

Table B.3: Descriptive Statistics

	All Observations		**No Sanctions**		**Sanctions**	
	Mean	*N*	Mean	*N*	Mean	*N*
Polity2	2.00	6,070	2.19	4,716	1.34	1,354
Human Rights	0.27	6,070	0.43	4,716	–0.31	1,354
lag Population Growth	0.11	6,070	0.12	4,716	0.10	1,354
lag *log*(GDP pc)	8.19	6,070	8.25	4,716	7.99	1,354
lag Resources Rents Share	7.65	6,070	7.71	4,716	7.45	1,354
Years since last coup	8.97	6,070	8.14	4,716	11.85	1,354
	$X = 1$	*N*	$X = 1$	*N*	$X = 1$	*N*
Coups	197	6,070	162	4,716	35	1,354
Successful Coups	92	6,070	73	4,716	19	1,354
Previous Coups (10 years)	3,273	6,070	2,392	4,716	881	1,354
Intrastate Conflict	981	6,070	617	4,716	364	1,354
Interstate Conflict	71	6,070	45	4,716	26	1,354

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

Appendix B2: Additional Results

Table B.4: Effect Heterogeneity for Sanction Objectives

			Coups		
	Excl. Democracy	**Excl. Human Rights**	**Excl. Destab. Regime**	**Excl. Policy Change**	**Excl. Prevent War**
Lag Western	–0.0095	0.0127	0.0072	0.0137	0.0104
	(0.0099)	(0.0096)	(0.0080)	(0.0087)	(0.0081)
Lag Non-Western	–0.0013	0.0012	–0.0102	–0.0116	–0.0071
	(0.0166)	(0.0201)	(0.0167)	(0.0189)	(0.0161)
Observations	5,037	4,935	5,329	5,249	5,335
	Excl. End War	**Excl. Territ. Conflict**	**Excl. Terrorism**	**Excl. Other Objectives**	**Full Dataset**
Lag Western	0.0068	0.0096	0.0111	0.0093	0.0092
	(0.0081)	(0.0081)	(0.0083)	(0.0091)	(0.0081)
Lag Non-Western	–0.0086	–0.0090	–0.0082	–0.0070	–0.0097
	(0.0120)	(0.0171)	(0.0175)	(0.0180)	(0.0169)
Observations	5,305	5,382	5,325	5,137	5,419
			Successful Coups		
	Excl. Democracy	**Excl. Human Rights**	**Excl. Destab. Regime**	**Excl. Policy Change**	**Excl. Prevent War**
Lag Western	0.0087	0.0195***	0.0173***	0.0215***	0.0178***
	(0.0079)	(0.0069)	(0.0056)	(0.0059)	(0.0057)
Lag Non-Western	0.0143	0.0042	0.0189	–0.0034	0.0103
	(0.0118)	(0.0116)	(0.0116)	(0.0143)	(0.0114)
Observations	3,728	3,822	4,342	4,281	4,501
	Excl. End War	**Excl. Territ. Conflict**	**Excl. Terrorism**	**Excl. Other Objectives**	**Full Dataset**
Lag Western	0.0164***	0.0175***	0.0184***	0.0182***	0.0172***
	(0.0060)	(0.0055)	(0.0056)	(0.0060)	(0.0055)
Lag Non-Western	0.0103	0.0066	0.0069	0.0095	0.0063
	(0.0120)	(0.0113)	(0.0114)	(0.0119)	(0.0111)
Observations	4,365	4,542	4,496	4,319	4,570

Notes: Marginal effects of panel logit models according to Eqs. (1)–(4). LHS variable: (successful) coup in country i at year t. Standard errors are clustered by country. ***/**/* indicates significance at the 1%/5%/10% level. The differences in the number of observations are due to (i) the absorption of data spells by the fixed effects and (ii) the exclusion of sanctions with specific objectives. Models include all controls, region- and year-fixed effects.

Table B.5: Post-Cold War Effect Heterogeneity for Sanction Objectives

			Coups		
	Excl. Democracy	**Excl. Human Rights**	**Excl. Destab. Regime**	**Excl. Policy Change**	**Excl. Prevent War**
Lag Western	0.0120*	0.0228***	0.0166***	0.0235***	0.0187***
	(0.0063)	(0.0055)	(0.0060)	(0.0082)	(0.0057)
Lag Non-Western	0.0050	0.0072	0.0014	–0.0082	0.044
	(0.0122)	(0.0129)	(0.0123)	(0.0188)	(0.0139)
Observations	2,975	2,920	3,280	2,466	3,260
	Excl. End War	**Excl. Territ. Conflict**	**Excl. Terrorism**	**Excl. Other Objectives**	**Full Dataset**
Lag Western	0.0145***	0.0186***	0.0206***	0.0203***	0.0182***
	(0.0050)	(0.0059)	(0.0062)	(0.0068)	(0.0058)
Lag Non-Western	0.0106	0.0017	0.0031	0.0028	0.0015
	(0.0110)	(0.0125)	(0.0129)	(0.0136)	(0.0123)
Observations	3,213	3,284	3,235	3,051	3,316
			Successful Coups		
	Excl. Democracy	**Excl. Human Rights**	**Excl. Destab. Regime**	**Excl. Policy Change**	**Excl. Prevent War**
Lag Western	0.0186***	0.0241***	0.0208***	0.0325***	0.0240***
	(0.0067)	(0.0062)	(0.0062)	(0.0089)	(0.0062)
Lag Non-Western	0.0119	0.0193*	0.0100	–0.0012	0.0160
	(0.0130)	(0.0107)	(0.0125)	(0.0242)	(0.0127)
Observations	1,683	1,972	2,250	1,658	2,276
	Excl. End War	**Excl. Territ. Conflict**	**Excl. Terrorism**	**Excl. Other Objectives**	**Full Dataset**
Lag Western	0.0205***	0.0231***	0.0255***	0.0264***	0.0227***
	(0.0058)	(0.0060)	(0.0064)	(0.0068)	(0.0060)
Lag Non-Western	0.0182*	0.0107	0.0117	0.0126	0.0105
	(0.0097)	(0.0120)	(0.0121)	(0.0132)	(0.0118)
Observations	2,235	2,393	2,354	2,194	2,417

Notes: Marginal effects of panel logit models according to Eqs. (1)–(4). LHS variable: (successful) coup in country i at year t. Standard errors are clustered by country. ***/**/* indicates significance at the 1%/5%/10% level. The difference in the number of observations are due to (i) the absorption of data spells by FEs, and (ii) the exclusion of sanctions with specific objectives. Models include controls, region- and year-FEs.

Table B.6: Effect Heterogeneity for Coup Type

	Full Dataset	
	Military Coups	**Successful Military Coups**
Lag Western	0.0085	0.0168
	(0.0075)	(0.0052)
Lag Non-Western	–0.0174	–0.0006
	(0.0169)	(0.0118)
Observations	5,405	4,415
	Post-Cold War	
Lag Western	0.0197***	0.0251***
	(0.0069)	(0.0079)
Lag Non-Western	–0.0114	–0.0058
	(0.0159)	(0.0198)
Observations	2,534	1,708

Notes: Marginal effects of panel logit models according to Eqs. (1)–(4). LHS variable: (successful) coup in country i at year t. Standard errors are clustered by country. ***/**/* indicates significance at the 1%/5%/10% level. The difference in the number of observations are due to (i) the absorption of data spells by the FEs, and (ii) the Cold War sample split, (iii) the exclusion of non-military coups. Models include all controls, region- and year-fixed effects.

Table B.7: Baseline Results - Propensity-Score Weighting Estimation Results

	Coups	Successful Coups	Coups	Successful Coups
	(1)	(2)	(3)	(4)
Sanctions				
… lag Western	0.0171**	0.0179***	0.0165*	0.0214***
	(0.0077)	(0.0051)	(0.0089)	(0.0062)
… lag Non-Western	0.0122	0.0120	0.0234	0.0263*
	(0.0251)	(0.0150)	(0.0216)	(0.0146)
lag Polity2			0.0018	0.0012
			(0.0011)	(0.0008)
lag Human Rights			0.0030	0.0036
			(0.0060)	(0.0044)
lag Internal Conflict			0.0341**	0.0163
			(0.0154)	(0.0121)
lag Interstate Conflict			0.0204	0.0124
			(0.0276)	(0.0213)
Previous Coup (10 years)	0.0007	0.0083	-0.0086	0.0076
	(0.0113)	(0.0080)	(0.0125)	(0.0093)
Years since last coup	-0.0013***	-0.0005	-0.0017***	-0.0005
	(0.0005)	(0.0003)	(0.0006)	(0.0003)
lag Population Growth	0.1096**	0.0585*	0.0469	0.0220
	(0.0464)	(0.0345)	(0.0338)	(0.0213)
lag *log*(GDP pc)	-0.0130*	-0.0066	-0.0173**	-0.0114*
	(0.0073)	(0.0053)	(0.0078)	(0.0059)
lag Resources Rents Share	-0.0001	-0.0003	-0.0001	-0.0003
	(0.0005)	(0.0003)	(0.0005)	(0.0004)
Region FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	5,411	4,564	5,411	4,564

Notes: Marginal effects of panel logit models according to Eqs. (1)–(4), and Eqs. (6)–(8). The LHS variable indicates whether a coup occurred in country i at year t. Standard errors are clustered at the country level. ***/**/* indicates significance at the 1%/5%/10% level. The differences in the number of observations are due to the absorption of data spells by the fixed effects.

Table B.8: Baseline Results - Firth's Logit Model

	Coups	Successful Coups	Coups	Successful Coups
	(1)	(2)	(3)	(4)
Sanctions				
… lag Western	0.0172***	0.0178***	0.0093	0.0166***
	(0.0061)	(0.0042)	(0.0064)	(0.0045)
… lag Non-Western	0.0051	0.0098	–0.0075	0.0076
	(0.0107)	(0.0069)	(0.0111)	(0.0071)
lag Polity2			0.0004	0.0003
			(0.0005)	(0.0004)
lag Human Rights			–0.0082***	–0.0016
			(0.0031)	(0.0023)
lag Intrastate Conflict			0.0196***	0.0090*
			(0.0067)	(0.0051)
lag Interstate Conflict			0.0291*	0.0128
			(0.0156)	(0.0119)
Previous Coup (10 years)	0.0093*	0.0114***	0.0060	0.0110***
	(0.0050)	(0.0038)	(0.0050)	(0.0038)
Years since last coup	–0.0009***	–0.0003	–0.0011***	–0.0003
	(0.0003)	(0.0002)	(0.0003)	(0.0002)
lag Population Growth	0.0170	–0.0154	0.0297	–0.0117
	(0.0301)	(0.0196)	(0.0297)	(0.0197)
lag *log*(GDP pc)	–0.0139***	–0.0077***	–0.0108***	–0.0074***
	(0.0030)	(0.0024)	(0.0031)	(0.0024)
lag Resources Rents Share	–0.0002	–0.0002	–0.0004	–0.0003
	(0.0003)	(0.0002)	(0.0003)	(0.0002)
Region FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	6,070	6,070	6,070	6,070

Notes: Marginal effects of penalized panel logit models according to Firth (1993). The LHS variable indicates whether a coup occurred in country i at year t. Due to technical reasons, standard errors are not clustered at the country level. ***/**/* indicates significance at the 1%/5%/10% level. As penalization is used to adjust for separation issues, no observations are removed in the estimation process.

Table B.9: Baseline Results - Incidental Parameter Bias Correction

	Coups	Successful Coups	Coups	Successful Coups
	(1)	(2)	(3)	(4)
Sanctions				
… lag Western	0.0168***	0.0182***	0.0089	0.0168***
	(0.0065)	(0.0049)	(0.0068)	(0.0052)
… lag Non-Western	0.0032	0.0086	-0.0094	0.0062
	(0.0118)	(0.0085)	(0.0122)	(0.0088)
lag Polity2			0.0004	0.0003
			(0.0005)	(0.0004)
lag Human Rights			-0.0081**	-0.0017
			(0.0033)	(0.0027)
lag Intrastate Conflict			0.0192***	0.0088
			(0.0071)	(0.0059)
lag Interstate Conflict			0.0266	0.0098
			(0.0168)	(0.0148)
Previous Coup (10 years)	0.0092*	0.0118***	0.0059	0.0112**
	(0.0052)	(0.0043)	(0.0053)	(0.0044)
Years since last coup	-0.0009***	-0.0003	-0.0011***	-0.0004
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
lag Population Growth	0.0161	-0.0138	0.0283	-0.0105
	(0.0325)	(0.0246)	(0.0322)	(0.0243)
lag *log*(GDP pc)	-0.0139***	-0.0080***	-0.0108***	-0.0076***
	(0.0032)	(0.0028)	(0.0033)	(0.0029)
lag Resources Rents Share	-0.0003	-0.0002	-0.0004	-0.0003
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Region FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	5,471	4,621	5,471	4,621

Notes: Marginal effects of bias corrected panel logit models according to Fernández-Val (2009). The LHS variable indicates whether a coup occurred in country i at year t. Due to technical reasons, standard errors are not clustered at the country level. ***/**/* indicates significance at the 1%/5%/10% level. The differences in the number of observations are due to (i) the absorption of data spells by the fixed effects and (ii) the bias correction, which can reduce the need to drop groups due to separation as a result of slightly different convergence paths.

Table B.10: Baseline Results - Linear Probability Model

	Coups	Successful Coups	Coups	Successful Coups
	(1)	(2)	(3)	(4)
Sanctions				
… lag Western	0.0142*	0.0151**	0.0071	0.0138*
	(0.0078)	(0.0054)	(0.0083)	(0.0056)
… lag Non-Western	0.0049	0.0082	–0.0008	0.0071
	(0.0112)	(0.0079)	(0.0116)	(0.0081)
lag Polity2			–0.0006	–0.0001
			(0.0006)	(0.0004)
lag Human Rights			–0.0030	0.0002
			(0.0030)	(0.0020)
lag Intrastate Conflict			0.0322**	0.0097
			(0.0107)	(0.0069)
lag Interstate Conflict			0.0414	0.0104
			(0.0323)	(0.0175)
Previous Coup (10 years)	0.0160*	0.0163**	0.0121	0.0155*
	(0.0092)	(0.0059)	(0.0095)	(0.0061)
Years since last coup	–0.0006*	–0.0002	–0.0007**	–0.0002
	(0.0002)	(0.0001)	(0.0002)	(0.0001)
lag Population Growth	0.0218	–0.0092	0.0292	–0.0084
	(0.0236)	(0.0164)	(0.0229)	(0.0161)
lag *log*(GDP pc)	–0.0092***	–0.0032*	–0.0060*	–0.0027
	(0.0026)	(0.0016)	(0.0030)	(0.0020)
lag Resources Rents Share	–0.0003	–0.0002	–0.0004	–0.0002
	(0.0003)	(0.0001)	(0.0003)	(0.0001)
Region FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	6,070	6,070	6,070	6,070

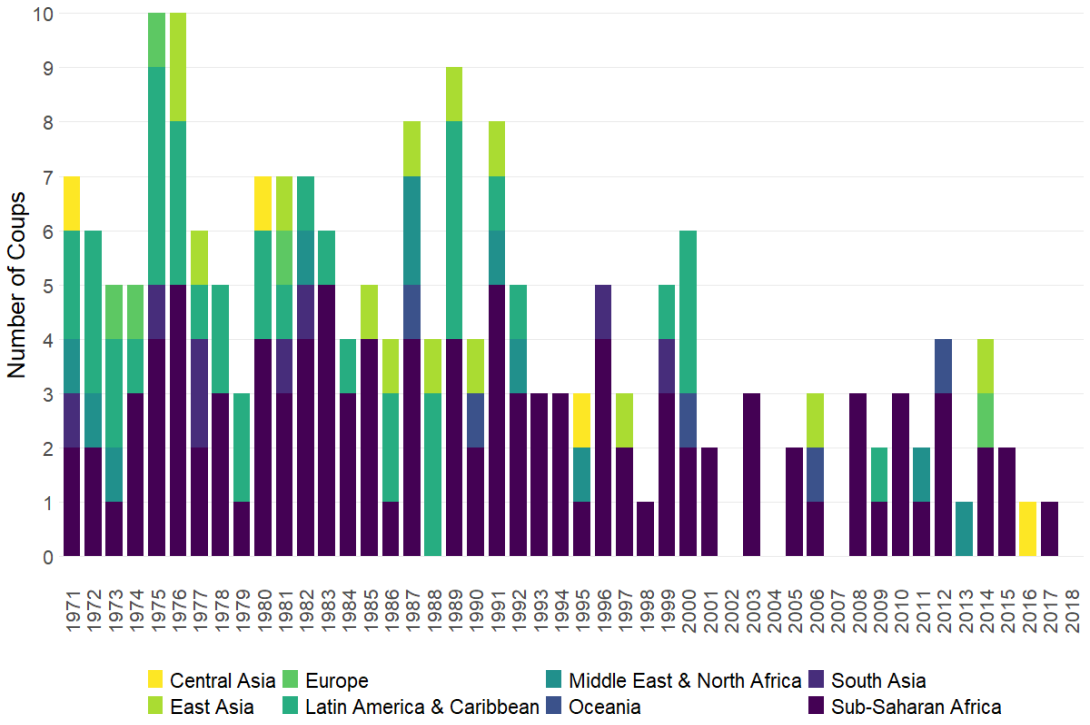
Notes: Coefficients of linear probability models. The LHS variable indicates whether a coup occurred in country i at year t. Standard errors are clustered by country. ***/**/* indicates significance at the 1%/5%/10% level.

Table B.11: Baseline Results - Coup Data from Bjørnskov and Rode (2020)

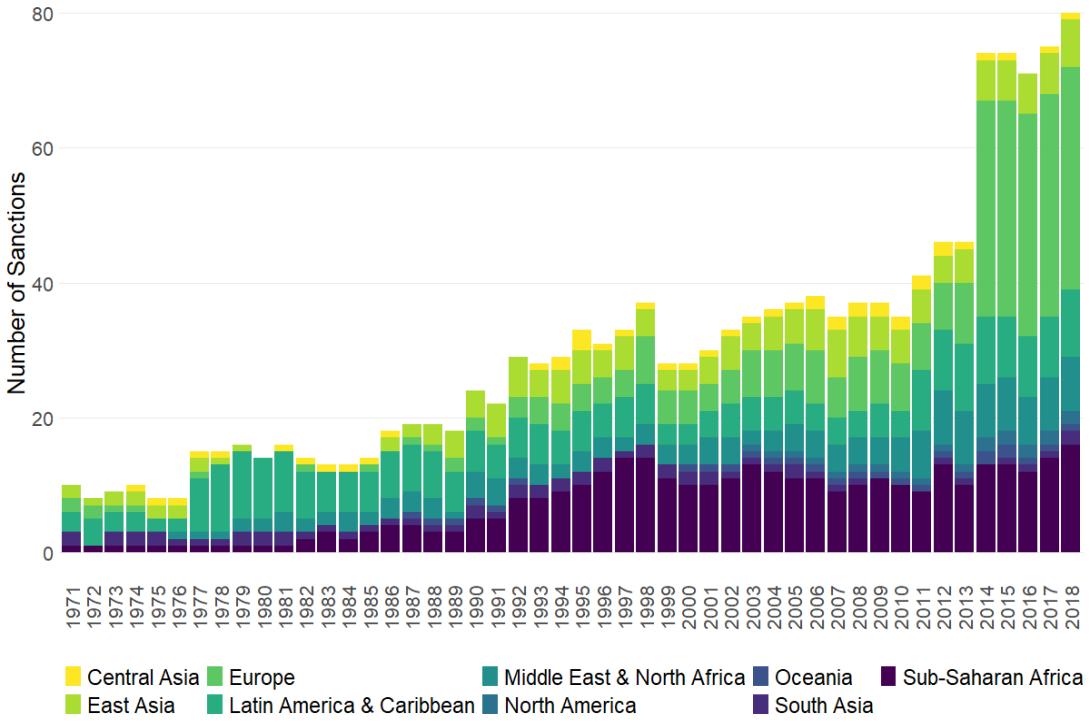
	Coups	Successful Coups	Coups	Successful Coups
	(1)	(2)	(3)	(4)
Sanctions				
… lag Western	0.0246**	0.0307***	0.0048	0.0224***
	(0.0106)	(0.0078)	(0.0110)	(0.0079)
… lag Non-Western	–0.0110	–0.0084	–0.0375*	–0.0168
	(0.0185)	(0.0142)	(0.0211)	(0.0143)
lag Polity2			–0.0009	–0.0012**
			(0.0007)	(0.0006)
lag Human Rights			–0.0133***	–0.0042
			(0.0050)	(0.0037)
lag Intrastate Conflict			0.0254**	0.0118
			(0.0117)	(0.0104)
lag Interstate Conflict			–0.0172	–0.0316
			(0.0199)	(0.0229)
Previous Coup (10 years)	–0.0783***	–0.0437***	–0.0825***	–0.0458***
	(0.0129)	(0.0088)	(0.0132)	(0.0089)
Years since last coup	–0.0095***	–0.0059***	–0.0092***	–0.0058***
	(0.0013)	(0.0010)	(0.0013)	(0.0009)
lag Population Growth	–0.0156	–0.0682**	0.0033	–0.0607**
	(0.0525)	(0.0275)	(0.0485)	(0.0278)
lag *log*(GDP pc)	–0.0259***	–0.0166***	–0.0182**	–0.0130**
	(0.0084)	(0.0053)	(0.0076)	(0.0051)
lag Resources Rents Share	–0.0004	–0.0004	–0.0003	–0.0005
	(0.0005)	(0.0004)	(0.0004)	(0.0004)
Region FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	6,004	4,571	6,004	4,571

Notes: Marginal effects of panel logit models according to Eqs. (1)–(4). The LHS variable indicates whether a coup occurred in country i at year t. Standard errors are clustered at the country level. ***/**/* indicates significance at the 1%/5%/10% level. The differences in the number of observations are due to (i) the absorption of data spells by the fixed effects and (ii) small differences between the two coup datasets.

Figure B.1: Comparative visualizations of coups and sanctions across time and regions.



Annual number of coups by region.



Annual number of sanctions by target region.

Figure B.2: Annual number of total and Western sanctions.

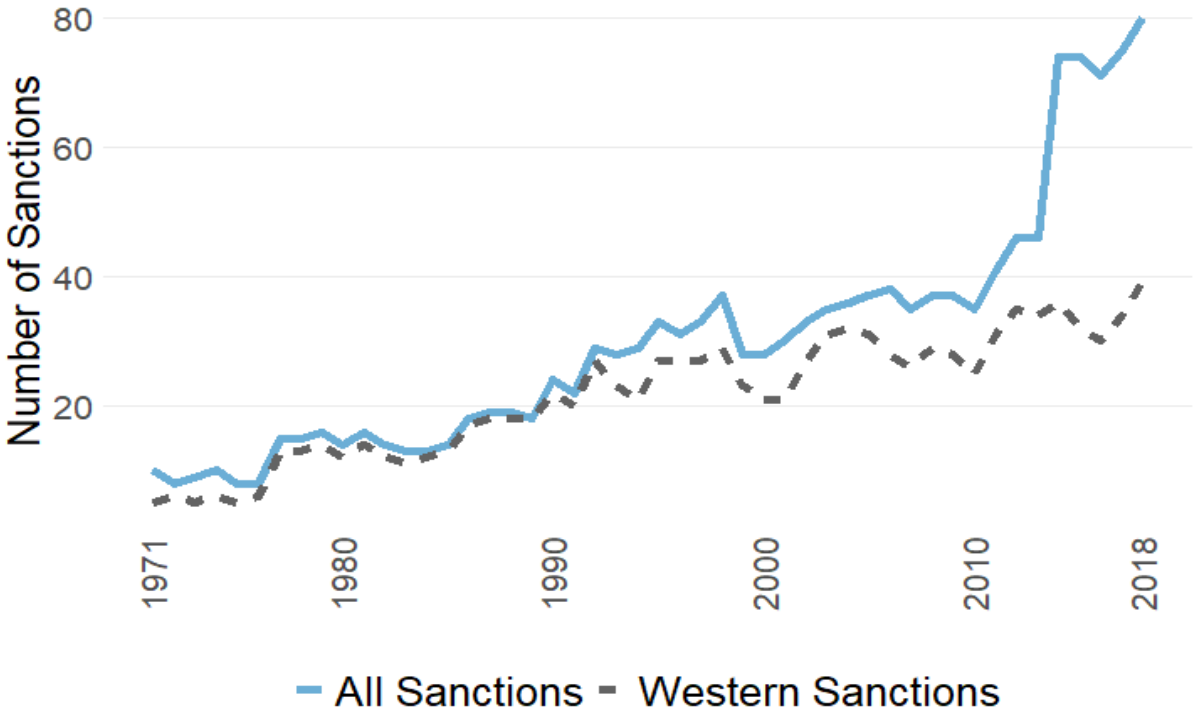
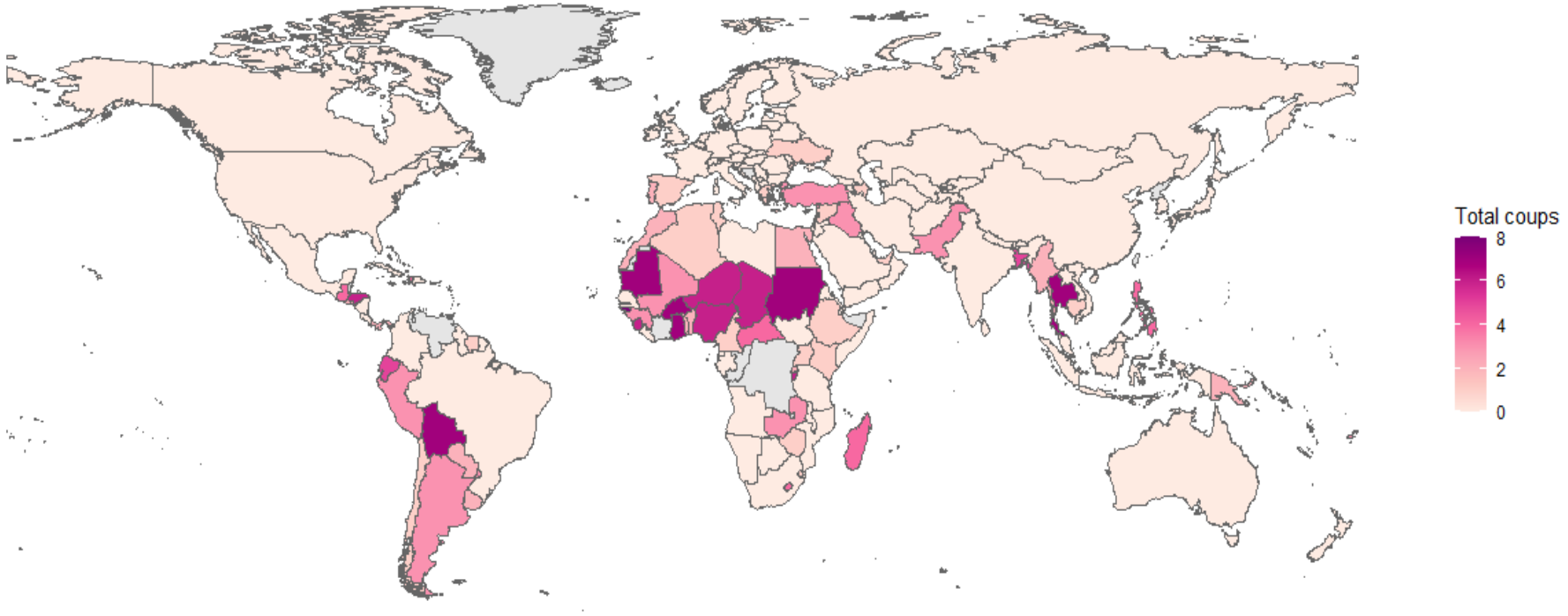


Figure B.3: Global Map of Coups.



4 Leader Similarity and International Sanctions

"Leader Similarity and International Sanctions"

with Prof. Dr. Matthias Neuenkirch and Dr. Jerg Gutmann

Published in the *Journal of Conflict Resolution*[1]

Abstract

It is well-established that political leaders matter for domestic outcomes, but statistical evidence for their relevance in international politics is comparatively scarce. We ask whether the personal relationship between political leaders can change the propensity for nonviolent conflict between nation-states in the form of sanctions. Panel probit models with data from 1970 to 2004 are estimated to evaluate whether more similar leaders are less likely to sanction each other. Our results indicate that higher leader similarity reduces the likelihood of sanction imposition. The effect is most pronounced for sanctions imposed through unilateral political decisions. The probability of such sanctions ranges from 2.3% at the highest observed leader similarity to 7.2% at the lowest. Leader similarity especially matters for sanctions aimed at democratic change or human rights, for non-trade sanctions, and when at least one autocracy is involved. Finally, leader similarity has become more important after the Cold War.

1. We would like to thank Sage Publishing for publishing the article in this dissertation.

4.1 Introduction

I believe him, and I'm convinced that he is [a flawless democrat].
(German chancellor Gerhard Schroeder on Vladimir Putin, 2004).

I knew Putin very well. I got along with him great. He liked me. I liked him.
(Former US President Donald Trump, 2022).

In recent years, economists and political scientists have abandoned the idea that political leaders can be characterized solely by a policy platform, which has long dominated political economy research. It has, for example, been shown that political leaders' ideology, education, and professional background matter for policy decisions (Dreher and Yu 2020; Dreher et al. 2009; Funke et al. 2023; Gutmann, Metelska-Szaniawska, et al. 2024a; Hayo and Neumeier 2016; Mehmood and Seror 2023; Neumeier 2018; Peveri 2022). It has also been demonstrated that whether politicians are, based on their looks, perceived as attractive or trustworthy determines their electoral success, their political work, and the performance of their national economy (e.g., Berggren et al. 2010; François et al. 2023; Gründler et al. 2024). Most of these studies are concerned with domestic politics, while less attention has been paid to whether such traits of politicians matter for international cooperation and conflict.[2] Still, some recent studies have emphasized that leader characteristics and leaders' interactions with each other can shape foreign policy behavior and international outcomes (Byman and Pollack 2001; Saunders 2011; Dube and Harish 2020; Horowitz and Stam 2014; Ellis et al. 2015; Hermann 1980; Holmes and Yarhi-Milo 2017; Holmes 2018; Yarhi-Milo 2018; Holmes and Wheeler 2020; Wheeler 2018).

In international relations, it is natural to think of political leaders not just as isolated decision-makers, but as being continuously involved in interactions with other leaders. This leads to the question of which of these leader pairs cooperate more successfully or are more likely engaged in conflict with each other. At the country level,

2. Our focus here is on quantitative tests of psychological or sociological models of political leader behavior. Of course, the political psychology of international relations has formulated theories and conducted qualitative research on the role of leaders at least since the 1960s (see Holsti 1976, for criticisms of and challenges affecting this early strand of research).

it has been demonstrated that the intensity of bilateral economic exchange is determined by countries' shared history and cultural similarities (Guiso et al. 2009). Only few empirical studies have started to generate comparable insights for political leaders. Foster and Keller (2023), for example, provide some evidence suggesting that pairs of cognitively simple leaders experience more interstate conflicts than pairs of cognitively complex leaders or asymmetric pairs that consist of both types of leaders. At the same time, cases where personal relationships between leaders have affected those between their nations exist in abundance (e.g., Zurcher and Murphy 2025). When Mark Carney replaced Justin Trudeau as Canada's prime minister, relations with US president Trump and his government improved instantly. Carney is not only older than Trudeau (i.e., closer to Trump's age), he also had a successful career in international banking and running financial institutions, allowing him to accumulate significant personal wealth.

Here, we seek to determine whether international sanctions are imposed less often between political leaders who share greater similarity (measured as described in Section 4.3.1). The literature on international sanctions has, thus far, ignored individual leader characteristics as a factor in sanction imposition and effectiveness, despite the attention they receive in the broader international political economy literature. Levy (2023) also considers the incorporation of psychological factors into influential bargaining models a central task for future international relations research. Most similar to our study is DiLorenzo and Rooney (2025). They find that pairs of countries whose leaders share a similar background are less likely engaged in an interstate conflict with each other. Our focus on sanctions instead of conflict implies some notable differences between our study and DiLorenzo and Rooney (2025). First, international sanctions occur much more frequently than military interstate disputes. The number of imposed sanctions has increased dramatically since the end of the Cold War (Felbermayr et al. 2020). If leader similarity has a significant effect on international sanctions, it is of great importance to the day-to-day business of international politics, and not just to the most severe escalations of interstate conflict. Moreover, the larger number

of international sanctions compared to military conflicts facilitates the study of the heterogeneity of causes of different types of sanctions. Second, sanction imposition is typically a more legalistic process than the decision to start an interstate conflict (see, e.g., Eaton and Sykes 2002).[3] It is, therefore, more difficult for leader traits to influence the decision to impose sanctions than that to start a war.

In our empirical analysis, we estimate panel probit models with sender-year and dyad fixed effects. Our dataset combines data from the Global Sanctions Data Base (GSDB, see Felbermayr et al. 2020) with data on leader similarity by DiLorenzo and Rooney (2025). It covers 293,594 observations corresponding to 156 potential sender countries, 155 potential target countries, and 13,242 directed country-pairs (or dyads) over the period from 1970 to 2004. As many sanctions are imposed by international organizations consisting of a group of countries, an individual political leader has only limited influence on these decisions. Thus, we estimate most models without these multilateral sanctions.

We find that higher leader similarity significantly reduces the likelihood of sanction imposition. This result holds across various model specifications and estimation techniques. The effect is especially pronounced when United Nations (UN) and European Union (EU) sanctions are excluded, that is, when sanctions are imposed through unilateral political decisions. The predicted probability of such sanctions being imposed ranges from 2.3% at the highest observed leader similarity in the sample to 7.2% at the lowest. Moreover, the influence of leader similarity is strongest in sanctions aimed at political or human rights improvements—compared to those related to conflict, terrorism, regime destabilization, or policy change. Political leaders can be expected to enjoy most discretion regarding the imposition of these types of sanctions. Finally,

3. In the United States, as an example of a democracy with substantial executive authority, sanctions operate within explicit legal frameworks such as the International Emergency Powers Act and the Trading with the Enemy Act. To impose sanctions, the president must declare a state of emergency based on defined criteria and must report regularly to Congress. The sanctions further have to be continuously renewed. Congress can terminate the underlying emergency by joint resolution, which the president cannot veto. Further government agencies are involved in the implementation of the sanctions. A US military attack, on the other hand, requires far fewer documented procedural steps, although prolonged conflict requires affirmative congressional votes. In other democracies, such as Canada, the gap between the procedural requirements of sanctions and warfare can be even wider.

leader similarity has become more important after the Cold War; and the imposition of non-trade sanctions (e.g., arms and military sanctions) and of sanctions from or against autocracies depends most on the similarity of the involved political leaders.

The remainder of this article is structured as follows. Section 4.2 briefly outlines the theoretical rationale for why sanctions are more likely imposed between leaders with a less similar background. Section 4.3 describes our estimation strategy and the data used in our empirical analysis. We discuss our empirical results, including extensions and robustness tests, in Section 4.4. Section 4.5 concludes.

4.2 Theory

Sanctions are an instrument of foreign influence that is supposed to induce a policy change by the target country's government. In a globally interconnected world with abundant political and economic international externalities, sanctions offer an alternative policy instrument for the sender country's government where voluntary (self-enforcing) international agreements fail to internalize the most significant externalities (Aidt et al. 2021, 2025; Gutmann, Neuenkirch, and Neumeier 2025). How much compliance can be extracted via sanctions depends on the cost of sanctions to the sender and the target, as well as both parties' patience (Eaton and Engers 1992).[4]

In practice, sanctions are costly to both senders and targets (Gutmann, Neuenkirch, and Neumeier 2023a, 2023b) and a first-best outcome, thus, cannot be achieved when they are employed. In a world of complete information, that is, zero transaction costs, sanctions would never need to be used, as credible sanction threats would be anticipated and the target would adjust its behavior in advance (Coase 1960; Eaton and Engers 1999). In the remaining cases, sanctions would also not be deployed in equilibrium, although the target makes no concession, and the sender government would, therefore, not even bother to formulate a threat in the first place. Therefore, implemented international sanctions are a second-best instrument for a world of incomplete information. Transaction costs include the cost of the bargaining process and

4. Li (2025) demonstrates that leaders with a reputation for madness are less likely to back down when targeted with sanctions.

determine, for example, whether information on the sender's and the target's type is available to both parties. If the costliness of sanctions to the other party or the other party's patience is private information, bargaining can fail and sanctions may be used in equilibrium (Hovi et al. 2005; Spaniel and Smith 2015).[5] Sanctions may also be used because of asymmetric information between the sender government and an audience, such as voters or the international community, if the sender government wants to be seen as doing something, for example, about a violation of international law, even though it is not able to change the target government's policy choice (Hovi et al. 2005). Diplomacy is generally supposed to reduce information asymmetries between governments. However, because significant transaction costs are involved and diplomats are often incentivized not to reveal truthful information (e.g., regarding their government's resolve), information asymmetries persist to a large extent and political leaders often become involved in communication and negotiations that determine whether political disagreements escalate to the level of sanctions.

Based on this theory of sanction imposition, the level of similarity of two political leaders may affect the likelihood that one is imposing sanctions against the other via different channels. While it is not our goal to test them against each other, they provide alternative and complementary explanations for how leader similarity affects sanction policy. First, leader similarity reduces the cost of contracting and, thereby, widens the scope for voluntary agreements between the two nations (see also Gerber et al. 2013; Giannetti and Yafeh 2012). Many international externalities are accordingly internalized without the need for sanction threats. Second, by improving communication and promoting trust,[6] leader similarity can reduce the likelihood that sanctions are imposed when there is uncertainty about the other leader's policies or their intentions. Arms sanctions could, for example, be avoided if the sender trusted the target government to not use acquired arms to wage war or commit atrocities (Efrat and Yair 2024).

5. The government and opposition parties may also differ in their commitment to sanctions, which further complicates the evaluation of their credibility as a threat (Onder 2019).

6. Trust is defined, here, following Gambetta (1988) as a sufficiently high probability on a subjective probability scale with which leader A judges that leader B will perform a particular action that affects A's own choice. Trust corresponds to a probability high enough that A is willing to cooperate with B and, for example, not impose sanctions.

Absent such trust, sanctions may be used to further incentivize or coerce the target government to comply with the expected behavior. Since trust is a subjective leader characteristic, the level of trust between two political leaders may determine in an otherwise identical situation if sanctions are imposed. Third, if leader similarity allows one leader to better judge the type of the other (see, e.g., Holmes 2013, 2018), that is, how patient they are or how costly sanctions would be to them, fewer or no sanctions would be imposed in equilibrium. This is because potential targets would know when to acquiesce, and potential senders would know that imposing sanctions in the remaining cases would not yield results.[7] Finally, if more similar leaders have more trust in each other, using sanctions could potentially erode that trust and thereby reduce their ability to conclude unrelated mutually beneficial agreements in the future. In other words, sanctions would be less likely to be imposed between more similar leaders, because their opportunity costs are higher.

To understand why similarity is expected to enhance trust, communication, and mutual understanding, as we have assumed thus far, we have to integrate non-rational-choice based theories of social interaction into our framework of analysis (see Akerlof and Kranton 2000, on integrating identity into rational choice models).[8] Social interactions are frequently characterized by homophily and in-group bias (Currarini and Mengel 2016), two of the most widely observed social phenomena. Homophily describes the tendency to associate with similar others and leads to smoother coordination and communication as well as enhanced trust (Békés and Ottaviano 2025; Currarini et al. 2009; Ertug et al. 2020). In-group bias describes the tendency to trust and give favorable treatment to members of one's perceived in-group (Tajfel and Turner 1986). Both similarity to others and the delineation of one's in-group can be based on a variety of personal traits, including people's age, gender, ethnicity, religion, social

7. Sanctions might still be used to deter transgressions of third-party leaders who are less similar to the potential sender, but this incentive is independent of the similarity of the sender and target at hand.

8. Although similarity effects may be more pronounced in the automatic than in the reflective system of thinking, our arguments do not presuppose particular types of decision situations, as heuristics can be expected to be employed either way (see, e.g., Vis 2019). Moreover, to the extent that similarity reduces transaction costs, heuristics are not needed for leader similarity to have an effect and the applied system of thinking should be even less relevant in that case.

class, nationality, education, and occupation.[9] Both homophily and in-group bias can be motivated either by taste or by expectations about the behavior of others. Cetre et al. (2024), for example, find that 80% of the ethnically motivated in-group bias they observe in the United States and Germany is taste-based and only 20% is attributable to anticipated differences in trustworthiness. Psychological research has shown that perceived similarity plays an important role in the evolution of cooperation in a prisoner's dilemma (Fischer 2009), and that perceived similarity in a trust game is conducive to trust (Clerke and Heerey 2021). Similarity can be expected to matter for trust when trust is largely localized, rather than generalized (see Tabellini 2008). Or, in the terminology of Enke (2019, 2020), similarity is more conducive to trust, the more individuals have communal rather than universalistic values. Interestingly, based on the work of Jervis (2017), one might interpret the increased willingness to cooperate with more similar leaders as the consequence of predictable misperceptions that cause leaders to overestimate the goodwill and underestimate the threat of similar leaders.

These general insights about how similarity shapes trust and, thereby, successful cooperation have rarely been applied to the interactions of leaders in international politics. Their decisions are often assumed to be guided by strict rationality (Levy 2023), also because of the support by professional administrative bodies and the existence of institutional guardrails, which are meant to mitigate the effects of cognitive biases and human error.[10] This assumption, however, has been challenged. DiLorenzo and Rooney (2025), for example, argue that leaders' shared life experiences facilitate social bonding and the emergence of trust, which in turn reduces information and commitment problems that could lead to military disputes. Foster and Keller (2023)

9. Falk and Zehnder (2013) show in a field experiment that in-group bias even exists among inhabitants of the same city district. Even-Tov et al. (2023) find favoritism simply due to a shared first name.

10. However, there is research that explicitly addresses non-rationality in international relations regarding direct interactions between leaders (Holmes 2018; Holmes and Wheeler 2020; Holmes and Yarhi-Milo 2017). It specifically deals with the biological and cultural mechanisms that are used in face-to-face interactions to infer a counterpart's intentions. In other words, whereas we are interested in predicting which leaders are generally more likely to successfully cooperate, this strand of international relations literature can explain in detail how cooperation is achieved while two leaders interact in person. There are also important overlaps in the argumentation. Holmes (2013, 2018), for example, argues that the other's intentions can still be inferred if interacting leaders are very dissimilar, but to a lesser degree.

argue that conflict is more likely when cognitively simple leaders interact with each other.

The main implication of our theoretical arguments can be summarized in the following hypothesis:

Hypothesis 1. *Pairs of more similar leaders are less likely to impose sanctions against each other than leader pairs with fewer similarities.*

4.3 Data and Estimation Strategy

4.3.1 Data

Our dataset contains 293,594 observations covering 156 potential sender countries, 155 potential target countries, and 13,242 directed country-pairs for the period from 1970 to 2004. A list of countries can be found in Table C.1. Our binary dependent variable indicates whether a potential target country is sanctioned by a potential sender country in a given year. Many sanctions are imposed by international organizations, wherein decisions are made by a group of countries. The effect of an individual country and its leader on the imposition of UN sanctions is—except for the five veto powers—very limited. The same applies to a lesser extent to sanctions imposed by the EU. Accordingly, we estimate all models (i) excluding observations with UN and EU sanctions, (ii) excluding only observations with UN sanctions, and (iii) using the full sample. Data on sanctions is taken from release 3 of the Global Sanctions Data Base (GSDB, see Felbermayr et al. 2020; Kirilakha et al. 2021; Syropoulos et al. 2024). In the full dataset, 30,793 out of 293,594 dyad-years (10.5%) are subject to sanctions. In the most restrictive estimation sample, this number drops to 785 out of 7,160 dyad-years (11.0%), as observations with UN and EU sanctions are excluded, and many observations are absorbed by dyad fixed effects due to a lack of within-dyad variation in the dependent variable.

Our main independent variable is a leader similarity index introduced by DiLorenzo and Rooney (2025). They calculate Pearson's correlation coefficient over

58 leader characteristics, separately for every leader pair.[11] The data on leader characteristics comes from the well-established but discontinued LEAD dataset by Ellis et al. (2015). It covers personal background information and life experiences in seven categories (military experience, education, family variables in childhood, family variables in adulthood, occupation, political experience, and others).[12] Observed values of the index range from –0.09 to 1.00. For example, the similarity score of Gerhard Schroeder and Vladimir Putin is 0.95. As part of our robustness tests, we re-calculate the leader similarity index by leaving out one of the seven categories at a time and, alternatively, by including only theoretically well-justified characteristics (see Section 4.4.2). Similarity scores are calculated for leader pairs, not for dyad-years. To deal with leadership changes within a given year while aggregating scores to the dyad-year level, we construct the leader similarity score in years of leadership transition as the average similarity of the different leader pairs who are in office in that year, weighted by the number of days each leader pair is in office. The use of this leader characteristic data, is based on the assumption that the personal background and life experiences of political leaders are not only themselves observable, but they also serve as proxies for leaders' "operational code" (Holsti 1970; Schafer and Walker 2023), that is, the way in which leaders perceive and interpret their physical and social environment.

To account for the various causes of sanction imposition in the target country, we control for a number of political and (socio-)economic factors, lagged by one year to mitigate potential reverse causality. The economic situation is captured by the coun-

11. As part of our robustness tests, we also calculate a different leader similarity metric based on the reversed Gower's distance (Gower 1971). This measure can directly handle different types of data (nominal, ordinal, and continuous) and might be more appropriate than the Pearson correlation coefficient, which is designed for continuous data.

12. The individual variables are **Military experience:** Military service (combat/noncombat), military education, rebel participation, national war participation (victory/loss), and rebel war participation (victory/loss). **Education:** Primary education, boarding school, and education level. **Family variables, childhood:** Only/firstborn/middle-born/"illegitimate" child, first son/daughter, parental status, orphan, royalty, father occupation, mother in labor force, and childhood health. **Family variables, adulthood:** Married (ever/in power), divorced (ever), number of spouses in life, number of sons/daughters, number of children/adopted children, and children died. **Occupation:** Education, journalism, law, engineering, medicine, science, agriculture, military career, religion, labor, activist, career politician, writer, film/music, aristocrat/landowner, police, and interpreter. **Political experience:** Years in politics before becoming leader and puppet leader. **Others:** Physical/mental health and gender. For further details, we refer to Ellis et al. (2015).

try's real GDP per capita (in logs). In addition, our models include the population size (also in logs) and a globalization index (KOFGI, see Dreher 2006; Gygli et al. 2019). The target country's political conditions—and thus the likely causes of sanctions—are described by a democracy dummy (indicating a *polity2* score by Marshall and Gurr 2020 above five), a latent human rights score by Fariss (2019), and dummy variables for a successful coup (Bjørnskov and Rode 2020) as well as minor and major conflicts (Gleditsch et al. 2002).

Table C.2 provides a list of variables alongside their definitions and the underlying data sources. Table C.4 shows descriptive statistics for the full dataset (Panel A) and the dataset on which most of the estimations are based (Panel B), that is, without dyad-years subject to UN or EU sanctions and without data spells that are absorbed by the two-way fixed effects. In both panels, we also distinguish between non-sanctioned and sanctioned observations. Leader similarity is lower in dyad-years subject to sanctions. Moreover, sanctioned countries tend to be less globalized and have a lower GDP per capita. They show more serious infringements of human rights, are less often democratic, and more often experience a successful coup. In the full dataset, countries subject to sanctions experience minor and major conflicts at a much higher frequency. However, this no longer holds once UN sanctions are removed from the dataset—presumably because (major) conflicts are one of the primary reasons for the UN Security Council to reach agreement on imposing sanctions.

4.3.2 Estimation Strategy

To evaluate the effect of leader similarity on sanction imposition, we estimate panel probit models. These allow modeling binary outcomes while accounting for unobserved heterogeneity and non-linear time trends. The general model specification can be described as follows:

$$y^*_{i,j,t} = \beta S_{i,j,t} + \gamma X^{pol}_{j,t-1} + \delta X^{econ}_{j,t-1} + \tau_{i,t} + \alpha_{i,j} + \epsilon_{i,j,t} \tag{1}$$

$y^*_{i,j,t}$ is the latent (unobserved) variable that corresponds to the observed binary outcome variable $y_{i,j,t}$. In this notation, country i (the sender) imposes sanctions on country j (the target) in year t ($y_{i,j,t} = 1$) or not ($y_{i,j,t} = 0$). $S_{i,j,t}$ represents the (weighted)[13] similarity of the leader in sender country i and the leader in target country j. $\tau_{i,t}$ represents sender-year fixed effects that capture all (time-varying) political, economic, and social conditions in the sender country. Importantly, these fixed effects account for a sender government's general (and potentially time-varying) inclination to employ international sanctions in disputes with other countries. Including these fixed effects is vital, as it separates leader similarity from the characteristics of the political leader imposing the sanctions, which are held constant. Moreover, these fixed effects nest the less granular year fixed effects, which capture any global (non-linear) time trend in the use of sanctions, such as their increased use since the end of the Cold War. Dyad fixed effects $\alpha_{i,j}$ absorb factors such as shared languages and history as well as cultural, genetic, geographic, and political proximity, to the extent that they are time-invariant (Guiso et al. 2009, e.g., show that bilateral exchange depends on shared history and culture). In addition, dyad fixed effects nest the less granular sender fixed effects and target fixed effects. Thus, they account for all time-invariant sender and target country characteristics. Since dyad fixed effects reduce the scope for potential bias in the estimation at the cost of significantly reduced efficiency (because then only variation within dyads over time is analyzed), we present estimation results with and without dyad fixed effects. We do not include target-year fixed effects, as target country leaders do not choose to be targeted by sanctions.[14] We account explicitly for a number of potentially confounding time-varying target country characteristics. $X^{pol}_{j,t-1}$ and $X^{econ}_{j,t-1}$ represent one-year lagged political and economic control variables. These

13. The weighting procedure is explained in Section 4.3.1.

14. Target-year fixed effects would fully account for the number of countries by which a target is sanctioned in any given year, and leader similarity would then only serve to explain the composition of those sender countries. Moreover, all observations of targets of UN sanctions or countries not sanctioned in a given year would be dropped. This would substantially reduce our sample size to 852 observations and discard most of the variation in the sanction imposition variable, ultimately causing the probit models to fail to converge in light of 598 fixed effects to be estimated (305 sender-year, 143 dyad-year, and 150 target-year fixed effects). For comparison, DiLorenzo and Rooney (2025) control for dyad fixed effects and year fixed effects.

are described in more detail in Section 4.3.1. $\epsilon_{i,j,t}$ is an idiosyncratic error term for which we assume clustering on the dyad level.

The observed binary outcome $y_{i,j,t}$ is linked to the latent variable $y^*_{i,j,t}$ as follows:

$$y_{i,j,t} = \begin{cases} 1 & \text{if } y^*_{i,j,t} > 0 \\ 0 & \text{if } y^*_{i,j,t} \leq 0 \end{cases} \tag{2}$$

Hence, the probability of observing $y_{i,j,t} = 1$ is given by the cumulative distribution function of the standard normal distribution, which is represented by $\Phi(\cdot)$:

$$P\left(y_{i,j,t} = 1 \mid S_{i,j,t}, X^{pol}_{j,t-1}, X^{econ}_{j,t-1}, \tau_{i,t}, \alpha_{i,j}\right) = \Phi\left(\beta S_{i,j,t} + \gamma X^{pol}_{j,t-1} + \delta X^{econ}_{j,t-1} + \tau_{i,t} + \alpha_{i,j}\right) \tag{3}$$

The parameters of the probit model are obtained using maximum likelihood estimation.

As these estimates describe the effects on the latent variable, we have to calculate marginal effects to quantify the effect of a change in an independent variable on the probability of the observable binary outcome occurring. The marginal effects are obtained by taking the partial derivative of the probability function in Eq. (3) with respect to the variable of interest. For example, the marginal effect for leader similarity ($S_{i,j,t}$) is calculated as follows:

$$\frac{\partial P\left(y_{i,j,t} = 1 \mid S_{i,j,t}, X^{pol}_{j,t-1}, X^{econ}_{j,t-1}, \tau_{i,t}, \alpha_{i,j}\right)}{\partial S_{i,j,t}} = \phi\left(\beta S_{i,j,t} + \gamma X^{pol}_{j,t-1} + \delta X^{econ}_{j,t-1} + \tau_{i,t} + \alpha_{i,j}\right) \cdot \beta \tag{4}$$

Since marginal effects in probit models vary across observations, due to the non-linearity of the link function, we report average marginal effects (AME), that is, we calculate the marginal effects for each observation and then take the mean value of these marginal effects. We use the delta method to estimate the standard errors corre-

sponding to the AME:

$$\begin{aligned} \text{SE(AME)} &= \sqrt{\text{Var}(\beta)} \cdot \frac{1}{N} \sum_{i=1}^{N} \phi\left(\beta S_{i,j,t} + \gamma X_{j,t-1}^{pol} + \delta X_{j,t-1}^{econ} + \tau_{i,t} + \alpha_{i,j}\right) \\ &= \sqrt{\text{Var}(\beta)} \cdot \text{AME} \end{aligned} \tag{5}$$

Hahn and Newey (2004) demonstrate in Monte Carlo studies that the bias from the incidental parameters problem in panel probit models is small. Nevertheless, we present estimates in Section 4.4.2 that are based on the bias correction method by Fernández-Val (2009) to address remaining concerns about incidental parameter bias. Finally, we also test the robustness of our results to OLS estimation (Angrist 2001).

4.4 Empirical Results

4.4.1 Baseline Results

Table 4.1 shows the AMEs from our baseline panel probit estimations. In a first step, we include only control variables that are undoubtedly exogenous to economic sanctions, that is, the lagged (socio-)economic conditions in the target country. Columns (1)–(6) include sender-year fixed effects and Columns (2), (4), and (6) additionally account for dyad fixed effects. Columns (1) and (2) exclude all observations with UN or EU sanctions in place, Columns (3) and (4) exclude observations with UN sanctions in place, and Columns (5) and (6) are based on the full sample. The discussion of the baseline results focuses on the more conservative estimates in Columns (2), (4), and (6).[15] The substantial differences in the number of observations underlying the different regression models result from two factors: (i) the exclusion of cases with UN or both UN and EU sanctions, and (ii) the absorption of data spells by the fixed effects.

15. These model specifications are indeed very conservative. All dyads in which we observe no (or only) sanctions are dropped from our sample, although such a pattern could, according to our theory, be the consequence of permanently high (low) leader similarity between two countries. This concerns about 82%–93% of the original sample.

Table 4.1: Explaining Sanctions with Leader Similarity and Socio-Economic Variables (Narrow)

	(1) w/o UN/EU	(2) w/o UN/EU	(3) w/o UN	(4) w/o UN	(5) All Obs.	(6) All Obs.
Leader Similarity	–0.0203***	–0.1792***	–0.0338***	–0.1212***	–0.0241***	–0.0588***
	(0.0023)	(0.0450)	(0.0024)	(0.0337)	(0.0021)	(0.0135)
lag log(GDP pc)^T	0.0063***	–0.1041**	0.0038***	–0.1456***	0.0200***	–0.2559***
	(0.0011)	(0.0499)	(0.0011)	(0.0307)	(0.0008)	(0.0124)
lag log(Population)^T	0.0083***	0.8252***	0.0092***	1.1100***	0.0133***	0.0424*
	(0.0004)	(0.1784)	(0.0004)	(0.1124)	(0.0004)	(0.0220)
lag Globalization^T	–0.0015***	–0.0099***	–0.0016***	–0.0072***	–0.0055***	–0.0222***
	(0.0001)	(0.0027)	(0.0001)	(0.0022)	(0.0001)	(0.0011)
Sender-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Dyad FE		Yes		Yes		Yes
Observations	165,364	7,160	167,012	11,924	293,594	51,097
Unique Dyads	11,801	431	11,990	563	13,242	1,937
Std. Dev. of Similarity	0.3504	0.3610	0.3508	0.3698	0.3505	0.3628

Notes: Marginal effects of panel probit models. The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level. Differences in the number of observations are due to (i) the exclusion of UN and EU sanctions or UN sanctions and (ii) the absorption of data spells by the fixed effects.

Leader similarity consistently has a negative effect on the likelihood of sanction imposition that is significant at the 1% level across all three specifications. The estimated effect increases when UN sanctions are excluded, and even further when also EU sanctions are excluded from the sample. This would suggest that the impact grows as a leaders' say in imposing sanctions increases. A one-unit increase in leader similarity is associated with a 5.9 percentage point (pp) decrease in sanction probability, a 12.1 pp decrease when excluding UN sanctions are excluded, and a 17.9 pp when excluding both UN and EU sanctions.[16] Since the similarity variable is based on Pearson's correlation coefficient, the AME can be interpreted as the average observed shift from no correlation (0) to perfect correlation (1) in the characteristics of the leader pair.

In a second step, we control for the political conditions in the target country that are commonly considered the primary causes of sanction imposition (Hufbauer et al. 2009). Table 4.2 shows the AMEs derived from these estimations. Again, we pay particular attention to the more conservative model specifications corresponding to Columns (2), (4), and (6). The estimates are smaller than those in Table 4.1 and reach significance at the 10% level only in Column (2) and Column (4), that is, when UN or UN and EU sanctions are excluded. As before, the effect is strongest when both UN and EU sanctions are excluded: here, a one-unit increase in leader similarity corresponds to a 7.3 pp decrease in sanction occurrence.

16. This pattern is not replicated in the specifications without dyad fixed effects, where the estimated similarity effect remains stable across samples.

Table 4.2: Explaining Sanctions with Leader Similarity, Socio-Economic Variables, and Reasons for Sanction Imposition (Broad)

	(1) w/o UN/EU	(2) w/o UN/EU	(3) w/o UN	(4) w/o UN	(5) All Obs.	(6) All Obs.
Leader Similarity	–0.0142***	–0.0725*	–0.0259***	–0.0507*	–0.0164***	0.0185
	(0.0022)	(0.0390)	(0.0023)	(0.0295)	(0.0020)	(0.0133)
lag log(GDP pc)^T	0.0062***	–0.0822*	0.0040***	–0.1146***	0.0250***	–0.2343***
	(0.0011)	(0.0486)	(0.0011)	(0.0284)	(0.0008)	(0.0142)
lag log(Population)^T	0.0067***	0.5956***	0.0065***	0.8705***	–0.0055***	–0.0011
	(0.0006)	(0.1628)	(0.0006)	(0.1050)	(0.0007)	(0.0137)
lag Globalization^T	–0.0007***	–0.0064**	–0.0008***	–0.0027	–0.0032***	–0.0115***
	(0.0001)	(0.0025)	(0.0001)	(0.0019)	(0.0001)	(0.0007)
lag Democracy^T	–0.0290***	–0.2962***	–0.0307***	–0.2717***	–0.0189***	–0.2951***
	(0.0020)	(0.0412)	(0.0020)	(0.0368)	(0.0016)	(0.0202)
lag Human Rights^T	–0.0052***	–0.0811***	–0.0080***	–0.0682***	–0.0433***	–0.1124***
	(0.0009)	(0.0209)	(0.0009)	(0.0172)	(0.0010)	(0.0069)
lag Successful Coups^T	0.0018	0.0594**	0.0085	0.0688***	–0.0030	0.0098
	(0.0040)	(0.0295)	(0.0039)	(0.0192)	(0.0027)	(0.0085)
lag Minor Conflict^T	0.0132***	0.1130***	0.0127***	0.0850***	0.0218***	0.0126
	(0.0017)	(0.0250)	(0.0018)	(0.0187)	(0.0014)	(0.0079)
lag Major Conflict^T	–0.0359***	0.0536	–0.0331***	0.0159	0.0450***	0.1292***
	(0.0034)	(0.0443)	(0.0032)	(0.0316)	(0.0019)	(0.0101)
Sender-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Dyad FE		Yes		Yes		Yes
Observations	165,364	7,160	167,012	11,924	293,594	51,097
Unique Dyads	11,801	431	11,990	563	13,242	1,937
Std. Dev. of Similarity	0.3504	0.3610	0.3508	0.3698	0.3505	0.3628

Notes: Marginal effects of panel probit models. The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level. Differences in the number of observations are due to (i) the exclusion of UN and EU sanctions or UN sanctions and (ii) the absorption of data spells by the fixed effects.

This reduction in effect size and significance is expected, given the correlation between political control variables and the target country's leader characteristics. One could, however, argue that by adding the political control variables, we are underestimating the effect of leader similarity on sanctions. If political leaders in target countries have some idea what they can get away with without getting sanctioned by a particular potential sender country, their decision to enter conflicts or commit human rights violations itself is affected by how similar they are to other political leaders who may consider sanctioning them. In that sense, the estimates in Tables 4.1 and 4.2 could be considered reasonable upper and lower bounds.

Turning briefly to the (lagged) control variables, we find that a higher GDP per capita in the target country reduces the likelihood of sanctions. Globalization shows a similar negative effect, though not in all samples. Sanctions are also less likely used against democratic countries and those with greater respect for human rights. While conflicts (and successful coups) generally increase the likelihood of sanctions, this effect is not uniform across models; in particular, the results for major conflicts may reflect the absorption of numerous spells and the exclusion of UN sanctions. Comparing the effect of leader similarity with that of the main causes of sanction imposition suggests that leader similarity's impact is—not too surprisingly—smaller than that of the standard control variables. Yet, the effect of leader similarity is empirically relevant, as demonstrated by the predicted probability of sanction imposition across its observed empirical range (from –0.09 to 1). In the most conservative model (Column 2 of Table 4.2), this probability increases from 2.3% at the highest observed leader similarity to 7.2% at the lowest.

4.4.2 Extensions and Robustness Tests

In line with our theoretical considerations and the empirical evidence provided above (but also to conserve space), all results hereafter are based on estimation samples without UN and EU sanctions.

Alternative Estimation Methods. To test the robustness of our results, we first apply alternative estimation techniques. Table C.5 presents the marginal effects from probit estimations that incorporate the incidental parameter bias correction method proposed by Fernández-Val (2009).[17] The absolute values of the AMEs with the narrow set of controls are smaller than those reported in Column (1) of Table 4.1, yet they remain highly statistically significant. Estimates using the broad set of controls are nearly identical to those in Column (1) of Table 4.2. These results suggest that our estimations are not substantially affected by an incidental parameter problem.

Next, we re-estimate our baseline models as linear probability models using the least squares method. The results are presented in Table C.6. In two of these models, we add linear and quadratic time trends for the target countries (Columns 5 and 6 are extensions of Column 4) to further control for unobserved factors affecting these countries.[18] Except in Column (1), the estimated effects are larger and exhibit higher levels of statistical significance compared to the baseline probit estimations (see Columns 1 and 2 of Tables 4.1 and 4.2). This provides further evidence justifying an interpretation of our baseline results as a conservative lower bound for the effect of leader similarity on the likelihood of sanction imposition. Furthermore, our results remain robust and the estimated effect size even increases when we incorporate target country time trends in Columns 5 and 6.

Effect Heterogeneity. The GSDB records nine frequently coinciding categories of objectives or reasons for imposing sanctions (democracy, human rights, destabilize regime, policy change, prevent war, end war, territorial conflict, terrorism, and "others") based on information in official documents. To test if the effect of leader similarity depends on the context in which sanctions are imposed, we re-estimate our models,

17. These estimations converge when using sender-year fixed effects but fail to do so when dyad fixed effects are also included. Accordingly, we cannot employ bias correction as the default estimation method.

18. Probit models with these trends face convergence issues similar to those resulting from the use of target-year fixed effects (see Endnote 14).

excluding sanction cases with one declared objective at a time.[19] Table 4.3 presents the results using dyad fixed effects.

While our findings are in general robust to this jackknife-style test based on sanction objectives, it is striking that excluding sanctions aimed at promoting democracy or improving human rights in the target country causes the estimated effects to become much smaller and statistically insignificant (or just marginally significant when excluding human rights sanctions in the estimations using the narrow set of controls). This suggests that leader similarity plays most into the decision whether to impose sanctions in situations where democratic change or improvements in human rights are sought. These are arguably the most controversial types of foreign interventions, due to legitimacy concerns and their questionable track record (see, e.g., Coyne 2008; Steinbach et al. 2023). It is, therefore, not surprising that political leaders appear to have more discretion in these decisions than with regard to sanctions that target, for example, supporters or instigators of conflict and terrorism. Consistent with that interpretation, excluding conflict- or terrorism-related sanctions from the sample does not alter the results. The same holds for sanctions aiming at regime destabilization, policy changes, and other objectives. In fact, some coefficients are even larger than the baseline (see the bottom right panel of Table 4.3), indicating that leader similarity is less relevant to sanction imposition when these objectives are concerned.

19. The majority of sanctions have multiple objectives. When we exclude sanctions with one objective from the estimation, these sanctions, therefore, may have also had a number of other objectives. Our exercise here should, thus, be considered a robustness test rather than a way to cleanly separate sanctions with particular goals.

Table 4.3: Effect Heterogeneity for Sanction Objectives (w/o UN/EU Sanctions)

	Excl. "Democracy"		**Excl. "Human Rights"**	
	Narrow	Broad	Narrow	Broad
Leader Similarity	–0.0444	0.0095	–0.1068*	–0.0141
	(0.0657)	(0.0567)	(0.0555)	(0.0522)
Observations	4,452	4,452	4,813	4,813
Unique Dyads	296	296	271	271
	Excl. "Destab. Regime"		**Excl. "Policy Change"**	
	Narrow	Broad	Narrow	Broad
Leader Similarity	–0.1998***	–0.0924**	–0.2105***	–0.1453**
	(0.0410)	(0.0356)	(0.0680)	(0.0650)
Observations	7,048	7,048	3,160	3,160
Unique Dyads	429	429	197	197
	Excl. "Prevent War"		**Excl. "End War"**	
	Narrow	Broad	Narrow	Broad
Leader Similarity	–0.2242***	–0.1239*	–0.1746***	–0.0642*
	(0.0766)	(0.0686)	(0.0455)	(0.0389)
Observations	3,065	3,065	6,956	6,956
Unique Dyads	191	191	425	425
	Excl. "Territ. Conflict"		**Excl. "Terrorism"**	
	Narrow	Broad	Narrow	Broad
Leader Similarity	–0.1921***	–0.0872**	–0.1793***	–0.0791**
	(0.0441)	(0.0377)	(0.0455)	(0.0390)
Observations	6,982	6,982	6,770	6,770
Unique Dyads	424	424	414	414
	Excl. "Other Objectives"		**Full Dataset**	
	Narrow	Broad	Narrow	Broad
Leader Similarity	–0.2299***	–0.1275***	–0.1792***	–0.0725*
	(0.0424)	(0.0388)	(0.0450)	(0.0390)
Observations	6,688	6,688	7,160	7,160
Unique Dyads	417	417	431	431

Notes: Marginal effects of panel probit models. The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level. The differences in the number of observations are due to the exclusion of sanctions of a particular objective. Models include control variables (Narrow: lag log(GDP pc)^T, lag log(Population)^T, and lag Globalization^T; Broad (additionally): lag Democracy^T, lag Human Rights^T, lag Successful Coups^T, and two lagged conflict indicators), sender-year fixed effects, and dyad fixed effects. Estimates are available on request. "Full Dataset" replicates the results from Column (2) of Tables 4.1 and 4.2, respectively.

A second set of jackknife-style tests examines additional effect heterogeneity. Table 4.4 first presents results when sanctions from specific periods are excluded. When post-Cold War sanctions are omitted, the effects of leader similarity are smaller than in the baseline estimations (shown again in the bottom right panel of Table 4.4). In contrast, excluding Cold War sanctions amplifies these effects. This suggests that leader similarity has played a greater role since 1991. This finding is unsurprising, as—at least within our sample period, which ends in 2004—the world was no longer divided into two rival blocs. As a result, leaders had greater discretion in shaping foreign policy.

Next, the estimations exclude sanctions imposed on democratic countries, which strengthens the effect of leader similarity on sanction decisions, suggesting that the targeting of autocratic countries is more influenced by leader similarity. This aligns with the findings in Table 4.3 where sanctions aimed at promoting democratic change drive the overall results. A similar pattern emerges when US sanctions are excluded. Since this subsection already excludes UN and EU sanctions, the share of non-democratic sanctioning countries naturally increases significantly. Autocratic leaders typically have greater discretion in imposing sanctions, which explains the significantly stronger observed effect. Finally, removing trade sanctions—the most common type—also amplifies the effect of leader similarity. This suggests that the imposition of trade sanctions is constrained by a country's economic interests, diminishing the role of leader similarity compared to non-trade sanctions (e.g., arms, military, financial, travel, and "other" sanctions).[20]

20. Excluding sanctions against non-democracies, non-US sanctions, and non-trade sanctions—combined with the absorption of spells with no within-dyad variation—results in treatment groups that are too small for meaningful estimations.

Table 4.4: Effect Heterogeneity for Further Categories (w/o UN/EU Sanctions)

	Excl. Post-Cold War Sanctions		**Excl. Cold War Sanctions**	
	Narrow	Broad	Narrow	Broad
Leader Similarity	–0.2038**	–0.1115	–0.2281***	–0.1228**
	(0.0950)	(0.1399)	(0.0640)	(0.0624)
Observations	1,356	1,356	2,699	2,699
Unique Dyads	89	89	306	306
	Excl. Sanctions ag. Democracies		**Excl. US Sanctions**	
	Narrow	Broad	Narrow	Broad
Leader Similarity	–0.3263***	–0.0974***	–0.2454***	–0.2014***
	(0.0493)	(0.0350)	(0.0657)	(0.0618)
Observations	5,075	5,075	3,849	3,849
Unique Dyads	325	325	284	284
	Excl. Trade Sanctions		**Full Dataset**	
	Narrow	Broad	Narrow	Broad
Leader Similarity	–0.3480***	–0.2647***	–0.1792***	–0.0725*
	(0.0708)	(0.0948)	(0.0450)	(0.0390)
Observations	2,044	2,044	7,160	7,160
Unique Dyads	142	142	431	431

Notes: Marginal effects of panel probit models. The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level. The differences in the number of observations are due to the exclusion of sanctions of a particular category. Models include control variables (Narrow: lag log(GDP pc)^T, lag log(Population)^T, and lag Globalization^T; Broad (additionally): lag Democracy^T, lag Human Rights^T, lag Successful Coups^T, and two lagged conflict indicators), sender-year fixed effects, and dyad fixed effects. Estimates are available on request. "Full Dataset" replicates the results from Column (2) of Tables 4.1 and 4.2, respectively.

Dyadic Confounders. Our baseline models do not include indicators for the general affinity between two countries, as such variables could be heavily influenced by leader similarity and would thus be post-treatment confounders (i.e., bad control variables). However, controlling for several dyadic confounders remains a useful robustness test. We use a rather crude binary country affinity measure from Tomashevskiy (2025), which is quite stable over time and unlikely to be immediately affected by shifts in leader characteristics. Joint Democracy is coded one if both states score at least six on the polity2 index (Marshall and Gurr 2020). Rivalry is a binary variable based on the work of Thompson and Dreyer (2011) and operationalized by DiLorenzo and Rooney (2025). Joint IGOs counts the number of intergovernmental organizations in which both states are members (Pevehouse et al. 2021). These dyadic confounders are

largely consistent with those proposed by DiLorenzo and Rooney (2025). As shown in Table C.7, the results remain virtually the same as those in Column (2) of Table 4.2 after these dyadic confounders are individually and jointly controlled for. This reiterates that leader similarity does not just reflect the affinity between countries, which is anyway in large parts captured by the dyad fixed effects.

Leader Characteristics. Our next modification to the baseline model deals with variants of the leader similarity variable. As explained in Section 4.3.1 and Endnote 12, the original indicator is based on the correlation of 58 characteristics, spanning seven categories, between two leaders. Here, we conduct two robustness tests. First, we recalculate the index for each leader pair, but omitting one category of characteristics at a time. Second, we construct a theory-derived version of the similarity measure that includes only characteristics that can be theoretically justified.[21] These alternative leader similarity indices then serve as explanatory variables in models otherwise corresponding to Column (2) of Tables 4.1 and 4.2. Table C.8 presents the results. Excluding any single category of characteristics or basing the similarity indicator only on theoretically justified characteristics leaves the results virtually unchanged—except if we exclude political experience, which slightly increases both the estimated effect size and standard errors. Overall, these modifications suggest that no single category of characteristics or the consideration of theoretically implausible leader characteristics drives our results.

Alternative Similarity Metric. Our final robustness test concerns an alternative measurement of leader similarity, while using the same data as the original indicator. Gower's distance is calculated instead of the Pearson correlation coefficient, as it can directly account for nominal and ordinal variables. To make it comparable with the

21. Specifically, the 49 variables we identify as relevant are **Military experience:** Military service (combat/noncombat), military education, rebel participation, national war participation (victory/loss), and rebel war participation (victory/loss). **Education:** Primary education, boarding school, and education level. **Family variables, childhood:** parental status, orphan, royalty, father occupation and mother in labor force. **Family variables, adulthood:** Married (ever/in power), divorced (ever), number of spouses in life, number of sons/daughters, number of children/adopted children, and children died. **Occupation:** Education, journalism, law, engineering, medicine, science, agriculture, military career, religion, labor, activist, career politician, writer, film/music, aristocrat/landowner, police, and interpreter. **Political experience:** Years in politics before becoming leader and puppet leader.

original indicator, we subtract the Gower's distance metric from 1. Table C.9 presents the results of estimating probit models (Columns 1 and 2) and linear probability models (Columns 3 and 4) while using this alternative leader similarity indicator. Gower-based leader similarity also has a significant negative effect on sanction imposition. The marginal effects are larger than those in Tables 4.1, 4.2, and C.6, which can be explained by the lower standard deviation in Gower's similarity (0.06) compared to the correlation-based measure (0.35).

4.5 Conclusion

In this study, we seek to determine whether international sanctions are less likely imposed between pairs of political leaders who share greater similarity. To answer this question, we estimate panel probit models with sender-year and dyad fixed effects as well as different similarity measures and sets of target country control variables. Our dataset covers 293,594 observations corresponding to 156 potential sender countries, 155 potential target countries, and 13,242 directed country-pairs over the period from 1970 to 2004.

Our main finding suggests that higher leader similarity significantly reduces the likelihood of sanction imposition. This result holds across various specifications and estimation techniques, also when controlling for the primary reasons for sanction imposition. The effect is most pronounced when UN and EU sanctions are excluded, that is, when focusing on sanctions imposed following a unilateral political decision-making process. The probability of sanction imposition rises from 2.3% at the highest observed leader similarity in the sample to 7.2% at the lowest. Moreover, we find that the influence of leader similarity is strongest on the decision to impose sanctions aimed at democratic change or human rights improvements, where we argue that political leaders have some of the highest levels of discretion. Finally, leader similarity has become more important after the Cold War, and the imposition of sanctions from or against autocracies and of non-trade sanctions (e.g., arms and military sanctions) depends most on the similarity of the involved political leaders.

We study sanction imposition, but leader similarity may also influence the severity of imposed sanctions. It has been argued that weak sanctions are imposed to be perceived as doing something (Hovi et al. 2005). Weak sanctions may be imposed where only stronger sanctions would be effective, but the leader does not want to impose serious sanctions on the target and chooses weak sanctions to avoid looking unresponsive. This warrants further inquiry, but more and better data on sanction intensity is needed with a similar time and country coverage as in the Global Sanctions Data Base. Also with respect to leader characteristics, more detailed data and a broader country and time coverage would be desirable to further advance research on how leaders matter in international relations.

Finally, beyond our specific research question, our study contributes to the broader behavioral revolution in international relations (Hafner-Burton et al. 2017) and international law (van Aaken 2014; van Aaken and Simsek 2021). It highlights the potential of studying the relevance of leader types for foreign policy in a dyadic setting to better understand which combinations of leader types are conducive to international conflict or cooperation. Future studies may, for example, utilize recent advances in the measurement of politicians' projected persona through public statements (Birkenmaier and Lechner 2025). Projected personality traits can act both as moderators and mediators between more exogenous personality traits and policy outcomes. For our research question, it would seem particularly fruitful if public speech could be distinguished by whether it explicitly addresses international topics or audiences.

Appendix C

Appendix C1: Data Description

Table C.1: Countries

Afghanistan, Albania, Algeria, Angola, Argentina, Armenia, Australia, Austria, Azerbaijan, Bahrain, Bangladesh, Belarus, Belgium, Benin, Bhutan, Bolivia, Botswana, Brazil, Bulgaria, Burkina Faso, Burundi, Cambodia, Cameroon, Canada, Cape Verde, Central African Republic, Chad, Chile, China, Colombia, Comoros, Costa Rica, Croatia, Cuba, Cyprus, Czech Republic, Denmark, Djibouti, Dominican Republic, Ecuador, Egypt, El Salvador, Equatorial Guinea, Eritrea, Estonia, Ethiopia, Fiji, Finland, France, Gabon, Gambia, Georgia, Germany, Ghana, Greece, Guatemala, Guinea, Guinea-Bissau, Guyana, Haiti, Honduras, Hungary, India, Indonesia, Iran, Iraq, Ireland, Israel, Italy, Jamaica, Japan, Jordan, Kazakhstan, Kenya, Kuwait, Kyrgyzstan, Laos, Latvia, Lebanon, Lesotho, Liberia, Libya, Lithuania, Luxembourg, Madagascar, Malawi, Malaysia, Mali, Mauritania, Mauritius, Mexico, Moldova, Mongolia, Morocco, Mozambique, Myanmar, Namibia, Nepal, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, North Macedonia, Norway, Oman, Pakistan, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Poland, Portugal, Qatar, Romania, Russia, Rwanda, Saudi Arabia, Senegal, Serbia, Sierra Leone, Singapore, Slovakia, Slovenia, Solomon Islands, Somalia, South Africa, South Korea, Spain, Sri Lanka, Sudan, Suriname, Swaziland, Sweden, Switzerland, Syria, Tajikistan, Tanzania, Thailand, Togo, Tunisia, Türkiye, Turkmenistan, Uganda, Ukraine, United Arab Emirates, United Kingdom, United States of America, Uruguay, Uzbekistan, Venezuela, Vietnam, Yemen, Zambia, Zimbabwe.

Table C.2: Definitions of Variables and Data Sources

Variable	**Definition & Source**
Sanctions	Binary indicators for dyad-years with sanctions in place. *Source:* GSDB (Felbermayr et al. 2020; Kirilakha et al. 2021; Syropoulos et al. 2024).
Leader Similarity	Continuous measure of leader similarity calculated as Pearson's correlation coefficient (r) based on similarity data from the LEAD dataset. Weighted by the number of overlapping days of two leaders, if more than one leader pair exists in a dyad-year. *Source:* Leader Similarity (DiLorenzo and Rooney 2025), LEAD Dataset (Ellis et al. 2015).
Gower's Leader Similarity	Continuous measure of leader similarity calculated as the reversed Gower's Distance (or Gower-based similarity Gower 1971) based on similarity data from the LEAD dataset. Weighted by the number of overlapping days of two leaders, if more than one leader pair exists in a dyad-year. *Source:* LEAD Dataset (Ellis et al. 2015).
lag log(GDP pc)^T	Natural logarithm of real GDP per capita of the target country in USD, lagged by one year. *Source:* Trade and GDP Data (Gleditsch et al. 2002).
lag log(Population)^T	Natural logarithm of the population size of the target country, lagged by one year. *Source:* World Development Indicators (World Bank 2025).
lag Globalization^T	Index measuring the economic, social, and political dimensions of globalization in the target country, lagged by one year. *Source:* KOF Globalisation Index (Dreher 2006; Gygli et al. 2019).
lag Democracy^T	Binary democracy indicator based on the target country's polity2 index, which ranges from +10 (strongly democratic) to –10 (strongly autocratic). Coded "1" if the index is larger than five, lagged by one year. *Source:* Polity5 dataset (Marshall and Gurr 2020).

Table C.3: Definitions of Variables and Data Sources (Continued)

Variable	Definition & Source
lag Human Rights^T	Latent human rights variable with higher values indicating a better protection of human rights in the target country, lagged by one year. *Source:* Human Rights Protection Scores (Fariss 2019).
lag Successful Coups^T	Binary indicator coded "1" if a successful coup occurs in the target country, lagged by one year. *Source:* Coup Dataset (Bjørnskov and Rode 2020).
lag Minor Conflict^T / lag Major Conflict^T	Binary indicators coded "1" if armed conflicts resulting in 25 to 999 / at least 1,000 battle-related deaths occur in the target country, lagged by one year. *Source:* UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002; Davies et al. 2022).
Country Affinity	Binary indicator coded "1" for the presence of affinity links between country pairs. *Source:* Affinity Data (Tomashevskiy 2025).
Joint Democracy	Binary indicator coded "1" if the polity2 index of both sender and target is larger than five. *Source:* Constructed based on the Polity5 dataset (Marshall and Gurr 2020).
Rivalry	Binary indicator coded "1" if sender and target are classified as rivals, i.e., they perceive each other as threatening competitors. *Source:* Handbook of Interstate Rivalries (Thompson and Dreyer 2011).
Joint IGOs	Count variable indicating the number of intergovernmental organizations of which both sender and target are members. *Source:* Correlates of War Intergovernmental Organizations (Pevehouse et al. 2021).

Table C.4: Descriptive Statistics

Panel A: Full Dataset

	All Observations ($N = 293,594$)	No Sanctions ($N = 262,801$)	Sanctions ($N = 30,793$)
Continuous Variables (Mean Values)			
Leader Similarity	0.62	0.62	0.60
Gower's Leader Similarity	0.78	0.78	0.78
lag log(GDP pc)^T	8.32	8.37	7.88
lag log(Population)^T	9.06	9.03	9.29
lag Globalization^T	45.43	46.14	39.30
lag Human Rights^T	–0.07	0.03	–0.94
Joint IGOs	26.22	26.05	27.74
Binary Variables ($X = 1$ in percent)			
lag Democracy^T	34.21	36.14	17.68
lag Successful Coups^T	2.56	2.45	3.54
lag Minor Conflict^T	15.24	13.97	26.14
lag Major Conflict^T	6.12	4.95	16.05
Country Affinity	14.92	14.53	18.22
Joint Democracy	13.70	14.31	8.49
Rivalry	0.56	0.46	1.41

Panel B: Main Estimation Dataset (w/o UN/EU Sanctions and w/o Absorbed Spells)

	All Observations ($N = 7,160$)	No Sanctions ($N = 6,375$)	Sanctions ($N = 785$)
Continuous Variables (Mean Values)			
Leader Similarity	0.59	0.58	0.66
Gower's Leader Similarity	0.78	0.78	0.78
lag log(GDP pc)^T	8.11	8.10	8.12
lag log(Population)^T	9.67	9.67	9.69
lag Globalization^T	42.29	42.59	39.89
lag Human Rights^T	–0.25	–0.23	–0.40
Joint IGOs	24.99	24.70	27.36
Binary Variables ($X = 1$ in percent)			
lag Democracy^T	31.52	33.22	17.71
lag Successful Coups^T	2.75	2.51	4.71
lag Minor Conflict^T	17.07	17.22	15.80
lag Major Conflict^T	3.87	4.14	1.66
Country Affinity	14.82	14.38	18.34
Joint Democracy	12.85	12.99	11.72
Rivalry	0.67	0.66	0.76

Notes: Panel A shows descriptive statistics for the full dataset, Panel B for the dataset used in the estimations with sender-year and dyad fixed effects and without sanctions imposed by the UN or the EU.

Appendix C2: Additional Results

Table C.5: Bias Corrected Probit Estimations (w/o UN/EU Sanctions)

	(1) Narrow	(2) Broad
Leader Similarity	–0.0104***	–0.0148***
	(0.0020)	(0.0020)
lag log(GDP pc)^T	0.0027***	0.0047***
	(0.0008)	(0.0008)
lag log(Population)^T	0.0038***	0.0068***
	(0.0004)	(0.0005)
lag Globalization^T	–0.0006***	–0.0007***
	(0.0001)	(0.0001)
lag Democracy^T		–0.0273***
		(0.0017)
lag Human Rights^T		–0.0039***
		(0.0007)
lag Successful Coups^T		0.0009
		(0.0037)
lag Minor Conflict^T		0.0112***
		(0.0018)
lag Major Conflict^T		–0.0357***
		(0.0033)
Sender-Year FE	Yes	Yes
Observations	165,364	165,364
Unique Dyads	11,801	11,801
Std. Dev. of Similarity	0.3504	0.3504

Notes: Marginal effects of bias-corrected panel probit models according to Fernández-Val (2009). The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level.

Table C.6: Linear Probability Models (w/o UN/EU Sanctions)

	(1) Narrow	(2) Broad	(3) Narrow	(4) Broad	(5) Broad	(6) Broad
Leader Similarity	–0.0245***	–0.0198***	–0.1934***	–0.1016**	–0.1585***	–0.1507***
	(0.0028)	(0.0028)	(0.0448)	(0.0402)	(0.0307)	(0.0269)
lag log(GDP pc)^T	0.0062***	0.0053***	–0.0730*	–0.0642	–0.1064**	–0.1845***
	(0.0013)	(0.0013)	(0.0417)	(0.0420)	(0.0416)	(0.0317)
lag log(Population)^T	0.0086***	0.0090***	0.7962***	0.5588***	0.0970	–0.2604***
	(0.0005)	(0.0007)	(0.1379)	(0.1218)	(0.0880)	(0.0905)
lag Globalization^T	–0.0013***	–0.0008***	–0.0096***	–0.0058**	–0.0085***	–0.0152***
	(0.0001)	(0.0001)	(0.0029)	(0.0029)	(0.0022)	(0.0020)
lag Democracy^T		–0.0281***		–0.3218***	–0.1858***	–0.1148***
		(0.0017)		(0.0351)	(0.0264)	(0.0246)
lag Human Rights^T		–0.0003		–0.0515**	–0.1401***	–0.0588**
		(0.0008)		(0.0205)	(0.0192)	(0.0264)
lag Successful Coups^T		0.0001		0.0717***	0.0977***	0.0875***
		(0.0050)		(0.0276)	(0.0245)	(0.0201)
lag Minor Conflict^T		0.0104***		0.1127***	0.0119	0.0329
		(0.0021)		(0.0339)	(0.0235)	(0.0210)
lag Major Conflict^T		–0.0325***		0.0596	–0.0781**	–0.0338
		(0.0028)		(0.0453)	(0.0354)	(0.0287)
Sender-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Dyad FE			Yes	Yes	Yes	Yes
Linear Target Country Trends					Yes	Yes
Quadratic Target Country Trends						Yes
Observations	165,364	165,364	7,160	7,160	7,160	7,160
Unique Dyads	11,801	11,801	431	431	431	431
Std. Dev. of Similarity	0.3504	0.3504	0.3610	0.3610	0.3610	0.3610

Notes: Coefficients of linear probability models. The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level. Differences in the number of observations are due to the absorption of data spells by the fixed effects.

Table C.7: Controlling for Dyadic Confounders (w/o UN/EU Sanctions)

	(1)	(2)	(3)	(4)	(5)
Leader Similarity	–0.0739*	–0.0688*	–0.0723*	–0.0887**	–0.0830**
	(0.0390)	(0.0387)	(0.0390)	(0.0408)	(0.0413)
Country Affinity	–0.0410				–0.0003
	(0.0294)				(0.0268)
Joint Democracy		–0.0708			–0.0829
		(0.1277)			(0.1430)
Rivalry			–0.5228*		–0.6616**
			(0.2910)		(0.3248)
Joint IGOs				–0.0207***	–0.0208***
				(0.0045)	(0.0043)
lag log(Population)^T	0.5928***	0.5958***	0.5952***	0.3241**	0.3229**
	(0.1621)	(0.1629)	(0.1627)	(0.1412)	(0.1416)
lag log(GDP pc)^T	–0.0836*	–0.0834*	–0.0820*	–0.1246***	–0.1265***
	(0.0489)	(0.0487)	(0.0486)	(0.0476)	(0.0479)
lag Globalization^T	–0.0062**	–0.0064**	–0.0064**	–0.0064***	–0.0063***
	(0.0025)	(0.0025)	(0.0025)	(0.0024)	(0.0024)
lag Democracy^T	–0.2940***	–0.2314*	–0.2960***	–0.2267***	–0.1517
	(0.0410)	(0.1226)	(0.0413)	(0.0368)	(0.1334)
lag Human Rights^T	–0.0796***	–0.0810***	–0.0810***	–0.0797***	–0.0796***
	(0.0209)	(0.0209)	(0.0209)	(0.0236)	(0.0237)
lag Successful Coups^T	0.0614**	0.0608**	0.0594**	0.0836***	0.0852***
	(0.0295)	(0.0296)	(0.0295)	(0.0282)	(0.0285)
lag Minor Conflict^T	0.1139***	0.1138***	0.1130***	0.1013***	0.1025***
	(0.0253)	(0.0252)	(0.0250)	(0.0237)	(0.0241)
lag Major Conflict^T	0.0560	0.0546	0.0537	0.0486	0.0500
	(0.0444)	(0.0443)	(0.0443)	(0.0479)	(0.0480)
Sender-Year FE	Yes	Yes	Yes	Yes	Yes
Dyad FE	Yes	Yes	Yes	Yes	Yes
Observations	7,160	7,160	7,160	7,156	7,156
Unique Dyads	431	431	431	431	431
Std. Dev. of Similarity	0.3610	0.3610	0.3610	0.3610	0.3610

Notes: Marginal effects of panel probit models. The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level. Differences in the number of observations are due to the absorption of data spells by the fixed effects.

Table C.8: Jackknife-Style Exclusion of Leader Characteristics (w/o UN/EU Sanctions)

	(1) Narrow	(2) Broad
excl. Military Experience	–0.1764***	–0.0730*
	(0.0459)	(0.0394)
excl. Education	–0.1644***	–0.0663*
	(0.0405)	(0.0357)
excl. Childhood Family	–0.1745***	–0.0710*
	(0.0441)	(0.0382)
excl. Adult Family	–0.1743***	–0.0712*
	(0.0440)	(0.0384)
excl. Occupation	–0.1701***	–0.0677*
	(0.0426)	(0.0370)
excl. Political Experience	–0.1999***	–0.0835
	(0.0731)	(0.0742)
excl. Other	–0.1803***	–0.0724*
	(0.0449)	(0.0389)
Theory-based	–0.1755***	–0.0705*
	(0.0443)	(0.0385)
All Characteristics	–0.1792***	–0.0725*
	(0.0450)	(0.0390)
Observations	7,160	7,160
Unique Dyads	431	431

Notes: Marginal effects of panel probit models. The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level. Models include control variables (Narrow: lag log(GDP pc)^T, lag log(Population)^T, and lag Globalization^T; Broad (additionally): lag Democracy^T, lag Human Rights^T, lag Successful Coups^T, and two lagged conflict indicators), sender-year fixed effects, and dyad fixed effects. Estimates are available on request. "All Characteristics" replicates the results from Column (2) of Tables 1 and 2, respectively.

Table C.9: Gower's Leader Similarity (w/o UN/EU Sanctions)

	(1)	(2)	(3)	(4)
	Probit Models		Linear Probability Models	
	Narrow	Broad	Narrow	Broad
Gower's Leader Similarity	–0.7277***	–0.3979**	–0.7687***	–0.3313*
	(0.2177)	(0.1987)	(0.2097)	(0.1985)
lag log(GDP pc)^T	–0.0992*	–0.0772	–0.0650	–0.0611
	(0.0527)	(0.0495)	(0.0419)	(0.0420)
lag log(Population)^T	0.8585***	0.6146***	0.7729***	0.5431***
	(0.1781)	(0.1639)	(0.1347)	(0.1195)
lag Globalization^T	–0.0113***	–0.0068***	–0.0113***	–0.0065**
	(0.0028)	(0.0025)	(0.0030)	(0.0024)
lag Democracy^T		–0.3050***		–0.3315***
		(0.0428)		(0.0000)
lag Human Rights^T		–0.0829***		–0.0520**
		(0.0209)		(0.0120)
lag Successful Coups^T		0.0683**		0.0810***
		(0.0288)		(0.0026)
lag Minor Conflict^T		0.1111***		0.1099***
		(0.0245)		(0.0011)
lag Major Conflict^T		0.0481		0.0497
		(0.0452)		(0.2796)
Sender-Year FE	Yes	Yes	Yes	Yes
Dyad FE	Yes	Yes	Yes	Yes
Observations	7,160	7,160	7,160	7,160
Unique Dyads	431	431	431	431
Std. Dev. of Similarity	0.0662	0.0662	0.0662	0.0662

Notes: Marginal effects of panel probit models and coefficients of linear probability models. The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level.

5 International Sanctions and Corruption

"International Sanctions and Corruption"

with Prof. Dr. Matthias Neuenkirch and Dr. Jerg Gutmann

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.

5.1 Introduction

Empirical research has demonstrated the detrimental consequences of international sanctions for target economies (Gutmann, Neuenkirch, and Neumeier 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, Neuenkirch, and Neumeier 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, Neuenkirch, and Neumeier (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.[22] Tabellini (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.[23] 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-

22. 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.

23. 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).[24] 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 5,843 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

24. 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 5.2 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 5.3 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 5.4, we discuss our empirical findings alongside several extensions and robustness tests, before Section 5.5 concludes.

5.2 Theory

Corruption can be defined as any abuse of entrusted power for private gain.[25] 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).

25. 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.[26] 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.*

26. 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.[27] 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-

27. 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.*

5.3 Estimation Strategy and Data

5.3.1 Estimation Strategy

To evaluate how sanctions affect target country public-sector corruption, we follow Gutmann, Neuenkirch, and Neumeier (2023a) and Gutmann, Langer, 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:

$$y_{i,t} = \sum_{s=1}^{6} \beta^{s}_{sanc} D^{s}_{sanc,i,t-1} + \gamma_1 X^{pol}_{i,t-1} + \gamma_2 X^{econ}_{i,t-1} + \alpha_i + \tau_t + \epsilon_{i,t} \tag{1}$$

The dependent variable, $y_{i,t}$, measures the level of corruption based on country expert assessments.[28] The analysis is conducted at the country-year level, where i denotes a country in year t. Our key explanatory variables, $D^{s}_{sanc,i,t-1}$, 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).[29] 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-

28. 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.

29. 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, α_i, and year fixed effects, τ_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, Neuenkirch, and Neumeier 2023a; Gutmann, Langer, et al. 2024; Lohaus and Bussmann 2021). These consist of a vector of political indicators, $X^{pol}_{i,t-1}$, and economic variables, $X^{econ}_{i,t-1}$. 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, Neuenkirch, and Neumeier 2023a; Gutmann, Langer, 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).

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

The event study specification incorporates all control variables, $X_{i,t-1}^{pol}$ and $X_{i,t-1}^{econ}$, country fixed effects, α_i, year fixed effects, τ_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 5.3.2 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, $D_{sanc,i,tl}^{West\times Regime}$, 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.[30] $D_{pre,i,t-l}^{West\times Regime}$ 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.[31] 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

30. 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.

31. 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.

sanction indicators 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).

5.3.2 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.[32] 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-

32. 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.

variate correlations between the indices range from 0.92 to 0.98. They are, thus, very closely aligned, but not identical.[33]

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; Kirilakha 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 Inequality 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 5,843 observations for the period 1971 to 2019, including 1,531 country-years under sanctions (see Table D.5). Western sanctions account for the vast majority of cases (1,046 observations against 96 countries), while UN and

33. 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 D.1. Definitions and data sources for all variables are provided in Table D.3, while descriptive statistics are reported in Table D.4. 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.

5.4 Empirical Results

5.4.1 Baseline Results

Average Treatment Effects. Table 5.1 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.[34] 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.

34. 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 5.1: Baseline Results

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

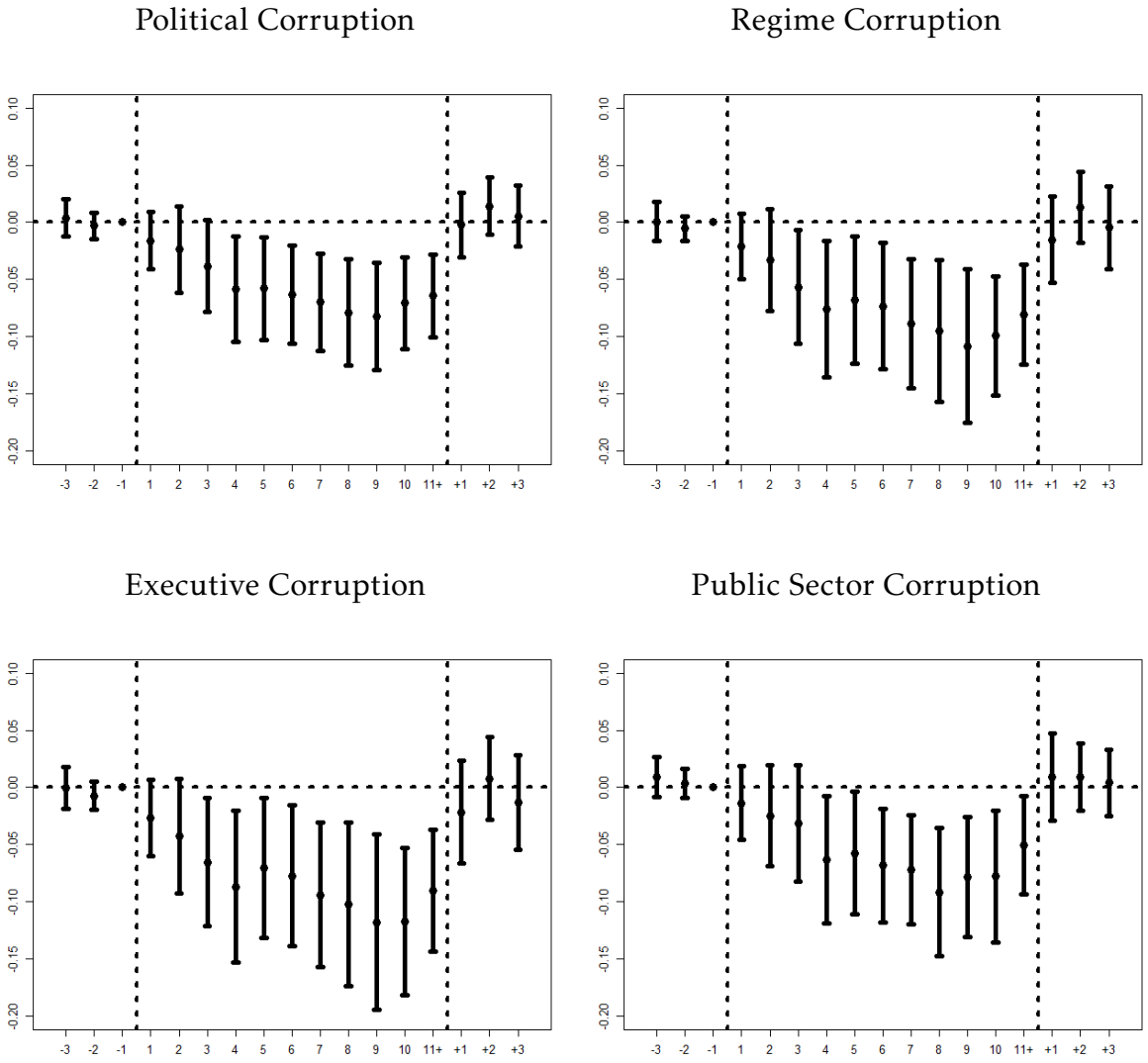
Effects of Sanctions over Time. Figure 5.1 shows the point estimates and corresponding 95% confidence intervals for Western sanctions targeting democracies, covering

the three years prior to a sanctions episode (−3, −2, −1), the duration of the episode (1, 2, …, 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 5.1, 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 D.5).[35]

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.

35. Figure D.1 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 5.1: 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*(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.

5.4.2 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.[36] 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 5.2. 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 D.5). 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.[37] Hence, our findings are not driven by transition phases.

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

37. Results available upon request.

Table 5.2: Effect Heterogeneity for Sanction Objectives

	Regime Corruption		**Political Corruption**	
Sanctions	**Democrat. & Human Rights**	**Other Objectives**	**Democrat. & Human Rights**	**Other Objectives**
… lag Western ag. Democracies	–0.1035***	–0.0502**	–0.0624**	–0.0493***
	(0.0379)	(0.0214)	(0.0260)	(0.0167)
… lag Western ag. Autocracies	0.0250	–0.0218	0.0249	–0.0268
	(0.0202)	(0.0171)	(0.0186)	(0.0141)
Observations	4,664	5,159	4,664	5,159
R^2	0.919	0.919	0.937	0.936
Within-R^2	0.066	0.049	0.067	0.058
	Executive Corruption		**Public Sector Corruption**	
Sanctions	**Democrat. & Human Rights**	**Other Objectives**	**Democrat. & Human Rights**	**Other Objectives**
… lag Western ag. Democracies	–0.1257***	–0.0494**	–0.0630**	–0.0496***
	(0.0471)	(0.0239)	(0.0303)	(0.0183)
… lag Western ag. Autocracies	0.0294	–0.0174	0.0475	–0.0352**
	(0.0219)	(0.0187)	(0.0315)	(0.0142)
Observations	4,664	5,159	4,664	5,159
R^2	0.900	0.901	0.908	0.911
Within-R^2	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*(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.[38] The results are shown in Table D.6. 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 5.1, 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 − LDV coefficient) are slightly larger than those in the baseline models.[39]

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.[40] Table D.7 presents the results. The estimated effects of Western sanctions against democracies remain virtually unchanged compared to Table 5.1, while the corruption-reducing effects of UN sanctions against democracies appear slightly stronger.

38. 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).

39. 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).

40. 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 D.7.

5.5 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 5,843 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).

Appendix D

Appendix D1: Data Description

Table D.1: List of Countries

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).

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 D.2: Definitions of Variables and Data Sources

Variable	Definition & Source
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 ($v2xnp_regcorr$). This index is related to but narrower than the political corruption index. *Source:* 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 ($v2x_corr$). This index is related to but broader than the regime corruption index. *Source:* 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 ($v2x_execorr$). This index is part of the political corruption index and its constituent elements are part of the regime corruption index. *Source:* 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 ($v2x_pubcorr$). This index is part of the political corruption index. *Source:* 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. *Source:* GSDB, v4 (Felbermayr et al. 2020; Kirilakha et al. 2021; Syropoulos et al. 2024).

Table D.3: Definitions of Variables and Data Sources (continued)

lag *log*(GDP pc)	Natural logarithm of real GDP per capita in 2015 USD, lagged by one year. *Source:* 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. *Source:* 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. *Source:* World Inequality Database (2025).
lag Democracy	Binary variable indicating whether a country is democratic or not, lagged by one year. *Source:* 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. *Source:* 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. *Source:* 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. *Source:* 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. *Source:* UCDP/PRIO Armed Conflict Dataset, v25.1 (Gleditsch et al. 2002; Davies et al. 2022).

Table D.4: Descriptive Statistics

	All Observations		**No Sanctions**		**Sanctions**	
	Mean	*N*	Mean	*N*	Mean	*N*
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 *log*(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
	$X = 1$	*N*	$X = 1$	*N*	$X = 1$	*N*
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.

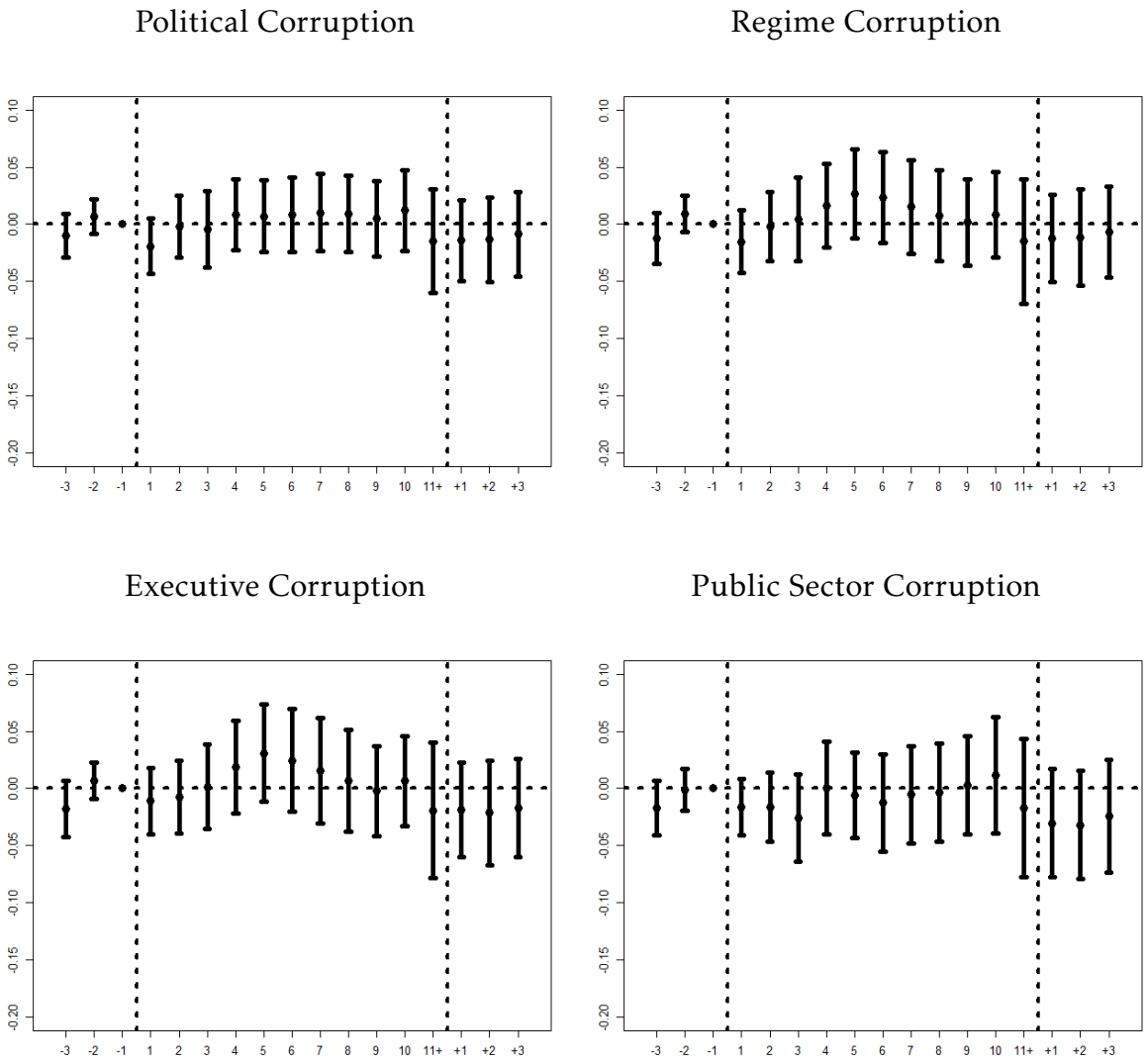
Table D.5: Frequency of Sanctions

Panel A: Panel DiD Estimations		**Panel B: Event Study Approach**	
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
Against Democracies		… Year 5	70
… Western	429 [81; 328]	… Year 6	65
… UN	43 [8; 20]	… Year 7	57
… Non-Western	252 [3; 239]	… Year 8	47
		… Year 9	46
Against Autocracies		… Year 10	34
… Western	617 [257; 196]	… Year 11+	323
… UN	206 [3; 80]		
… Non-Western	32 [0; 20]	… Post-Trend +1 Year	90
		… Post-Trend +2 Years	82
		… Post-Trend +3 Years	77

Notes: Frequency of observations of the different sanction indicators for which all control variables (see Table E.5) 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."

Appendix D2: Additional Results

Figure D.1: 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*(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 D.6: Results Controlling for Persistence in Corruption

	(1) **Regime Corruption**	(2) **Political Corruption**	(3) **Executive Corruption**	(4) **Public Sector Corruption**
lag Dependent Variable	0.9063***	0.9150***	0.8994***	0.9070***
	(0.0103)	(0.0090)	(0.0108)	(0.0093)
Sanctions				
… lag Western ag. Democracies	–0.0102**	–0.0053*	–0.0135***	–0.0082**
	(0.0042)	(0.0031)	(0.0050)	(0.0038)
… lag Western ag. Autocracies	0.0019	0.0008	0.0031	–0.0015
	(0.0030)	(0.0024)	(0.0033)	(0.0023)
… lag UN against Democracies	–0.0151*	–0.0082	–0.0197**	–0.0129*
	(0.0091)	(0.0078)	(0.0100)	(0.0071)
… lag UN against Autocracies	0.0031	0.0048	0.0001	0.0006
	(0.0041)	(0.0035)	(0.0045)	(0.0041)
… lag Non-Western ag. Democ.	0.0040	0.0038	0.0050	0.0055**
	(0.0035)	(0.0025)	(0.0039)	(0.0025)
… lag Non-Western ag. Autoc.	0.0094	0.0067	0.0108	0.0087*
	(0.0155)	(0.0092)	(0.0179)	(0.0050)
Observations	5,839	5,839	5,839	5,839
R^2	0.9849	0.9898	0.9794	0.9842
Within-R^2	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*(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 D.7: Two-Stage Difference-in-Differences Results

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

6 International Sanctions and Constitutional Compliance

"International Sanctions and Constitutional Compliance"

with Prof. Dr. Matthias Neuenkirch and Dr. Jerg Gutmann

Abstract

Judging governments' responses to international sanctions based on their compliance with a universal human rights standard can be criticized as an imposition of Western values. We propose an alternative benchmark for government action that is not subject to the same potential criticism: whether governments comply with their own national constitutions as codified forms of the social contract. Our analysis of 182 countries from 1962 to 2022 using state-of-the-art panel DiD and event study estimators corroborates our theoretical expectation that government responses depend on regime type. Whereas democracies improve their constitutional compliance under sanction pressure, autocracies start complying even less. These results suggest that sanctions can only make governments comply with their legal commitments if those governments are dependent on broad popular support.

6.1 Introduction

Since the end of the Cold War, sanctions have become a key instrument in international politics to influence foreign governments' decision-making. Sanctions are used to internalize international externalities (Aidt et al. 2021), to benefit national or particularistic interests in the sender country (Kaempfer and Lowenberg 1988), and to enforce international legal norms (van Aaken and Simsek 2021). Much of the empirical research on the effects of international sanctions has examined how sanctions affect human rights in the target country. Empirical studies have systematically found that international sanctions undermine the protection of human rights (Adam and Tsarsitalidou 2019; Escribà-Folch 2012; Gutmann, Neuenkirch, and Neumeier 2020; Peksen and Drury 2009; Wood 2008), including those sanctions imposed with the declared goal of promoting human rights protection (Steinbach et al. 2023). Some authors have argued against human rights sanctions on an even more fundamental level, which may partially explain why these sanctions are often ineffective. Wall (1998), for example, criticizes human rights sanctions for assuming a consensus on universal human rights that may not exist and for disregarding the state's right to self-determination.[41] The targets of human rights or democracy-promoting sanctions also frequently denounce them as imperialist (Soest 2024). And even political leaders who were not subject to international sanctions have refused to be judged by a universal human rights standard. For example, Lee Kuan Yew, Singapore's founding prime minister, famously argued that Western human rights were unsuitable for Asian societies and instead championed "Asian values".

If the legitimacy of enforcing human rights and democratic participation against target country governments is drawn into question, this suggests that sanctions may instead have to be evaluated by a normative benchmark that target states would (hypothetically) agree to. The natural benchmark for this exercise would be the target state's own social contract (Boettke 2025; Buchanan 1975; Buchanan and Tullock 1965). In

41. This concern is consistent with the empirical finding that many populations' understanding of key concepts like democracy is authoritarian rather than liberal, implying effectively a reversal of the meaning when support for democracy is declared in such populations (Kirsch and Welzel 2019).

modern nation-states, the legally codified form of such a social contract is the constitution (Berggren 1996). In economics, a strand of research has recently emerged that makes constitutional compliance measurable and studies its causes and consequences (Gutmann, Metelska-Szaniawska, et al. 2024b; Voigt 2021). If the external enforcement of human rights through sanctions is questioned for lack of a consensus on universal human rights, external interference aimed at making a government comply with the national constitution cannot be subject to the same criticism, as long as the constitution itself was not externally imposed (Law 2018). Here, we ask whether international sanctions—and particularly those imposed to prevent human rights violations and promote democratic change—are harmful to constitutional compliance, as they are to human rights protection, or whether they incentivize target governments to align their actions with constitutional commitments.

To study the effects of sanctions on constitutional compliance, we combine version 3.0 of the Comparative Constitutional Compliance Database (Gutmann, Metelska-Szaniawska, et al. 2024b) with indicators constructed from the Global Sanctions Data Base (Felbermayr et al. 2020; Kirilakha et al. 2021; Syropoulos et al. 2024). The resulting dataset covers the six decades from 1962 to 2022 for 182 potential target countries. To identify causal effects, we rely on difference-in-differences (DiD) estimators and event study designs. These identification strategies have evolved rapidly in recent years, triggered primarily by concerns about bias arising from the combination of staggered treatments and potentially heterogeneous treatment effects. To address these concerns, we use state-of-the-art estimation techniques. Specifically, we rely on the Two-Stage DiD estimator (2SDiD, see Butts and Gardner 2022; Gardner et al. 2025) and implement event study analyses using the Local Projections Difference-in-Differences estimator (LP-DiD, see Dube et al. 2025), which adapts the local projection method developed by Jordà (2005) to DiD settings.

We find that Western sanctions and United Nations (UN) sanctions imposed against democracies indeed lead to an improvement in constitutional compliance. The effect is even more pronounced when sanctions were imposed with the objective to promote

democracy or human rights protection in the target country. An analysis of constitutional compliance in specific legal dimensions shows that basic human rights and political rights in particular are increasingly complied with. Sanctions against autocracies, however, have the opposite effect. Consistent with prior findings on the adverse effects of sanctions on human rights protection reported in the literature, we find that Western sanctions and UN sanctions imposed against autocracies cause an accelerated decline in constitutional compliance. These adverse effects on compliance with the constitution are not focused on particular legal domains. For non-Western sanctions, that is, those imposed by China and Russia, we find no effects on constitutional compliance, independent of the target regime type, sanction objective, or legal domain in question. Sanctions aimed at improving democracy or human rights do not perform better than those imposed with other declared objectives.

Our study contributes to two strands of research at the interface of economics, law, and political science. The first literature studies the often unintended consequences of international sanctions (see Gutmann and Neuenkirch 2025; Gutmann, Neuenkirch, and Neumeier 2023b; 2025, for surveys of that literature). Aside from adverse effects on human rights protection, this literature has demonstrated declining income (Gutmann, Neuenkirch, and Neumeier 2023a; Neuenkirch and Neumeier 2015), increasing inequality (Afesorgbor and Mahadevan 2016; Neuenkirch and Neumeier 2016), and diminished health (Gutmann et al. 2021; Rodríguez et al. 2025) as some of the collateral effects of sanctions on target country populations. The second strand of literature deals with the determinants of government compliance with constitutions (e.g., Bologna Pavlik and Young 2026; Gutmann, Metelska-Szaniawska, et al. 2024a, 2024b, 2026; Lewkowicz et al. 2025). This research has found that the process of constitution-making, the contents of the constitution, and the traits of political leaders and the political system can all be relevant for how much the government chooses to comply with the constitution. So far, only one study has linked sanctions to constitutional compliance, but without a detailed analysis or efforts to identify causal effects. Choutagunta

et al. (2024) find that the first two years of a sanction episode tend to exhibit reduced levels of constitutional compliance compared to years without sanctions.[42]

The remainder of this paper is structured as follows. In Section 6.2, we develop our theoretical predictions and derive a set of hypotheses. The data and estimation strategy used to test these hypotheses are presented in Section 6.3. The results of our hypothesis tests, including a set of extensions and robustness tests, are discussed in Section 6.4. Section 6.5 concludes with a summary of results and outlines some open research questions.

6.2 Theory

International sanctions are a political instrument that is used to exert influence on foreign governments (Aidt et al. 2021). As such, both a positive and a negative effect of sanctions on government compliance with the constitution is conceivable. On the one hand, sanctions exert pressure and are typically imposed in combination with a promise that they will be lifted if the sender's demands are met. Sanctions by the European Union (EU) and the United States (US) against Russia after its full-scale invasion of Ukraine, for example, have been linked to ending hostilities and reversing territorial violations. Sanctions are, however, also frequently used to promote human rights protection or democratic reforms. The EU, for example, has imposed sanctions against China for alleged abuses against Uyghurs in the Chinese province of Xinjiang. If sanctions provide strong enough incentives for target state governments to change their unconstitutional policies, they may cause significant improvements in constitutional compliance. Aside from directly exerting pressure on the national government, sanctions may also create a focal point for coordinating citizens' resistance against violations of constitutional rules (Weingast 1997).

On the other hand, sanctions have different effects that may undermine constitutional compliance. First, sanctions deplete the government's resources used for buying

42. Choutagunta et al. (2024) study sanctions only as one among a variety of extreme events that could temporarily diminish constitutional compliance. The focus of their sanction indicator on the first two years of a sanction episode is based on the finding by Gutmann, Neuenkirch, and Neumeier (2023a) that this is where the significant adverse income effects are concentrated.

loyalty from critical supporters. This may shift the political survival strategy of the incumbent, especially in autocracies, toward using more state repression to deter political challengers. If sanctions are imposed by states that are considered to be (or framed as) an external threat to the nation, target governments may benefit from a rally-around-the-flag effect (Farzanegan and Gholipour 2026; RezaeeDaryakenari et al. 2025), where large shares of society unite behind their government. That gain in support may allow the government to get away even with otherwise unpopular violations of the constitution. Beyond the government's enhanced general ability to commit constitutional transgressions, sanctions may trigger emergency powers that allow the government to operate temporarily outside the constitutional rules that apply in normal times (Bjørnskov and Voigt 2024). Oftentimes, such emergency powers are interpreted broadly during a political crisis and potential abuses of these powers are only determined after the end of the crisis, if at all.

Taken together, these theoretical arguments suggest that the effect of sanctions on constitutional compliance is theoretically ambiguous, as can be expressed in the following set of hypotheses:

Hypothesis 1a. *States under international sanctions experience an increase in the government's compliance with the constitution.*

Hypothesis 1b. *States under international sanctions experience a decrease in the government's compliance with the constitution.*

These predicted general effects may, however, hide differential effects of sanctions on constitutional compliance depending on the regime type of the sender and target governments. Unlike the average effects of sanctions, this effect heterogeneity is not subject to comparable theoretical ambiguity. Regarding the identity of the sanctions sender, we distinguish three (categories of) actors. Western sanctions are imposed by the EU, the US, or both. UN sanctions are decided by the United Nations Security Council (UNSC), specifically through resolutions adopted under Chapter VII of the UN Charter. The UNSC's 15 members vote on sanctions resolutions: five permanent members (the US, the UK, France, China, and Russia) with veto power and ten

non-permanent members. A resolution passes with nine affirmative votes and no vetoes from permanent members. Finally, non-Western sanctions are those imposed by China, Russia, or both.

Beyond the identity of the sender, heterogeneity may also be captured by the stated objectives of sanctions. In particular, one can ask whether sanctions are imposed with the publicly declared goal of promoting human rights or democracy.[43] For EU and US sanctions, human rights and democracy promotion are most likely to be a relevant consideration—both have specific legal regimes for that purpose. Also UN sanctions can be imposed with the goal of stopping human rights violations, as the protection of human rights is part of the UN's mandate. While the UN is not charged with promoting democracy, it tends to pursue democracy support (e.g., through its constitutional assistance program or election monitoring) based on the argument that democratic institutions are conducive to the achievement of several of the UN's main goals: protecting human rights, fostering peace, and enabling human development. Finally, non-Western sanctions are very unlikely to consider or target human rights violations or promote democracy. China and Russia themselves are not democracies and have poor human rights records. Their sanctions tend to be reactive measures against Western interventions than to aim at improvements in the governance of target nations (Ferguson 2025). We can summarize these theoretical considerations in the following hypothesis:

Hypothesis 2. *States under Western or UN sanctions experience an increase in the government's compliance with the constitution.*

At the same time, we expect no such effects for non-Western sanctions.

With respect to the targets of sanctions, we distinguish sanctions targeting autocracies from those aimed at democracies. Although sanctions against democracies are less common, they are far from a rare occurrence (see Table E.4 in the Appendix). The mechanisms described above can be expected to be differently relevant depending on the regime type. In terms of the constitutional compliance enhancing effect of

43. Both are imperfect measurements of how and with what intention sanctions are designed, which is why each of them will be considered in the empirical analysis.

sanctions, democracies should be more susceptible to the economic and political pressure created by sanctions. As democratic governments have to stand for reelection, they face more pressure to adapt their policies in order to reduce the harm caused by sanctions to their broad political base. Nondemocratic governments have a narrower selectorate, which might either be more resilient to sanctions in the first place or which can be shielded more easily though active government measures. At the same time, autocratic government are more dependent on repression for their political survival and the stakes of losing political office are higher for incumbent autocrats. It can, therefore, be expected that autocracies lacking public resources due to sanctions are more likely to increase the use of repression to ensure the regime's survival. In addition, autocratic governments with control over the media and public discourse have better means to cover up their violations of constitutional rules for their citizens (Treisman and Guriev 2023). In sum, we can conclude that the constitutional compliance enhancing effects of sanctions are more powerful when democracies are targeted and the constitutional compliance undermining effects of sanctions are more relevant to sanctions against autocracies. In its most extreme form, this may lead to effects of sanctions that work in opposite directions:

Hypothesis 3a. *Democracies under international sanctions experience an increase in the government's compliance with the constitution.*

Hypothesis 3b. *Autocracies under international sanctions experience a decrease in the government's compliance with the constitution.*

6.3 Estimation Strategy and Data

6.3.1 Estimation Strategy

Identifying the causal effect of international sanctions on constitutional compliance requires constructing a counterfactual that is credible in a setting characterized by staggered treatment timing and potentially heterogeneous treatment effects. To address this challenge, our primary specification follows the Two-Stage Difference-in-

Differences (2SDiD) estimator proposed by Gardner et al. (2025) and implemented in Butts and Gardner (2022). To analyze dynamic effects, we complement the 2SDiD approach with a Local Projections Difference-in-Differences (LP-DiD) framework developed by Dube et al. (2025), who adapt the local projection method developed by Jordà (2005) to DiD settings.

Two-Stage DiD Estimator. The 2SDiD estimator is specifically designed for settings with staggered treatment adoption and heterogeneous treatment effects across cohorts and over time. Unlike traditional two-way fixed effects estimators, it avoids the negative weighting problem and yields consistent estimates of average treatment effects.

In the first stage, we partial out country and year fixed effects as well as lagged political and economic controls using only observations that are not yet treated. This is achieved by estimating:

$$y_{i,t} = \gamma_1 X_{i,t-1}^{pol} + \gamma_2 X_{i,t-1}^{econ} + \alpha_i + \tau_t + u_{i,t}. \tag{1}$$

The dependent variable $y_{i,t}$ measures the level of constitutional compliance in country i during year t. The specification includes country fixed effects (α_i) to account for time-invariant heterogeneity, year fixed effects (τ_t) to capture common global shocks and trends, and time-varying one-year lagged controls for political ($X_{i,t-1}^{pol}$) and economic ($X_{i,t-1}^{econ}$) conditions in the target country that may confound the relationship between sanction exposure and constitutional compliance.

From estimating Eq. (1) we obtain the residualized constitutional compliance score:

$$\tilde{y}_{i,t} = y_{i,t} - \widehat{\gamma_1} X_{i,t-1}^{pol} - \widehat{\gamma_2} X_{i,t-1}^{econ} - \widehat{\alpha_i} - \widehat{\tau_t} = \widehat{u}_{i,t}. \tag{2}$$

In the second stage, we regress the residualized outcome on the treatment indicators of interest. This procedure ensures that the identifying variation stems from appropriate comparisons between treated and not-yet-treated units and thus avoids the biases that can arise from two-way fixed effects estimation of staggered DiD models (Callaway

and Sant'Anna 2021; Chaisemartin and d'Haultfoeuille 2020):

$$\tilde{y}_{i,t} = \sum_{s=1}^{6} \beta_{sanc}^{s} D_{sanc,i,t-1}^{s} + \delta_1 X_{i,t-1}^{pol} + \delta_2 X_{i,t-1}^{econ} + \epsilon_{i,t} \tag{3}$$

Our key explanatory variables, $D_{sanc,i,t-1}^{s}$, are six binary indicators capturing the presence of sanctions, differentiated by both the identity of the sender (Western, UN, or non-Western) and the regime type of the target country (democracy or autocracy).[44] We define Western sanctions as non-UN sanctioned measures imposed by the US or the EU; non-Western sanctions are those non-UN sanctions imposed by China or Russia. Each indicator equals 1 if a sanction of the respective type is active against country i in year t, and 0 otherwise. To ensure that the treatment precedes the outcome in time, all sanction variables are lagged by one year.

Treatment timing is determined by a binary indicator capturing the first exposure to *any* sanction. The six sanction indicators then serve to estimate heterogeneous treatment effects. The controls are also included in the second stage to improve estimation efficiency. Standard errors are clustered at the country level to account for within-country dependence and heterogeneity in the error structure. For comparison, we additionally estimate a conventional two-way fixed effects DiD estimator.

Local Projections DiD Approach. To examine the dynamic effects of sanctions and assess the plausibility of the parallel trends assumption, we complement the 2SDiD estimates with a LP-DiD approach. Unlike conventional event study estimators, the LP-DiD framework directly estimates horizon-specific treatment effects and is robust to staggered treatment timing and treatment effect heterogeneity.

Due to the small number of observations for some sanction sender-target regime combinations, the estimation of horizon-specific treatment effects in the LP-DiD framework is not feasible at a disaggregated level. Hence, we apply LP-DiD only to two aggregated sanction categories. The first model evaluates the dynamic effects of

44. The six dummies identify: (i) Western sanctions against democracies, (ii) UN sanctions against democracies, (iii) non-Western sanctions against democracies, (iv) Western sanctions against autocracies, (v) UN sanctions against autocracies, and (vi) non-Western sanctions against autocracies.

sanctions imposed on democratic regimes, irrespective of the sender's identity, while the second model focuses on sanctions imposed on non-democratic regimes. In each model specification, treatment timing is defined by the first occurrence of sanctions against the respective regime type. For each horizon $h \in \{-3, -2, -1, 0, 1, \ldots, 10\}$, we estimate:

$$y_{i,t+h} = \beta_h D^s_{onset,i,t} + \sum_{s=1}^{3} \beta^s_{sanc} D^s_{sanc,i,t} + \theta_{1,h} X^{pol}_{i,t-1} + \theta_{2,h} X^{econ}_{i,t-1} + \alpha_i + \tau_t + \varepsilon_{i,t+h}, \qquad (4)$$

where $D^s_{onset,i,t}$ indicates the onset of sanctions. The coefficients β_h trace the dynamic response of constitutional compliance from three years prior to treatment to ten years after treatment. The year immediately preceding treatment ($h = -1$) serves as the reference period. To improve estimation efficiency when identifying the effect of sanctions on a specific target regime type, each specification also includes indicator variables for the other sanction categories ($D^s_{sanc,i,t}$): In the specification analyzing sanctions against democracies (autocracies), we control for (i) Western sanctions against autocracies (democracies), (ii) UN sanctions against autocracies (democracies), and (iii) non-Western sanctions against autocracies (democracies).

The estimated models allow for non-absorbing treatment, meaning that countries can exit treatment status and re-enter treatment after a period without sanctions. Observations that are sufficiently distant from previous sanction episodes (i.e., after at least three consecutive years without sanctions) are again considered part of the control group. This approach accounts for the possibility that sanction episodes may end and later resume. We rule out composition effects by fixing the set of control units across all event-time horizons. Moreover, we control for the same lagged political and economic covariates used in the 2SDiD model specification. Finally, we employ cluster-robust standard errors.

6.3.2 Data

Our sample comprises 8,751 country-year observations with complete data on constitutional compliance, sanctions, and control variables, including 2,106 country-years

in which countries were subject to sanctions (see Table E.4). It spans 182 countries and covers the period from 1962 to 2022. Table E.1 in the Appendix lists all countries included in the analysis. Western sanctions make up the largest share of the sanctioned country-years, accounting for 1,382 observations. UN and non-Western sanctions, for comparison, are observed in 376 and 432 country-years. Within these broader categories, some subcategories are notably rare. For example, only 59 observations involve UN sanctions against democracies, and 66 involve non-Western sanctions against autocracies. The dataset includes 116 sanction onsets against democratic targets and 105 onsets against autocratic targets (defined as the start of a sanction episode for a given target regime type, preceded by at least three consecutive non-sanctioned years). The data on sanctions is from the *Global Sanctions Data Base* (GSDB, Felbermayr et al. 2020; Kirilakha et al. 2021; Syropoulos et al. 2024), which offers comprehensive coverage of sanction episodes initiated by major actors, such as the US and the EU (classified as Western countries), Russia and China (classified as non-Western countries), and the UN.

Our dependent variable, constitutional compliance, is obtained from version 3.0 of the *Comparative Constitutional Compliance Database* (CCCD) developed by Gutmann, Metelska-Szaniawska, et al. (2024b). The database combines information on 18 types of (de jure) constitutional rules from the *Comparative Constitutions Project* with information on their de facto implementation according to the *V-Dem* dataset (Elkins et al. 2009; Pemstein et al. 2025; Coppedge et al. 2025). For each of the 18 types of rules, a compliance score of 1 is assigned if a rule is both constitutionally guaranteed (de jure) and respected in practice (de facto), 0 if it is guaranteed but violated (indicating a de jure-de facto gap), and 0.5 if it is not constitutionally guaranteed. The rules are aggregated in five legal domains: (i) government accountability, (ii) compliance with basic human rights, (iii) compliance with civil rights, (iv) compliance with political rights, and (v) compliance with property rights.[45] The overall constitutional com-

45. A correlation matrix reveals strong positive associations (between 0.61 and 0.81) among the compliance domains (ii)–(v), indicating that these four dimensions are closely related. By contrast, the government accountability dimension displays substantially lower correlations with all other measures (between 0.10 and 0.22), suggesting that it represents a more distinct dimension.

pliance index is then computed as the average of these five area-specific compliance indicators.

To account for potential time-variant, country-specific confounders that may influence constitutional compliance while being correlated with, but not caused by, the imposition of sanctions, we include a set of one-year lagged covariates. Basic socioeconomic conditions are captured through the natural logarithm of real GDP per capita and the natural logarithm of the population size (World Bank 2025). We also control for political and institutional characteristics and events. These include a binary democracy indicator from Bjørnskov and Rode (2020), which employs a minimalist definition of democracy in terms of the existence of competitive elections for political offices. Additionally, we incorporate binary variables for successful coups (Bjørnskov and Rode 2020) and for six different kinds of armed conflict (UCDP/PRIO Armed Conflict Dataset, see Davies et al. 2022; Gleditsch et al. 2002). These conflicts are disaggregated by intensity (minor conflicts with 25–999 battle-related deaths vs wars with 1,000+ deaths) and by type (interstate vs intrastate, with the latter further differentiated based on foreign military involvement).

Table E.2 provides the definitions and data sources of all variables, while Tables E.4 and E.5 present key descriptive statistics. Notably, countries targeted by sanctions tend to display lower constitutional compliance, lower income, a lower prevalence of democracy, a higher frequency of successful coups, and a greater exposure to every category of conflict. These patterns highlight the importance of addressing selection bias to fulfill the conditional parallel trends assumption underlying our identification strategy.

6.4 Empirical Results

6.4.1 Baseline Results

Effects of Sanctions over Time. We start with the LP-DiD estimates to illustrate the effects of sanctions over time, irrespective of the sender's identity.[46] Figure 6.1 shows the point estimates and corresponding 95% confidence intervals for the two sanction indicators, covering the three years prior to a sanctions episode (−3, −2, −1), treatment onset (0), and the ten years after treatment onset (1, 2, …, 10). The level of constitutional compliance in the year immediately preceding sanction imposition (−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 as well as pre-treatment trends.

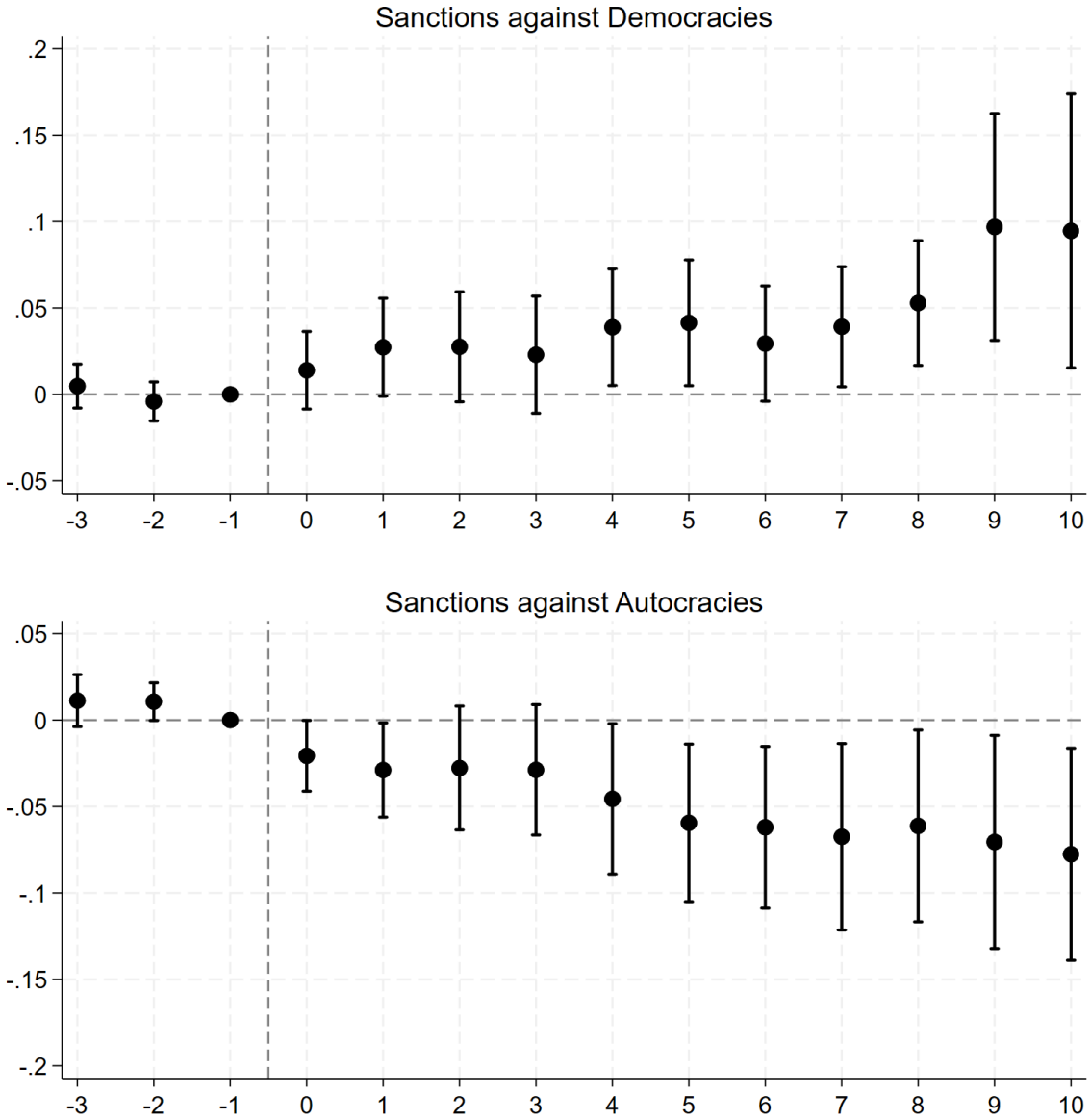
The estimates indicate no evidence of a significant pre-treatment trend for democracies, supporting the conditional parallel trends assumption and suggesting that treated and control units follow similar trajectories prior to sanction onset. At the time of treatment, the coefficient becomes positive and eventually statistically significant four years after the start of a sanction episode, indicating a gradual increase in constitutional compliance. The effect grows stronger over the subsequent years, with coefficients remaining consistently positive and statistically significant thereafter (with the exception of the sixth year after treatment). Estimated significant magnitudes range from approximately +0.04 to +0.10, pointing to sustained improvements in constitutional compliance following sanction imposition. The pooled post-treatment onset estimate corroborates this finding, indicating a statistically significant average increase of about +0.07.

The results for autocratic regimes also do not reveal evidence of statistically significant pre-treatment differences. At the same time, the post-treatment onset estimates display a clear and persistent negative pattern. The estimated effects range between approximately −0.02 and −0.08, indicating a gradual but sustained deterioration in

46. As explained in Section 6.3.1, differentiating sanctions in this event study design further by sender would require estimating parameters based on very few treated observations.

constitutional compliance. The post-treatment onset effects are statistically significant with the exception of the second and third year after the start of a sanction episode. The pooled post-treatment onset estimate corroborates this pattern, suggesting an average decrease of about −0.06. We discuss the substantive importance of these effects based on the estimated average treatment effects below.

Figure 6.1: Local Projection Difference-in-Differences



Notes: Coefficient estimates according to Eq. (4). Effects of sanctions over time (0, 1, 2, …, 10) alongside pre-trend (−3, −2, −1). Treatment is modeled as non-absorbing, allowing countries to exit and later re-enter sanction status (after at least three consecutive non-sanctioned years). Each sanction spell is treated as a separate event. Standard errors are clustered at the country level. Models include control variables (other sanction indicators, lag *log*(GDP pc), lag *log*(Population), lag Successful Coups, lag Democracy, and six lagged conflict indicators), country fixed effects, and year fixed effects. 95% confidence bands are indicated by whiskers.

Taken together, our results are consistent with Hypotheses 3a and 3b. Therefore, Hypotheses 1a and 1b do not describe the general patterns in the data and are each only partially corroborated.

Average Treatment Effects. As a next step, we analyze the average treatment effects over the duration of a sanctions episode. Put differently, we inquire which sanction senders drive the overall results against democracies and autocracies documented in Figure 6.1. Table 6.1 presents the results of our 2SDiD estimations.

Constitutional compliance improves specifically in democracies subject to UN sanctions (+0.07), equivalent to roughly 58% of a within-country standard deviation. Western sanctions have a statistically significant negative effect on constitutional compliance in autocracies, amounting to an average effect of −0.05 (about 41% of a within-country standard deviation). A slightly stronger effect is found for UN sanctions against autocracies (−0.07 or roughly 59% of a within-country standard deviation). Finally, we find no significant effects of non-Western sanctions or of Western sanctions targeting democracies.[47] Turning to the control variables, we find that the effects of sanctions are of similar magnitude to those of major interstate conflicts (−0.04) or major internal conflicts with foreign intervention (−0.08). This underscores the substantive importance of economic sanctions for constitutional compliance.[48]

47. However, the signs of these estimates are also consistent with that of the LP-DiD estimates.

48. The corresponding two-way fixed effects DiD results are reported in Table E.6 in the Appendix. The estimates are very similar to those in Table 6.1, although the point estimate and statistical significance for UN sanctions against democracies become somewhat smaller.

Table 6.1: Baseline Results – Two-Stage Difference-in-Differences

Dependent Variable: **Constitutional Compliance**	
Sanctions	
… lag Western against Democracies	0.0200
	(0.0200)
… lag UN against Democracies	0.0679***
	(0.0231)
… lag Non-Western against Democracies	0.0099
	(0.0138)
… lag Western against Autocracies	–0.0485**
	(0.0215)
… lag UN against Autocracies	–0.0693**
	(0.0297)
… lag Non-Western against Autocracies	–0.0478
	(0.0685)
lag *log*(GDP pc)	–0.0024**
	(0.0012)
lag *log*(Population)	0.0013**
	(0.0006)
lag Successful Coups	–0.0000
	(0.0089)
lag Democracy	0.0006
	(0.0025)
Interstate Conflicts	
… lag Minor	0.0216
	(0.0174)
… lag Major	–0.0445**
	(0.0205)
Internal Conflicts w/o Intervention	
… lag Minor	–0.0081
	(0.0078)
… lag Major	0.0100
	(0.0158)
Internal Conflicts w/ Intervention	
… lag Minor	0.0178
	(0.0209)
… lag Major	0.0808**
	(0.0347)
Observations	8,677
2nd Stage R^2 on Residualized Outcomes	0.0499
Within SD of Dependent Variable	0.1176

Notes: Coefficient estimates according to Eqs. (1)–(3). Standard errors in parentheses are clustered at the country level. Models include country fixed effects and year fixed effects. ***/**/* indicates significance at the 1%/5%/10% level.

The 2SDiD results confirm our findings from the LP-DiD approach that the more differentiated Hypotheses 3a and 3b are consistent with the data, whereas the more general predictions in Hypotheses 1a and 1b are refuted. Consistent with this picture, we also do not find support for Hypothesis 2, as the effects of Western and UN sanctions go in opposite directions depending on the target country's regime type. Only for UN sanctions against democracies do we find an improvement in constitutional compliance. This draws into question whether sanctions can be an effective instrument for forcing improvements in target countries' governance. Therefore, we extend our analysis in the next section to evaluate if sanctions aimed at improving democracy or human rights achieve better outcomes.

6.4.2 Extensions and Robustness Tests

Effect Heterogeneity Across Sanctions Objectives. The GSDB identifies nine types of justifications for imposing sanctions provided in official documentation: democracy promotion, human rights protection, regime destabilization, policy change, war prevention, war termination, territorial conflict, counter-terrorism, and others. A priori, one would expect sanctions explicitly aimed at democratization or human rights improvements to have a more positive effect on constitutional compliance than those pursuing other objectives (Steinbach et al. 2023, however, provides some cause for pessimism). To examine this heterogeneity in effects without losing too much statistical power, we disaggregate sanctions into two categories: (i) sanctions with the stated objective of promoting democracy or human rights and (ii) sanctions pursuing only other goals. Observations with sanctions that mix the objectives in the two categories are excluded from the analysis to allow for a clean comparison. For this analysis, we re-run the baseline estimations while dropping sanction cases in one of the two categories at a time.

The results of this jackknife-style extension are presented in Table 6.2. We find clear differences in sanction effects depending on their objectives for only one of the sanction indicators. Western sanctions against autocracies show a decline in constitu-

tional compliance when they are explicitly aimed only at democratization or human rights objectives, but not when only other objectives are stated. UN sanctions against democracies lead to an improvement in constitutional compliance for both types of stated objectives, with the effect being marginally larger for sanctions that are explicitly aimed at democratization or human rights improvements. In contrast, the effects of UN sanctions against autocracies are negative and of similar magnitude for both types of objectives. Overall, our findings do not support that sanctions intended to improve democracy or human rights perform better in their effects on the target country's constitutional compliance, which is consistent with the findings of Steinbach et al. (2023) for the human rights effects of sanctions.

Table 6.2: Effect Heterogeneity for Sanction Objectives

Dependent Variable:	**Objectives**	
Constitutional Compliance	**Democrat. & HR**	**Other**
Sanctions		
… lag Western against Democracies	–0.0057	0.0259
	(0.0334)	(0.0231)
… lag UN against Democracies	0.0921***	0.0711***
	(0.0266)	(0.0193)
… lag Non-Western against Democracies	0.0068	0.0056
	(0.0102)	(0.0135)
… lag Western against Autocracies	–0.0851***	–0.0080
	(0.0146)	(0.0252)
… lag UN against Autocracies	–0.0945**	–0.1003***
	(0.0377)	(0.0265)
… lag Non-Western against Autocracies	–0.0676	0.0108
	(0.1318)	(0.0160)
Observations	7,058	7,835
2nd Stage R^2 on Residualized Outcomes	0.0419	0.0313

Notes: Selected coefficient estimates according to Eqs. (1)–(3). The LHS variable represents the constitutional compliance in target country i in year t. 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 dropped, and vice versa. Models include control variables (lag *log*(GDP pc), lag *log*(Population), lag Successful Coups, lag Democracy, and six lagged conflict indicators), country fixed effects, and year fixed effects. Estimates are available on request.

Effect Heterogeneity in Different Legal Dimensions. Given that sanctions have a significant effect on constitutional compliance, it seems important to understand whether it is compliance with particular parts of the constitution that changes. Gutmann, Neuenkirch, and Neumeier (2020), for example, find that the effects of sanctions on different human rights dimensions can not only differ, but they may even work in opposite directions. The CCCD offers five subindices of constitutional compliance: (i) compliance with rules for government accountability, (ii) basic human rights compliance, (iii) civil rights compliance, (iv) political rights compliance, and (v) property rights compliance. We re-run the baseline estimations, replacing the dependent variable with the five subindices, one at a time.

The results of this extension are reported in Table E.7 in the Appendix.[49] First of all, all statistically significant coefficient estimates have the same sign as those for overall constitutional compliance. UN sanctions against democracies are linked to substantial improvements in compliance with basic human rights (+0.21) and political rights (+0.13) in the constitution, while no systematic effects emerge for the other three subindices. Western sanctions against autocracies are associated with declines in four of the five subindices, with the largest reductions observed for political rights and property rights (−0.07, respectively), followed by civil rights (−0.06) and rules for government accountability (−0.03). Only compliance with basic human rights, such as the right to life, freedom from torture, etc., does not decline significantly. UN sanctions against autocracies are associated with marked declines in compliance with property rights (−0.13) and constitutional rules for government accountability (−0.05), with no effects on the other three subindices. Overall, these patterns suggest that the positive effects of UN sanctions on democracies are concentrated in a few domains. The same holds for the negative effects of UN sanctions against autocracies. The effects of Western sanctions against autocracies are weaker but apply to many parts of the constitution.

49. The variation in the number of observations across the models reflects differences in data availability for the five subindices. Restricting all estimations to a common sample leaves the results virtually unchanged.

6.5 Conclusion

The effects of sanctions on the governance of societies are frequently evaluated by universal standards, such as internationally recognized human rights. This approach, however, invites criticism, as human rights have originated in Western nations and many governments refuse to be evaluated by this benchmark. Our study takes these concerns seriously and offers an alternative evaluation. We ask whether governments comply in practice with the commitments made in their national constitution. In other words, governments are judged by their social contract instead of some universal—but not necessarily universally agreed upon—standard.

Our evaluation of the causal effects of international sanctions on constitutional compliance brings to light a differentiated picture. We find no general increase or decrease in compliance levels among states under sanctions. Even if we focus on Western sanctions or sanctions imposed with the declared goal of promoting human rights or democracy, we do not find a uniform effect on constitutional compliance. That is because the response by governments to sanctions depends on their regime type. In line with our theoretical expectation, we find that democracies are prone to improving constitutional compliance under sanctions. These governments are more responsive to the economic pressure on their general population, their political survival depends less on repression, and their limited control over public media makes it more difficult for them to get away with violating the constitution. Consistent with our theoretical predictions, we find exactly the opposite for nondemocratic target governments. Their compliance with the constitution decreases while they are subject to international sanctions. These effects are driven by Western and UN sanctions, whereas sanctions by China and Russia do not affect constitutional compliance at all. We find that democracies under UN sanctions improve particularly their compliance with basic human rights and political rights, whereas sanctioned autocracies show declining compliance across almost every legal dimension.

For policymakers, this means that sanctions are not a suitable tool for making governments comply with their legal commitments where it would be needed the most.

Sanctioned autocracies, such as Cuba, Iran, or North Korea, should not be expected to comply more because international sanctions are imposed on them. At the same time, the often overlooked cases of sanctions against democracies show that improvements in constitutional compliance are possible where governments are more dependent on broad support from their population and where violations of the constitution are difficult to cover up.

Our findings also suggest some interesting questions for future research: How do national media report potential violations of the constitution, while the state is under foreign pressure due to sanctions? Do rally-around-the-flag effects allow governments to get away with violating the constitution? In other words, do citizens exhibit less constitutional loyalty during these periods (see Gutmann, Sarel, et al. 2025)? And are violations of constitutional rules under sanctions excused—by politicians or citizens—as being justified by the use of emergency powers, both if the country has a formal emergency constitution and if it does not (see Bjørnskov and Voigt 2024)?

Appendix E

Appendix E1: Data Description

Table E.1: List of Countries

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

Afghanistan (22; 22; 0; 20), Albania (42; 11; 0; 11), Algeria (60; 11; 0; 0), Angola (42; 20; 0; 9), Antigua and Barbuda (42; 3; 0; 3), Argentina (48; 11; 7; 4), Armenia (28), Australia (61; 9; 0; 9), Austria (61; 10; 0; 10), Azerbaijan (32; 11; 0; 11), Bahamas (50), Bahrain (50), Bangladesh (51; 2; 0; 0), Barbados (57), Belarus (29; 23; 19; 2), Belgium (61; 9; 0; 9), Belize (42; 20; 0; 15), Benin (59; 12; 0; 12), Bhutan (52), Bolivia (58; 17; 4; 12), Bosnia and Herzegovina (28; 12; 0; 11), Botswana (57), Brazil (61; 9; 0; 9), Brunei Darussalam (48), Bulgaria (42; 13; 2; 11), Burkina Faso (42; 4; 0; 4), Burundi (56; 10; 0; 1), Cambodia (47; 34; 11; 17), Cameroon (61; 11; 9; 0), Canada (61; 20; 0; 20), Cape Verde (42), Central African Republic (51; 18; 8; 0), Chad (43; 2; 0; 2), Chile (61; 22; 6; 3), China (61; 52; 26; 18), Colombia (61; 28; 0; 28), Comoros (38), Costa Rica (61; 22; 0; 22), Croatia (32; 15; 0; 15), Cuba (52; 52; NA; 52), Cyprus (47; 36; 0; 36), Czech Republic (32; 10; 0; 10), Denmark (61; 9; 0; 9), Djibouti (9), Dominica (45; 2; 0; 2), Dominican Republic (61; 13; 0; 13), DR Congo (55; 29; 3; 5), Ecuador (54; 9; 0; 9), Egypt (61; 24; 1; 14), El Salvador (40; 8; 0; 5), Equatorial Guinea (41; 9; 9; 0), Eritrea (15; 8; 0; 8), Estonia (32; 16; 0; 16), Ethiopia (48; 12; 0; 5), Fiji (53; 22; 21; 0), Finland (61; 9; 0; 9), France (61; 32; 0; 32), Gabon (61), Gambia (53; 18; 12; 0), Georgia (31; 8; 0; 8), Germany (61; 10; 0; 10), Ghana (61; 5; 0; 5), Greece (60; 16; 6; 10), Guatemala (61; 38; 26; 10), Guinea (52; 20; 14; 0), Guinea-Bissau (50; 13; 13; 0), Guyana (57; 3; 3; 0), Haiti (59; 38; 8; 1), Honduras (61; 6; 1; 3), Hungary (61; 39; 0; 27), Iceland (61; 8; 0; 8), India (61; 44; 0; 44), Indonesia (61; 34; 18; 13), Iran (61; 42; 0; 8), Iraq (61; 43; 0; 39), Ireland (61; 25; 0; 25), Israel (61; 22; 0; 22), Italy (61; 9; 0; 9), Jamaica (56; 12; 0; 9), Japan (61), Jordan (46; 4; 0; 4), Kazakhstan (30; 2; 0; 2), Kenya (60; 13; 4; 9), Kiribati (44), Kuwait (40; 2; 0; 2), Kyrgyzstan (27; 1; 0; 1), Laos (38; 16; 0; 16), Latvia (32; 16; 7; 9), Lebanon (34; 27; 0; 11), Lesotho (57; 7; 0; 7), Liberia (61; 28; 0; 15), Libya (61; 45; 0; 33), Lithuania (31; 11; 0; 11), Luxembourg (61; 9; 0; 9), Madagascar (61; 9; 0; 0), Malawi (59; 5; 2; 3), Malaysia (61), Maldives (52), Mali (55; 8; 3; 0), Malta (52; 9; 0; 9), Marshall Islands (44), Mauritania (56; 4; 3; 0), Mauritius (55), Mexico (61; 1; 0; 1), Micronesia (45), Moldova (32; 20; 0; 20), Monaco (52), Mongolia (39), Montenegro (17; 8; 0; 8), Morocco (56), Mozambique (42; 2; 0; 2), Myanmar (61; 40; 35; 0), Namibia (33), Nauru (52), Nepal (61; 1; 1; 0), Netherlands (61; 9; 0; 9), New Zealand (61; 1; 0; 1), Nicaragua (61; 22; 0; 17), Niger (60; 7; 5; 0), Nigeria (52; 25; 0; 14), North Macedonia (32; 1; 0; 1), Norway (61; 13; 0; 5), Oman (25), Pakistan (40; 17; 2; 15), Palau (42), Panama (61; 16; 1; 12), Papua New Guinea (48), Paraguay (61; 7; 5; 0), Peru (61; 15; 3; 11), Philippines (60; 21; 0; 20), Poland (32; 10; 0; 10), Portugal (61; 18; 0; 18), Qatar (20), Romania (32; 15; 6; 9), Russia (33; 10; 0; 5), Rwanda (53; 11; 0; 2), Samoa (52), Sao Tome and Principe (48), Saudi Arabia (31; 5; 5; 0), Senegal (61), Serbia (27; 19; 8; 2), Seychelles (44), Sierra Leone (53; 20; 0; 18), Singapore (60), Slovakia (31; 9; 0; 9), Slovenia (32; 9; 0; 9), Solomon Islands (42), Somalia (50; 22; 3; 0), South Africa (61; 40; 0; 31), South Korea (61; 5; 5; 0), Spain (45; 9; 0; 9), Sri Lanka (51; 5; 3; 2), St. Lucia (45), Sudan (52; 34; 3; 0), Suriname (42; 2; 2; 0), Swaziland (18), Sweden (49; 9; 0; 9), Switzerland (24), Syria (44; 17; 0; 16), Tajikistan (29), Tanzania (46; 8; 4; 2), Thailand (29; 6; 6; 0), Timor Leste (21), Togo (52; 16; 16; 0), Trinidad and Tobago (61), Tunisia (49), Turkey (61; 16; 6; 9), Turkmenistan (16; 4; 0; 3), Tuvalu (37), Uganda (28; 2; 1; 0), Ukraine (35; 15; 0; 14), United Arab Emirates (52; 1; 0; 1), United Kingdom (61; 9; 0; 9), Uruguay (38), USA (61; 9; 0; 9), Uzbekistan (31; 11; 10; 1), Vanuatu (43), Vietnam (38; 27; 0; 17), Yemen (32; 12; 0; 3), Zambia (59; 3; 3; 0), Zimbabwe (58; 40; 35; 5).

Notes: If only one number is reported in parentheses, the respective country was never subject to sanctions in the dataset. If the objective of sanctions pursued both democratization or human rights improvements as well as other objectives (i.e., destabilizing the regime, policy change, preventing war, ending war, territorial conflict, counterterrorism, or other objectives), the corresponding observation was removed from both datasets to allow for a clear separation of effects. Therefore, the number of "democratization or human rights sanctions" and "non-democratization and non-human rights sanctions" does not necessarily add up to the number of "total sanctions."

Table E.2: Definitions of Variables and Data Sources

Variable	Definition & Source
Constitutional Compliance	Measures constitutional compliance using 18 rule indicators from the Constitutional Compliance Project (de jure) and V-Dem (de facto). Compliance is coded 1 if a rule is both constitutionally guaranteed (de jure) and respected in practice (de facto), 0 if it is guaranteed but violated, and 0.5 if it is not constitutionally guaranteed. The rules are aggregated into five legal domains (Government Accountability, Basic Human Rights, Civil Rights, Political Rights, Property Rights and the Rule of Law) and an overall index is computed by taking the mean value of the five area-specific compliance indicators. *Source:* The Comparative Constitutional Compliance Database (Gutmann, Metelska-Szaniawska, et al. 2024b; Elkins et al. 2009; Pemstein et al. 2025; Coppedge et al. 2025).
Government Accountability Compliance	Measures compliance with government accountability rules on a scale from 0 to 1, covering three constitutional rules concerning: elections of head of state and head of government, and meritocratic organization of the civil service. *Source:* CCCD (Gutmann, Metelska-Szaniawska, et al. 2024b; Elkins et al. 2009; Pemstein et al. 2025; Coppedge et al. 2025).
Basic Human Rights Compliance	Measures compliance with basic human rights on a scale from 0 to 1, covering three constitutional rules concerning: right to life, freedom of slavery, and protection from torture. *Source:* CCCD (Gutmann, Metelska-Szaniawska, et al. 2024b; Elkins et al. 2009; Pemstein et al. 2025; Coppedge et al. 2025).
Civil Rights Compliance	Measures compliance with civil rights on a scale from 0 to 1, covering four constitutional rules concerning: academic freedom, free media, free speech, free movement, and religious freedom. *Source:* CCCD (Gutmann, Metelska-Szaniawska, et al. 2024b; Elkins et al. 2009; Pemstein et al. 2025; Coppedge et al. 2025).
Political Rights Compliance	Measures compliance with political rights on a scale from 0 to 1, covering three constitutional rules concerning: freedom of association, freedom of assembly, and the right to form parties. *Source:* CCCD (Gutmann, Metelska-Szaniawska, et al. 2024b; Elkins et al. 2009; Pemstein et al. 2025; Coppedge et al. 2025).
Property Rights Compliance	Measures compliance with political rights on a scale from 0 to 1, covering four constitutional rules concerning: private property rights, judicial independence, equality before the law, and rule of law. *Source:* CCCD (Gutmann, Metelska-Szaniawska, et al. 2024b; Elkins et al. 2009; Pemstein et al. 2025; Coppedge et al. 2025).

Table E.3: Definitions of Variables and Data Sources (continued)

lag Sanctions	Binary indicators for country-years with sanctions in place, lagged by one year. *Source:* GSDB (Felbermayr et al. 2020; Kirilakha et al. 2021; Syropoulos et al. 2024).
lag *log*(GDP pc)	Natural logarithm of real GDP per capita in 2015 USD, lagged by one year. *Source:* World Development Indicators (World Bank 2025).
lag *log*(Population)	Natural logarithm of the population size, lagged by one year. *Source:* See previous variable.
lag Successful Coups	Binary indicator for country-years in which successful coups have occurred, lagged by one year. *Source:* Bjørnskov and Rode (2020).
lag Democracy	Binary variable indicating whether a country is democratic or not, lagged by one year. *Source:* See previous variable.
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. *Source:* UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002; Davies et al. 2022).
lag Interstate Conflict	Conflicts between two states, lagged by one year. *Source:* See previous variable.
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. *Source:* See previous variable.

Table E.4: Frequency of Sanctions

Panel A: 2SDiD Estimations			
Sanctions in General			
… No Sanctions	6,645		
… Sanctions	2,106 (414; 1,263)		
Sanction Senders			
… Western	1,382 (374; 774)		
… UN	376 (28; 162)		
… Non-Western	432 (17; 391)		
Sanctions against Democracies		Sanctions against Autocracies	
… Western	563 (93; 430)	… Western	819 (281; 344)
… UN	59 (11; 29)	… UN	317 (17; 133)
… Non-Western	366 (7; 349)	… Non-Western	66 (10; 42)
Panel B: LP-DiD Approach			
Sanctions against Democracies		Sanctions against Autocracies	
… Onsets	116	… Onsets	105

Notes: Frequency of observations of the different sanction indicators for which all control variables (see Table E.5) are available. Total number of observations in the dataset: 8,751. Sanctions enacted by the UNSC are not counted as Western or Non-Western sanctions. However, Western and Non-Western sanctions can coincide. Numbers in parentheses represent the number of sanctions with the objective democratization or human rights improvements and the number of sanctions with alternative objectives (i.e., destabilizing the regime, policy change, preventing war, ending war, territorial conflict, counter-errorism, or other objectives). If the objective of sanctions pursued both democratization or human rights improvements as well as other objectives, the corresponding observation was removed from both datasets to allow for a clear separation of effects. Therefore, the number of "democratization or human rights sanctions" and "non-democratization and non-human rights sanctions" does not necessarily add up to the number of "total sanctions." Onsets refer to the start of sanction episodes of a given target type for which at least the previous three years were untreated.

Table E.5: Descriptive Statistics

	All Observations		**No Sanctions**		**Sanctions**	
	Mean	SD	Mean	SD	Mean	SD
Constitutional Compliance	0.54	0.22	0.56	0.21	0.50	0.24
lag log(GDP pc)	8.28	1.48	8.33	1.48	8.12	1.45
lag log(Population)	15.50	2.10	15.20	2.13	16.47	1.66
	$X = 1$		$X = 1$		$X = 1$	
lag Democracy	53.1%		55.8%		44.5%	
lag Successful Coups	1.8%		1.5%		2.5%	
lag Minor Conflict	11.7%		8.7%		21.2%	
… Interstate	0.8%		0.5%		1.5%	
… Internal w/ Intervention	1.5%		0.8%		3.6%	
… Internal w/o Intervention	9.5%		7.3%		16.2%	
lag Major Conflict	3.3%		2.0%		7.6%	
… Interstate	0.4%		0.3%		0.9%	
… Internal w/ Intervention	0.9%		0.3%		2.8%	
… Internal w/o Intervention	2.0%		1.4%		3.9%	

Notes: Mean values, standard deviations (SD), and share of non-zero observations ($X = 1$) for all LHS variables and control variables. The full dataset comprises 8,751 observations, of which 6,645 are without sanctions in place and 2,106 are with sanctions in place.

Appendix E2: Additional Results

Table E.6: Baseline Results – Two-way Fixed Effects DiD

Dependent Variable: **Constitutional Compliance**	
Sanctions	
… lag Western against Democracies	0.0178
	(0.0150)
… lag UN against Democracies	0.0405*
	(0.0231)
… lag Non-Western against Democracies	–0.0013
	(0.0126)
… lag Western against Autocracies	–0.0565***
	(0.0166)
… lag UN against Autocracies	–0.0736***
	(0.0237)
… lag Non-Western against Autocracies	–0.0323
	(0.0617)
Observations	8,751
R^2	0.8393
Within-R^2	0.2933

Notes: Coefficient estimates of conventional two-way fixed effects DiD estimation. Standard errors in parentheses are clustered at the country level. Model includes control variables (lag *log*(GDP pc), lag *log*(Population), lag Successful Coups, lag Democracy, and six lagged conflict indicators), country fixed effects, and year fixed effects. Estimates are available on request. ***/**/* indicates significance at the 1%/5%/10% level.

Table E.7: Effect Heterogeneity for Subindices of Constitutional Compliance

Dependent Variable:	**Government Accountability**	**Basic HR Compliance**	**Civil Rights Compliance**	**Political Rights Compliance**	**Property Rights Compliance**
lag Sanctions					
… Western ag. Democracies	0.0104	0.0263	0.0206	0.0373	0.0061
	(0.0144)	(0.0274)	(0.0248)	(0.0427)	(0.0222)
… UN against Democracies	0.0254	0.2108***	0.0246	0.1325*	–0.0204
	(0.0307)	(0.0522)	(0.0382)	(0.0718)	(0.0556)
… Non-Western ag. Democ.	0.0114	–0.0049	–0.0032	0.0134	0.0110
	(0.0141)	(0.0218)	(0.0175)	(0.0262)	(0.0261)
… Western ag. Autocracies	–0.0295**	–0.0068	–0.0613**	–0.0743*	–0.0658***
	(0.0144)	(0.0276)	(0.0302)	(0.0384)	(0.0235)
… UN against Autocracies	–0.0532*	–0.0869	–0.0308	–0.0687	–0.1315***
	(0.0297)	(0.0539)	(0.0328)	(0.0644)	(0.0451)
… Non-Western ag. Autoc.	–0.0633	–0.0512	–0.0688	0.0134	–0.0461
	(0.0885)	(0.0767)	(0.0763)	(0.0538)	(0.0630)
Observations	8,677	7,989	8,614	7,989	7,989
2nd Stage R^2 on Res. Out.	0.0203	0.0308	0.0315	0.0342	0.0507

Notes: Coefficient estimates according to Eqs. (1)–(3). Standard errors in parentheses are clustered at the country level. The variation in the number of observations reflects differences in data availability for the five subindices. Models include control variables (lag *log*(GDP pc), lag *log*(Population), lag Successful Coups, lag Democracy, and six lagged conflict indicators), country fixed effects, and year fixed effects. Estimates are available on request. ***/**/* indicates significance at the 1%/5%/10% level.

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List of Acronyms

Acronym	Meaning
2SDiD	Two-Stage Difference-in Differences estimator
AME	Average Marginal Effects
CC	Constitutional Compliance
CCCD	Comparative Constitutional Compliance Database
CIA	Central Intelligence Agency
CoC	Control of Corruption indicator
CPI	Corruption Perception Index
DEMIG	Determinants of International Migration dataset
DiD	Difference-in-Differences estimator
EU	European Union
FE	Fixed Effect
GDP	Gross Domestic Product
GMM	Generalized Method of Moments estimator
GSDB	Global Sanctions Data Base
HR	Human Rights
IGO	Intergovernmental Organization
KGB	Committee for State Security (Komitet gossudarstwennoi besopasnosti)
KOFGI	KOF Globalisation Index
LDV	Lagged Dependent Variable
LEAD	Leader Experience and Attribute Descriptions dataset
log	(natural) logarithm
LP-DiD	Local Projections Difference-in-Differences estimator
OECD	Organisation for Economic Co-Operation and Development
OLS	Ordinary Least Squares estimation
pc	per capita
pp	percentage point(s)
PRIO	Peace Research Institute Oslo

Acronym	Meaning
SD *or* Std. Dev.	Standard Deviation
SE	Standard Errors
SMD	Standardized Mean Difference
UCDP	Uppsala Conflict Data Program
UK	United Kingdom
UN	United Nations
UNSC	United Nations Security Council
US	United States of America
USD	US-Dollar
V-Dem	Varieties of Democracy dataset
w/	with
w/o	without
WGI	World Governancy Indicators (provided by the World Bank)

Deutsche Zusammenfassung (German Summary)

Die vorliegende Arbeit "The Political Economy of International Sanctions" ist eine kumulativ verfasste Dissertation, bestehend aus einer einleitenden Übersicht und fünf einzelnen Forschungsartikeln zu den Einflussfaktoren und Auswirkungen internationaler Sanktionen.

Sanktionen sind gezielte wirtschaftliche oder politische Zwangsmaßnahmen, mit denen Staaten oder internationale Organisationen das Verhalten eines anderen Staates, einer Organisation, von Unternehmen oder relevanten Einzelakteuren beeinflussen wollen. Die konkrete Ausgestaltung der Sanktionen kann dabei unterschiedlich ausfallen. Sie reicht von Einreise- und Finanzmarktbeschränkungen gegen Teile oder die gesamte Bevölkerung des sanktionierten Staates, über das explizite Verbot jeglicher militärischer Kooperationen, einschließlich Waffenexporten, bis hin zu vollständigen Handelsrestriktionen. Die Ursachen und Ziele von Sanktionen sind meist Konfliktprävention oder -beilegung, ein Regime- oder Politikwechsel, Demokratisierung oder die Reaktion auf Menschenrechtsverletzungen. In der Praxis sind sowohl die Ursachen als auch die Konsequenzen von Sanktionen komplexer als öffentliche Diskussionen vermuten lassen und gehen oft mit unbeabsichtigten oder bewusst ignorierten Nebeneffekten einher, weshalb eine systematische Analyse der Motivationen, Ziele und Auswirkungen internationaler Sanktionen von höchster Notwendigkeit ist – sowohl für die sozialwissenschaftliche Forschung als auch für die praktische Politikgestaltung.

Sanktionen sind eines der gängigsten nichtmilitärischen Instrumente der Außenpolitik, deren Nutzung seit dem Ende des Zweiten Weltkrieges bis heute ein unvergleichliches Ausmaß erreicht hat. Insbesondere seit dem Ende des Kalten Krieges ist die Anzahl der verhängten Sanktionen dramatisch angestiegen.

Das erste Forschungspapier analysiert den Effekt internationaler Sanktionen auf globale Migrationsbewegungen, insbesondere die Emigration aus den sanktionierten Staaten (Kapitel 2). Im zweiten Artikel wird ein nuanciertes Verständnis der Beziehung zwischen Sanktionen und dem Zusammenbruch des sanktionierten Regimes entwickelt, mit einem besonderen Fokus auf Staatsstreiche bzw. Coups

d'État (Kapitel 3). Die darauffolgende Publikation untersucht die Rolle persönlicher Beziehungen zwischen politischen Führern mittels biographischer Gemeinsamkeiten im Kontext von Sanktionen (Kapitel 4). Die beiden letzten Kapitel erforschen den Einfluss von Sanktionen auf staatliche Vertreter und Institutionen, insbesondere auf die Korruption (Kapitel 5) und die Einhaltung der eigenen nationalen Verfassung (Kapitel 6).

1. Internationale Sanktionen und Emigration (Kapitel 2)

Das erste Forschungspapier "International Sanctions and Emigration" untersucht erstmals systematisch den Zusammenhang zwischen internationalen Sanktionen und internationalen Migrationsbewegungen. Während die bisherige Forschung vor allem die ökonomischen und politischen Auswirkungen von Sanktionen auf Wachstum, Handel, Armut, Regimestabilität oder Menschenrechte analysiert hat, blieb die Reaktion der Bevölkerung weitgehend unbeachtet. Das Paper schließt diese Forschungslücke, indem es fragt, ob Sanktionen Menschen dazu veranlassen, ihr Herkunftsland zu verlassen und auszuwandern.

Ausgangspunkt der Analyse ist Albert O. Hirschmans Konzept von Exit, Voice and Loyalty. Hirschman (1970) argumentiert, dass Menschen auf politische oder wirtschaftliche Missstände grundsätzlich auf zwei Arten reagieren können: durch *Voice*, also Protest, politische Opposition und öffentliche Kritik, oder durch *Exit*, also Rückzug aus dem bestehenden System. Im staatlichen Kontext bedeutet Exit häufig Emigration. Wir übertragen dieses Konzept auf Sanktionsregime. Wenn Sanktionen wirtschaftliche Not, politische Repression oder Perspektivlosigkeit erzeugen, könnte Auswanderung eine rationale Reaktion der betroffenen Bevölkerung darstellen.

Demgegenüber existiert auch die gegenteilige theoretische Möglichkeit, dass Sanktionen Migration erschweren, indem sie Einkommen reduzieren, Reisebeschränkungen verschärfen oder Hoffnungen auf politische Verbesserungen im Land selbst stärken. Entsprechend formulieren wir konkurrierende Hypothesen: (1a) Sanktionen erhöhen Emigration oder (1b) Sanktionen verhindern Emigration. Zusätzlich prüfen

wir, (2) ob Frauen stärker betroffen sind als Männer und (3) ob der Migrationseffekt in Staaten mit eingeschränkter Meinungsfreiheit größer ausfällt.

Die empirische Analyse basiert auf einem umfangreichen Datensatz mit 79.791 Länderpaar-Jahr-Beobachtungen. Erfasst werden bilaterale Migrationsströme von 157 Herkunftsländern in 32 Industriestaaten im Zeitraum von 1961 bis 2018. Die Migrationsdaten stammen aus den Datenbanken der *OECD* und des *Determinants of International Migration (DEMIG)* Projekts (DEMIG 2015; OECD 2020). Informationen zu Sanktionen werden der Global Sanctions Data Base (GSDB) entnommen (Felbermayr et al. 2020; Kirilakha et al. 2021; Syropoulos et al. 2024). Wir unterscheiden zwischen fünf Sanktionsarten: (i) Sanktionen der Vereinten Nationen (UN), (ii) gemeinsame Sanktionen von der EU und den USA, (iii) einseitige EU-Sanktionen, (iv) einseitige US-Sanktionen und (v) nicht-westliche Sanktionen von Russland und/oder China.

Methodisch verwenden wir zwei komplementäre Ansätze. Erstens ein Difference-in-Differences (DiD)-Modell, das die durchschnittlichen Effekte von Sanktionen auf Migration misst. Zweitens eine Event-Study-Analyse, mit der sich untersuchen lässt, wie sich Migration vor, während und nach Sanktionen entwickelt. Dadurch kann besser zwischen echten Sanktionseffekten und bereits vorher bestehenden Trends unterschieden werden. Kontrolliert wird unter anderem für das Bruttoinlandsprodukt (BIP) pro Kopf, den Demokratiegrad, Menschenrechte, Bewegungs- bzw. Reisefreiheit sowie bewaffnete Konflikte.

Die wichtigste Erkenntnis lautet, dass UN-Sanktionen sowie gemeinsame bzw. multilaterale EU-US-Sanktionen die Emigration signifikant erhöhen. Unter UN-Sanktionen steigen Migrationsströme aus dem Zielland durchschnittlich um etwa 17 bis 18 Prozent. Bei gemeinsamen Sanktionen von EU und USA beträgt der Anstieg sogar 22 bis 24 Prozent. Diese Effekte sind statistisch robust. Demgegenüber zeigen einseitige bzw. unilaterale Sanktionen der EU oder der USA keinen systematisch signifikanten Einfluss auf die Migration. Auch nicht-westliche Sanktionen weisen nur begrenzte und inkonsistente Effekte auf. Daraus lässt sich schließen, dass vor allem koordinierte, multilaterale Sanktionen das Auswanderungsverhalten beeinflussen.

Besonders aufschlussreich sind die Ergebnisse der Event-Study-Analyse. Bei multilateralen EU-US-Sanktionen steigt die Emigration nicht sofort sprunghaft an, sondern nimmt im Verlauf eines Sanktionsregimes schrittweise zu. In lang andauernden Sanktionsphasen erreicht der Effekt 80 bis 86 Prozent relativ zum letzten Jahr vor Einführung der Sanktionen. Migrationsentscheidungen werden also unter langanhaltendem Sanktionsdruck getroffen. UN-Sanktionen zeigen ebenfalls positive Effekte, diese sind jedoch schwächer und weniger dauerhaft. Der Höchstwert liegt hier bei rund 30 bis 31 Prozent. Nach dem Ende der Sanktionen kehren die Migrationsströme weitgehend auf ihr ursprüngliches Niveau zurück. Dies spricht dafür, dass tatsächlich die Sanktionen selbst den Effekt verursachen.

Entgegen der ursprünglichen Erwartung finden wir keine systematischen Unterschiede zwischen Männern und Frauen. Zwar deuten einzelne Schätzungen auf leicht stärkere Effekte für Frauen hin, diese Unterschiede sind statistisch jedoch nicht belastbar. Damit widerspricht das Ergebnis teilweise früheren Studien, die Frauen als besonders stark von Sanktionen betroffen identifizierten (Demir and Tabrizy 2022; Gutmann et al. 2021). Offenbar führt eine stärkere ökonomische Belastung nicht automatisch zu geschlechtsspezifisch unterschiedlichen Migrationsreaktionen.

Ein besonders wichtiger Befund betrifft die Rolle politischer Freiheitsrechte. Der positive Migrationseffekt ist vor allem in Ländern stark ausgeprägt, in denen die Freiheit politischer Meinungsäußerung eingeschränkt ist. Dort fungiert Emigration offenbar als Ersatz für Protest. Wenn Menschen ihre Unzufriedenheit nicht öffentlich äußern können, wählen sie eher den Weg des Exit. In Staaten mit höherer politischer Offenheit fällt der Effekt deutlich schwächer aus oder verschwindet. Dies bestätigt die Hirschman-Theorie: Wo Voice teuer oder gefährlich ist, gewinnt Exit an Attraktivität. Sanktionen können somit unbeabsichtigt gerade jene Bevölkerungsgruppen zur Auswanderung bewegen, die dem Regime kritisch gegenüberstehen.

Die Ergebnisse haben erhebliche außenpolitische Relevanz. Sanktionen gelten als nichtmilitärisches Druckmittel, um Staaten zu Verhaltensänderungen zu bewegen. Die Studie zeigt jedoch, dass sie auch unbeabsichtigte Nebenwirkungen erzeu-

gen können in Form von steigender Emigration. Für Senderstaaten, also sanktionierende Staaten, können daraus neue politische Kosten entstehen, etwa steigender Migrationsdruck oder humanitäre Verantwortung. Insgesamt liefert das Papier einen Beitrag zur Sanktions- und Migrationsforschung. Internationale Sanktionen beeinflussen nicht nur Regierungen und Märkte, sondern auch individuelle Lebensentscheidungen. Besonders multilaterale Sanktionen erhöhen die Auswanderung deutlich. Dieser Effekt ist vor allem in repressiven Staaten stark ausgeprägt, in denen Emigration zur Alternative politischer Mitsprache wird. Die Studie macht damit deutlich, dass Sanktionen stets auch als gesellschaftspolitisches Instrument mit weitreichenden humanitären Folgen betrachtet werden müssen. Wer Sanktionen verhängt, übt nicht nur Druck auf Regierungen aus, sondern verändert möglicherweise zugleich die Migrationsdynamik ganzer Gesellschaften.

2. Internationale Sanktionen und Staatsstreiche (Kapitel 3)

Die Studie "International Sanctions and Coups d'État" erforscht den Zusammenhang zwischen internationalen Sanktionen und dem Risiko von Staatsstreichen in sanktionierten Ländern. In der Forschung ist bisher die Frage offen, ob Sanktionen politische Machtwechsel durch innerstaatliche Eliten – insbesondere durch das Militär – begünstigen oder verhindern. Das Forschungspapier schließt diese Forschungslücke, indem es analysiert, wie Sanktionen die Wahrscheinlichkeit von Coups d'État beeinflussen und unter welchen politischen Rahmenbedingungen dieser Effekt besonders stark ausfällt.

Sanktionen werden häufig mit dem Ziel verhängt, politische Verhaltensänderungen oder sogar einen Führungswechsel im Zielland herbeizuführen. Ob sie diese Ziele tatsächlich erreichen, ist jedoch umstritten. Einerseits können Sanktionen Regierungen destabilisieren, wirtschaftliche Ressourcen verringern, Elitennetzwerke schwächen und dadurch innerstaatliche Herausforderer stärken. Andererseits können sie einen Rally-around-the-flag-Effekt auslösen, indem sich Bevölkerung oder politische Eliten angesichts externen Drucks hinter die amtierende Führung stellen.

Zur theoretischen Einordnung wird auf wegweisende Forschungsarbeiten in der Autokratieforschung von Tullock (1974, 1987, 2005) und Wintrobe (1990, 1998, 2012) sowie auf rational-choice-orientierte Coup-Theorien zurückgegriffen (Finer 1988; Galetovic and Sanhueza 2000; Gassebner et al. 2016; Leon 2014; Aidt and Leon 2019). Ein Staatsstreich wird demnach als strategisch-rationale Entscheidung politischer oder militärischer Eliten verstanden. Diese wägen die Kosten und den Nutzen eines Umsturzes rational gegeneinander ab. Gemäß Gassebner et al. (2016) ergibt sich daraus, dass der erwartete Nutzen für die potenziellen Putschisten nach dem Coup ($E(U_{coup})$ größer sein muss als als der aktuelle Nutzen ($U_{status\ quo}$). Der erwartete Nutzen ist dabei formal darstellbar als

$$E(U_{coup}) = p \times B + (1 - p) \times C \tag{1}$$

, wobei p die Erfolgswahrscheinlichkeit eines Coups, B den positive Nutzen im Falle eines erfolgreichen Coups und C die Kosten eines nicht-erfolgreichen Umsturzes darstellen. Dabei ist es wichtig festzuhalten, dass sich die obige Kosten-Nutzen-Abwägung ausschließlich auf potenzielle Coup-Teilnehmer, d.h. staatliche Eliten, bezieht.

Gleichzeitig betont die Studie, dass nicht alle politischen Systeme gleich reagieren. Demokratische Staaten verfügen über institutionalisierte Mechanismen des Machtwechsels, vor allem in Form von Wahlen, wodurch Unzufriedenheit eher zur Abwahl der politischen Führung als zu Coups führen dürften. Autoritäre Systeme hingegen besitzen oft keine friedlichen Alternativen zum Machtwechsel, aber gleichzeitig kann ein Rally-around-the-flag-Effekt auftreten. Daher formuliert das Papier drei zentrale Hypothesen: (1a) Sanktionen erhöhen das Coup-Risiko oder (1b) Sanktionen verringern das Coup-Risiko. (2) Ein positiver Effekt sollte besonders stark in autoritären Regimen auftreten und (3) institutionell stärker verankerte Autokratien sind widerstandsfähiger gegenüber Sanktionen als institutionell schwächer verankerte Autokratien.

Die Untersuchung stützt sich auf einen Paneldatensatz mit 6.070 Länder-Jahr-Beobachtungen für 158 Staaten im Zeitraum von 1971 bis 2019. Informationen zu Sanktionen stammen aus der GSDB, Daten zu Coups aus dem Coup Leader Dataset (Eschenauer-Engler and Herre 2023). Insgesamt werden 197 Staatsstreiche erfasst, von denen 92 erfolgreich verliefen.

Unterschieden wird zwischen zwei Gruppen von Sanktionen: (i) Westliche Sanktionen verhängt durch die USA und/oder EU-Staaten und (ii) Nicht-westliche Sanktionen, insbesondere durch Russland, China oder die Vereinten Nationen. Die Studie nutzt mehrere methodische Ansätzen. Im Zentrum stehen Panel-Logit-Modelle, mit denen die Wahrscheinlichkeit eines Coups unter Berücksichtigung von Sanktionen geschätzt wird. Ergänzt wird dies durch Event-Study-Analysen, die Entwicklungen vor und nach der Einführung von Sanktionen sichtbar machen, sowie Propensity-Score-Weighting, um die Vergleichbarkeit sanktionierter und nicht sanktionierter Länder zu verbessern. Da Staatsstreiche seltene Ereignisse darstellen, kommen zusätzlich spezielle Schätzmethoden für seltene Ereignisse zum Einsatz.

Die Ergebnisse zeigen, dass westliche Sanktionen das Coup-Risiko signifikant erhöhen. Im Durchschnitt steigt die Wahrscheinlichkeit eines Staatsstreichs um 1,7 bis 1,9 Prozentpunkte. Da Coups insgesamt selten auftreten, ist dieser Effekt substanziell, zum Teil sogar eine Verdopplung. Bei erfolgreichen Staatsstreichen liegt der geschätzte Effekt sogar bei 2,1 bis 2,3 Prozentpunkten. Für nicht-westliche Sanktionen findet die Studie hingegen keinen robusten statistischen Zusammenhang mit Coup-Ereignissen. Daraus lässt sich schließen, dass insbesondere westliche Sanktionen innerstaatliche Machtverhältnisse stärker beeinflussen.

Die dynamische Analyse zeigt, dass die Wirkung westlicher Sanktionen vor allem zu Beginn eines Sanktionsregimes sichtbar wird. Im ersten Jahr nach Einführung steigt die Wahrscheinlichkeit eines Coups um etwa 7,8 Prozentpunkte. In den Folgejahren bleiben die Koeffizienten zwar positiv, verlieren jedoch an statistischer Signifikanz.

Ein wesentlicher Beitrag der Studie liegt in der Analyse unterschiedlicher Herrschaftsformen. Der positive Zusammenhang zwischen Sanktionen und Coups

zeigt sich vor allem in autoritären Regimen. Werden demokratische Staaten aus der Stichprobe ausgeschlossen, fällt der Effekt deutlich stärker aus. Innerhalb der Gruppe autoritärer Systeme ergeben sich weitere Unterschiede. Personalistische Regime reagieren besonders empfindlich. Militärregime weisen ebenfalls erhöhte Putschrisiken auf. Einparteienregime erscheinen widerstandsfähiger, da sie über institutionalisierte Entscheidungsstrukturen und interne Karrieremechanismen verfügen. Diese Befunde unterstreichen, dass Sanktionen nicht pauschal wirken, sondern stark von der institutionellen Beschaffenheit des Ziellandes abhängen.

Ein weiterer Befund betrifft die historische Entwicklung. Für die Phase des Kalten Krieges findet die Studie keinen belastbaren Zusammenhang zwischen Sanktionen und Coups. Erst nach 1991 wird ein klar positiver Effekt sichtbar. In der Zeit nach dem Zusammenbruch der Sowjetunion erhöhen westliche Sanktionen die Wahrscheinlichkeit eines Coups um rund 1,8 Prozentpunkte, bei erfolgreichen Coups sogar um etwa 2,3 Prozentpunkte. Dies kann damit erklärt werden, dass während des Kalten Krieges häufiger direkte Interventionen, Geheimdienstoperationen oder militärische Einflussnahme eingesetzt wurden. Seit den 1990er Jahren haben Sanktionen diese Instrumente teilweise ersetzt und sind zu einem zentralen außenpolitischen Mittel geworden.

Die Resultate zeigen, dass Sanktionen autoritäre Regime tatsächlich destabilisieren können. Ein Staatsstreich bedeutet allerdings nicht zwangsläufig eine Demokratisierung, Rechtsstaatlichkeit oder politische Stabilität, sondern Coups können ebenso neue Gewaltzyklen, Machtkämpfe oder erneute autoritäre Herrschaft hervorbringen. Daraus folgt, dass die politischen Nebenwirkungen von Sanktionen stärker berücksichtigt werden sollten, insbesondere durch beispielsweise gezielte Sanktionen gegen einzelne Eliten, da diese selektiveren Druck erzeugen könnten als breit angelegte Wirtschaftssanktionen.

Insgesamt zeigt der Artikel, dass internationale Sanktionen nicht nur ökonomische und gesellschaftliche Folgen haben, sondern auch die politische Stabilität sanktionierter Staaten beeinflussen. Besonders westliche Sanktionen erhöhen das Risiko

von Staatsstreichen, vor allem in autoritären Regimen mit schwacher institutioneller Verankerung. Die Studie verdeutlicht damit, dass Sanktionen tief in innerstaatliche Machtstrukturen eingreifen können. Wer Sanktionen verhängt, beeinflusst nicht nur wirtschaftliche Entwicklungen, sondern unter Umständen auch die Frage, wer in einem Staat regiert und auf welche Weise Machtwechsel stattfinden.

3. Gemeinsamkeiten politischer Führungspersönlichkeiten und internationale Sanktionen (Kapitel 4)

Unser Paper "Leader Similarity and International Sanctions“ untersucht, ob persönliche Ähnlichkeiten zwischen politischen Staats- und Regierungschefs die Wahrscheinlichkeit beeinflussen, dass Staaten Sanktionen gegeneinander verhängen. Entgegen einer gängigen Annahme, dass internationale Politik ausschließlich durch institutionelle oder strukturelle Faktoren bestimmt wird, gehen wir davon aus, dass auch individuelle Eigenschaften politischer Führungspersonen eine wichtige Rolle spielen (Byman and Pollack 2001; Saunders 2011). Wir argumentieren, dass ähnliche Führungspersonen ein höheres Maß an Vertrauen, besseres gegenseitiges Verständnis sowie geringere Kommunikationskosten aufweisen. Dadurch könnten Konflikte häufiger diplomatisch gelöst werden, bevor wirtschaftliche oder politische Zwangsmaßnahmen notwendig werden. Theoretisch stützt sich die Studie auf Ansätze der Homophilie, wonach Ähnlichkeit Kooperation erleichtert (Currarini and Mengel 2016), auf die In-Group-Bias-Forschung, die größeres Vertrauen gegenüber ähnlichen Akteuren betont (Tajfel and Turner 1986), sowie auf verhaltenswissenschaftliche Perspektiven der Internationalen Beziehungen, die die Rolle persönlicher Wahrnehmungen und Interaktionen hervorheben (Holmes 2018; Holmes and Wheeler 2020). Daraus leiten wir die Hypothese ab, dass Staaten mit einander ähnlichen politischen Führern seltener Sanktionen gegeneinander einsetzen.

Zur empirischen Überprüfung kombinieren wir die GSDB-Daten mit einem Datensatz zu Eigenschaften politischer Führungspersonen (DiLorenzo and Rooney 2025). Analysiert werden 293.594 Beobachtungen von 156 potenziellen Senderstaaten und

155 Zielstaaten im Zeitraum 1970 bis 2004. Die Ähnlichkeit der Führungspersonen wird anhand von 58 Merkmalen gemessen, darunter Bildung, militärische Erfahrung, beruflicher Hintergrund, Familienmerkmale und politische Laufbahn. Mithilfe von Panel-Probit-Modellen mit Sender-Jahr- und Dyaden-Fixed-Effects zeigen die Ergebnisse, dass größere Ähnlichkeit die Wahrscheinlichkeit von Sanktionen signifikant reduziert. Besonders stark ist dieser Effekt bei einseitig verhängten Sanktionen außerhalb multilateraler Organisationen wie United Nations oder European Union. Die geschätzte Wahrscheinlichkeit solcher Sanktionen steigt von 2,3 % bei maximaler beobachteter Ähnlichkeit auf 7,2 % bei minimaler Ähnlichkeit. Damit besitzt die persönliche Beziehung zwischen politischen Führern einen substanziellen Einfluss auf außenpolitisches Konfliktverhalten.

Darüber hinaus identifiziert die Studie diverse Kontextbedingungen. Der Einfluss von Führungsähnlichkeit ist besonders ausgeprägt bei Sanktionen, die demokratische Reformen oder Verbesserungen der Menschenrechte erzwingen sollen, sowie bei nichthandelsbezogenen Sanktionen (z. B. Reise-, Finanz- oder Waffenrestriktionen). Ebenso zeigt sich der Effekt stärker in Beziehungen, an denen mindestens ein autokratischer Staat beteiligt ist sowie in der Zeit nach dem Ende des Kalten Krieges. Wir schließen daraus, dass individuelle Merkmale politischer Eliten ein bislang unterschätzter Faktor der Sanktionspolitik sind. Insgesamt leistet das Paper damit einen Beitrag zur verhaltensorientierten Forschung der Internationalen Beziehungen, indem es zeigt, dass nicht nur Staateninteressen und Institutionen, sondern auch persönliche Eigenschaften und zwischenmenschliche Dynamiken von Führungspersonen internationale Kooperation und Konflikte prägen.

4. Internationale Sanktionen und Korruption (Kapitel 5)

Wir erörtern in dem vorliegenden Beitrag erstmals systematisch, wie internationale Sanktionen die Korruption im öffentlichen Sektor beeinflussen. Ausgangspunkt unserer Analyse ist eine in Politik und Forschung kontrovers diskutierte Frage: Einerseits wird häufig argumentiert, Sanktionen verschärften Knappheiten, stärkten

Schwarzmärkte und eröffneten zusätzliche Gelegenheiten für Bestechung, Patronage und illegale Rentenabschöpfung. Andererseits werden Sanktionen zunehmend gezielt eingesetzt, um autoritäre Eliten unter Druck zu setzen, demokratische Reformen zu fördern oder korrupte Akteure direkt zu bestrafen. Bislang fehlte jedoch eine breit angelegte empirische Untersuchung, die diese konkurrierenden Annahmen international überprüft. Genau an dieser Stelle setzt unsere Studie an.

Theoretisch lassen sich beide Wirkungsrichtungen plausibel begründen. Frühere Fallstudien, etwa von Andreas (2005), zeigen, dass Sanktionen Umgehungsökonomien fördern können. Wenn Importe, Kapitalflüsse oder internationale Transaktionen eingeschränkt werden, entstehen Anreize zur illegalen Beschaffung knapper Güter. Solche Prozesse laufen häufig nicht ohne Mitwirkung staatlicher Stellen ab, sodass Korruption zunehmen kann. Ergänzend weisen Early and Peksen (2019) sowie Farzanegan (2013) darauf hin, dass Sanktionen informelle Wirtschaftssektoren stärken, in denen staatliche Kontrolle schwächer ist. Gleichzeitig kann Korruption institutionell verfestigt werden: Mishra (2006) und Tirole (1996) argumentieren, dass kurzfristige Schocks dauerhafte korrupte Normen etablieren können. Würde dies auf einen solchen Schock in From von Sanktionen zutreffen, wären auch nach deren Aufhebung langfristige Schäden zu erwarten. Somit lauten unsere Hypothesen, dass (1) Sanktionen einen Anstieg der Korruption verursachen und (2) dieser Anstieg sogar bestehen bleibt nach der Beendigung der Sanktionierung.

Demgegenüber steht eine zweite Perspektive, formuliert in einer alternativen Hypothese, nach der (3) Sanktionen Korruption reduzieren. Moderne Sanktionsregime richten sich häufig gegen politische Eliten, Oligarchen, Unternehmen mit Regierungsnähe oder Personen, denen Menschenrechtsverletzungen und Korruption vorgeworfen werden. Besonders hervorzuheben ist hier die Entwicklung gezielter Sanktionierung, die explizit auf die Korruptionsbekämpfung abzielt (Hamilton et al. 2024; Moiseienko 2024). Sanktionen können damit korrupte Netzwerke schwächen, Machtressourcen beschneiden und Reformdruck erzeugen. Darüber hinaus zeigen Studien von Allen (2005, 2008), Lektzian and Souva (2007) oder Major

(2012), dass demokratische Regime eher auf externen Druck reagieren als Autokratien. Daraus leiten wir die Erwartung ab, dass (4) insbesondere Demokratien unter Sanktionen institutionelle Anpassungen vornehmen und Korruption reduzieren.

Um diese Hypothesen zu testen, verwenden wir einen umfangreichen Länderdatensatz mit 5.843 Länder-Jahr-Beobachtungen für den Zeitraum 1971 bis 2019. Erfasst werden Sanktionen gegen 125 Staaten. Die Sanktionsdaten stammen aus der GSDB. Als Maß für Korruption nutzen wir vier Indikatoren aus dem Varieties of Democracy (V-Dem)-Datensatz (Coppedge et al. 2024): Regimekorruption *(Regime Corruption)*, politische Korruption *(Political Corruption)*, Exekutivkorruption *(Executive Corruption)* sowie Korruption im öffentlichen Sektor *(Public Sector Corruption)*. Diese Indizes decken unterschiedliche Dimensionen ab, korrelieren aber stark miteinander. Dadurch können wir robuste Aussagen treffen und gleichzeitig differenzieren, in welchen Staatsbereichen sich Veränderungen besonders zeigen.

Methodisch kombinieren wir erneut zwei empirische Ansätze. Erstens schätzen wir DiD-Modelle mit fixen Länder- und Jahres-Effekten. Dadurch vergleichen wir Korruptionsentwicklungen sanktionierter Staaten mit nicht sanktionierten Staaten unter Kontrolle zeitinvarianter Ländereigenschaften und globaler Trends. Zweitens nutzen wir Event-Study-Modelle, um zeitliche Dynamiken vor, während und nach Sanktionen sichtbar zu machen. Ergänzend kontrollieren wir ökonomische und politische Einflussfaktoren wie das Pro-Kopf-BIP, Ressourcenrenten, Ungleichheit, Demokratiegrad, Menschenrechtslage sowie bewaffnete Konflikte. Diese Vorgehensweise soll Selektionsprobleme reduzieren und eine kausale Interpretation ermöglichen. Wir unterscheiden hier zwischen sechs Sanktionsindikatoren: (i) Westliche (d.h. EU und/oder US) Sanktionen gegen Demokratien, (ii) Westliche Sanktionen gegen Autokratien, (iii) UN Sanktionen gegen Demokratien, (iv) UN Sanktionen gegen Autokratien, (v) Nicht-westliche (d.h. russische und/oder chinesische) Sanktionen gegen Demokratien, und (vi) Nicht-westliche Sanktionen gegen Autokratien.

Unsere zentralen Ergebnisse widersprechen der verbreiteten Annahme, Sanktionen erhöhten systematisch die Korruption. Vielmehr zeigen sich deutliche Unter-

schiede abhängig vom Regime des sanktionierten sowie des sanktionierenden Landes. Westliche Sanktionen, d.h. Sanktionen der USA oder der EU, senken Korruption in Demokratien signifikant. Je nach Indikator liegen die geschätzten Effekte zwischen -0,052 und -0,067 auf einer normierten Skala von 0 bis 1. Auch UN-Sanktionen gegen Demokratien weisen ähnliche Muster auf. Für Sanktionen gegen Autokratien finden wir hingegen keine systematischen Effekte. Ebenso zeigen Sanktionen Chinas oder Russlands keine konsistenten Auswirkungen. Diese Differenz zwischen Demokratien und Autokratien ist politökonomisch plausibel. Demokratien verfügen über stärker ausgeprägte Mechanismen öffentlicher Kontrolle, Wahlen, Medienfreiheit und eine ernstzunehmende Opposition. Wenn Sanktionen wirtschaftlichen oder reputativen Druck erzeugen, steigt dort die Wahrscheinlichkeit politischer Reaktionen. Autokratien dagegen stützen sich oft auf Patronage, klientelistische Netzwerke und selektive Ressourcenverteilung. Korruption ist dort nicht bloß das Fehlverhalten einzelner Akteure, sondern häufig Teil des Herrschaftsmechanismus. Eine Reduktion korrupter Praktiken könnte daher die Machtbasis des Regimes gefährden, weshalb externer Druck weniger Wirkung entfaltet.

Besonders aufschlussreich sind die dynamischen Befunde der Event-Study-Analyse. Vor Beginn westlicher Sanktionen gegen Demokratien beobachten wir keine signifikanten Voreffekte. Das stärkt die Annahme paralleler Trends und damit die Glaubwürdigkeit einer kausalen Interpretation. Die korruptionsmindernden Effekte setzen nicht sofort ein, sondern erst nach etwa drei bis vier Jahren. Danach bleiben sie während der gesamten Sanktionsdauer bestehen und erreichen ihren Höhepunkt etwa im achten oder neunten Jahr. Unmittelbar nach Aufhebung der Sanktionen kehren die Korruptionswerte jedoch wieder ungefähr auf das Ausgangsniveau zurück. Sanktionen erzeugen also temporäre Disziplinierungswirkungen, aber keine dauerhafte institutionelle Transformation.

Ein weiterer wichtiger Befund betrifft die Zielsetzung von Sanktionen. Wir zeigen, dass Maßnahmen mit explizitem Fokus auf Demokratisierung oder Menschenrechte stärkere korruptionssenkende Effekte entfalten als Sanktionen mit anderen Zielen.

Daraus lässt sich schließen, dass die konkrete Ausgestaltung und kommunikative Legitimation von Sanktionen entscheidend ist. Werden institutionelle Reformen klar eingefordert, steigt offenbar der Druck auf Regierungen, sichtbare Verbesserungen vorzunehmen. Unsere Robustheitsanalysen bestätigen die Stabilität der Resultate. Auch wenn die hohe Persistenz von Korruption berücksichtigt wird, bleiben die Haupteffekte bestehen. Dasselbe gilt bei Anwendung moderner zweistufiger Difference-in-Differences-Schätzer (2SDiD) nach Gardner et al. (2025) und Butts and Gardner (2022), die Probleme gestaffelter Behandlungs- bzw. sogenannter *Treatment*-eintritte und heterogener Behandlungseffekte adressieren.

Zusammenfassend kommen wir zu dem Schluss, dass internationale Sanktionen differenziert bewertet werden müssen. Die pauschale Kritik, Sanktionen würden zwangsläufig Korruption verschärfen, findet in unseren breit angelegten Daten keine Bestätigung. Stattdessen zeigen westliche und teilweise UN-Sanktionen gegenüber Demokratien eine messbare, wenn auch zeitlich begrenzte Verringerung öffentlicher Korruption. Zugleich offenbart das Ausbleiben dauerhafter Effekte eine wesentliche Grenze dieses Instruments: Sanktionen können Fehlverhalten temporär eindämmen, ersetzen aber keine tiefgreifenden institutionellen Reformen. Für die wissenschaftliche Debatte bedeutet dies, dass Sanktionen weniger als dauerhaftes Antikorruptionsinstrument, sondern eher als kurzfristiges Druckmittel verstanden werden können.

5. Internationale Sanktionen und Verfassungskonformität (Kapitel 6)

Im finalen Forschungsartikel der vorliegenden Dissertation untersuchen wir, wie internationale Sanktionen die Verfassungskonformitat von Regierungen, d.h. die Einhaltung der eigenen nationalen Verfassung, beeinflussen. Die Grundidee unserer Analyse ist, dass Sanktionen häufig anhand universeller Maßstäbe, insbesondere der Menschenrechte, bewertet werden. Ein solcher Bewertungsmaßstab ist jedoch politisch umstritten, da Regierungen sanktionierter Staaten ihn oft als Ausdruck westlicher

Normsetzung zurückweisen. Deshalb schlagen wir einen alternativen Zugang vor: Statt Regierungen an externen bzw. vermeintlich universellen Standards zu messen, bewerten wir sie daran, ob sie ihre eigenen verfassungsrechtlichen Verpflichtungen einhalten. Die nationale Verfassung verstehen wir dabei als kodifizierte Form des Gesellschaftsvertrages („social contract") eines Staates.

Ziel der Studie ist es somit zu klären, ob internationale Sanktionen Regierungen dazu bewegen, sich stärker an ihre Verfassung zu halten oder ob sie im Gegenteil verfassungswidriges Regierungshandeln begünstigen. Dabei knüpfen wir an eine umfangreiche Literatur an, die bereits negative Nebenwirkungen von Sanktionen auf Wohlstand, Ungleichheit, Gesundheit und Menschenrechte dokumentiert hat. Unsere Studie erweitert diese Forschungsperspektive um die Frage der Verfassungstreue bzw. der verfassungsrechtlichen Compliance.

Theoretisch sind sowohl positive als auch negative Auswirkungen von Sanktionen denkbar. Einerseits erzeugen Sanktionen politischen und ökonomischen Druck auf Regierungen. Da Sanktionen häufig mit der Aussicht auf Aufhebung bei Verhaltensänderung verbunden sind, könnten sie gemäß unserer Hypothese (1a) Regierungen motivieren, verfassungswidrige Praktiken zu beenden. Zudem können Sanktionen oppositionellen Gruppen und Bürgern als Koordinationspunkt dienen, um verfassungswidrige Herrschaft stärker zu kritisieren. Andererseits (1b) könnten Sanktionen verfassungswidrige Praktiken stärken, da sie staatliche Ressourcen schwächen, wirtschaftliche Lasten erhöhen und autoritäre Reaktionen provozieren können. Besonders in Autokratien könnte der Machtapparat stärker auf Repression zurückgreifen, um politische Stabilität zu sichern. Ebenso denkbar ist der bereits diskutierte Rally-around-the-flag-Effekt. Schließlich können Regierungen unter Hinweis auf die Sicherheitslage Ausnahmezustände ausrufen und damit verfassungsrechtliche Schranken umgehen.

Daraus leiten wir ab, dass die Wirkung von Sanktionen sowohl vom sanktionierenden Akteur als auch vom Regimetyp des sanktionierten Landes abhängt. In Hypothese (2) nehmen wir an, dass insbesondere Sanktionen westlicher Staaten sowie

der Vereinten Nationen zu einer höheren Verfassungstreue beitragen können, da sie häufig mit normativen Zielen wie Demokratie- und Menschenrechtsschutz begründet werden. Zugleich erwarten wir gemäß Hypothese (3a), dass Demokratien im Angesicht von Sanktionen die Verfassungskonformität erhöhen, weil Regierungen auf breite Wählerunterstützung angewiesen bleiben und den steigenden Druck politisch berücksichtigen müssen. Autokratien hingegen verfügen über schmalere Machtkoalitionen, stärkere Medienkontrolle und größere Möglichkeiten repressiver Krisenreaktionen. Daher gehen wir in Hypothese (3b) davon aus, dass Sanktionen dort eher zu sinkender Verfassungstreue führen, weil Regime ihre Herrschaft verstärkt durch Unterdrückung und Umgehung verfassungsrechtlicher Grenzen absichern.

Empirisch basiert unsere Untersuchung auf einem globalen Paneldatensatz mit 182 Staaten im Zeitraum 1962 bis 2022. Die Stichprobe umfasst 8.751 Länderjahre, darunter 2.106 Beobachtungen mit aktiven Sanktionen. Als Datenquelle für Sanktionen verwenden wir erneut die GSDB, welche Sanktionen westlicher Staaten, der Vereinten Nationen sowie Chinas und Russlands erfasst. Wir nutzen dabei diegleichen sechs Sanktionsindikatoren wie im zuvor diskutierten Forschungspapier. Für die Messung der Verfassungstreue greifen wir auf die Comparative Constitutional Compliance Database (CCCD) zurück (Gutmann, Metelska-Szaniawska, et al. 2024a). Diese Datenbank verknüpft formale Verfassungsrechte mit ihrer tatsächlichen Umsetzung in fünf Bereichen: Regierungsverantwortlichkeit *(Government Accountability Compliance)*, grundlegende Menschenrechte *(Basic Human Rights Compliance)*, bürgerliche Rechte *(Civil Rights Compliance)*, politische Rechte *(Political Rights Compliance)* sowie Eigentumsrechte *(Property Rights Compliance)*.

Im Zentrum der Analyse steht das bereits zuvor erwähnte Two-Stage DiD-Modell nach Gardner et al. (2025) und Butts and Gardner (2022), das speziell für gestaffeltes Treatment (hier: Sanktionierung) und heterogene Effekte entwickelt wurde. Ergänzend schätzen wir dynamische Wirkungen mittels Local Projection Difference-in-Differences (LP-DiD) nach Dube et al. (2025). Dadurch können wir nicht nur durchschnittliche Effekte identifizieren, sondern auch deren zeitliche Entwick-

lung vor und nach Beginn einer Sanktion untersuchen. Wie bereits erläutert, reduziert diese Vorgehensweise bekannte Verzerrungen klassischer Event Study-Ansätze, indem sie direkt zeithorizontspezifische Treatmenteffekte schätzt und somit auch bei zeitlich gestaffelten Eingriffen und variierenden Treatmenteffekten zuverlässig bleibt.

Die Ergebnisse zeigen zunächst keinen einheitlichen Gesamteffekt internationaler Sanktionen. Entscheidend ist vielmehr, ob das Zielland demokratisch oder autokratisch regiert wird. Für Demokratien beobachten wir nach Beginn von Sanktionen einen graduellen und statistisch signifikanten Anstieg der Verfassungstreue. Vier Jahre nach Einsetzen einer Sanktion werden positive Effekte deutlich. Diese Entwicklung deutet darauf hin, dass demokratische Regierungen auf Sanktionsdruck reagieren und ihre Politik stärker an rechtliche Bindungen anpassen. Für Autokratien ergibt sich das entgegengesetzte Bild. Nach Einsetzen von Sanktionen sinkt die Verfassungstreue kontinuierlich und signifikant. Dies spricht dafür, dass autokratische Regime unter externem Druck verstärkt auf Repression, Machtkonzentration und rechtsstaatswidrige Maßnahmen setzen. Sanktionen verschlechtern hier also gerade jene institutionellen Standards, deren Verbesserung sie häufig bezwecken wollen.

Bei der Differenzierung nach Sendern zeigt sich, dass UN-Sanktionen gegen Demokratien besonders wirksam sind. Dagegen verschlechtern westliche Sanktionen gegen Autokratien die Verfassungstreue, UN-Sanktionen gegen Autokratien sogar etwas mehr. Sanktionen Chinas und Russlands zeigen hingegen keine statistisch belastbaren Effekte.

Besonders relevant ist die Frage, ob Sanktionen mit ausdrücklich menschenrechtlicher oder demokratischer Zielsetzung erfolgreicher sind. Unsere Ergebnisse zeichnen ein gemischtes Gesamtbild, wobei sich grundsätzlich festhalten lässt, dass Sanktionen, die offiziell Demokratie oder Menschenrechte fördern sollen, nicht systematisch besser abschneiden als Sanktionen mit anderen Zielen. Damit bestätigen wir frühere Skepsis gegenüber sogenannten normativ begründeten Sanktionen (Steinbach et al. 2023).

Auch die Analyse einzelner Rechtsbereiche liefert ein differenziertes Bild. Bei Demokratien verbessern UN-Sanktionen vor allem die Einhaltung grundlegender Menschenrechte sowie politischer Rechte. In Autokratien hingegen verschlechtern westliche und UN-Sanktionen mehrere Bereiche gleichzeitig, darunter Eigentumsrechte, politische Rechte, bürgerliche Freiheiten und Regeln staatlicher Verantwortlichkeit.

Abschließend kommen wir zu dem Schluss, dass internationale Sanktionen kein universell geeignetes Instrument zur Durchsetzung rechtsstaatlicher Standards sind. Sie können Regierungen nur dann zu stärkerer Verfassungstreue bewegen, wenn diese Regierungen auf breite gesellschaftliche Unterstützung angewiesen sind – also vor allem in Demokratien. Dort erzeugen Sanktionen politischen Anpassungsdruck. In Autokratien hingegen verstärken Sanktionen tendenziell Machtmissbrauch und verfassungswidriges Regierungshandeln. Für die Praxis bedeutet dies, dass Sanktionen als außenpolitisches Instrument wesentlich selektiver bewertet werden müssen. Wer institutionelle Verbesserungen erreichen will, sollte stärker berücksichtigen, welche innenpolitischen Strukturen im Zielland bestehen und welche unbeabsichtigten Nebenwirkungen Sanktionen auslösen können.

Le temps passe, Nous restons...

Curriculum Vitae

EDUCATION

2021 – 2026	PhD/Doctorate in Economics, *Trier University* (Germany)
2025 – 2026	Matriculated Visiting DPhil Student, *University of Oxford* (United Kingdom)
2022 – 2023	UniGr Visiting PhD Student, Doctoral School in Economics, Finance, and Management, *University of Luxembourg*
2018 – 2022	M.A. in International Economics & Public Policy, *Trier University* (Germany)
2019 – 2021	M.Sc. in Applied Statistics, *Trier University* (Germany)
2020 – 2021	Erasmus+ Exchange, Economics & Data Science, *University of Liège*, *Hautes Études Commerciales/HÉC* (Belgium)
2015 – 2018	B.Sc. in Economics, *Trier University* (Germany)
2014 – 2017	B.A. in Political Science, *Trier University* (Germany)
2017	Erasmus+ Exchange, Economics, *Istanbul Technical University* (Türkiye)
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PUBLICATIONS

Leader Similarity & International Sanctions (with J. Gutmann and M. Neuenkirch), *Journal of Conflict Resolution*, 2026 (forthcoming).

International Sanctions & Emigration (with J. Gutmann and M. Neuenkirch), *Journal of Economic Behavior & Organization*, 2024, Vol. 226, 106709.

WORKING PAPERS & SELECTED WORK IN PROGRESS

International Sanctions & Coups d'État.

International Sanctions & Corruption (with J. Gutmann and M. Neuenkirch).

International Sanctions & Constitutional Compliance (with J. Gutmann and M. Neuenkirch).

The Dark Side of Sanctions: Evidence from Nighttime Lights (with K. Gründler, J. Gutmann and M. Neuenkirch).

Determinants of Inflation in the Eurozone: A Mixture Panel VAR Approach.

FURTHER CONTRIBUTIONS

A comment on 'Crisis in the Trajectory of Science: Evidence from the 2014 Ebola Outbreak' (with M. Baktash, S. Hartmann, M. Walz, and J. Weymeirsch), Replication Report.

Keeping Up with Psychological Science: PsychTopics – A Computational Assistance Tool for Researchers, Students, Journalists, and Policy-Makers (with A. Bittermann and S. Müller), Conference Paper.

FIELDS OF INTEREST

Geoeconomics, Political Economy, International Sanctions & Conflict Research, Macroeconomic Modelling, Economic Inequality, Economic History.

CONFERENCES & SEMINARS

2026	Political Economy Workshop (Oxford), European Public Choice Society Meeting (Madrid), Scotthish Economic Society Conference (Glasgow), Political Economy Workshop (Cambridge).
2025	European Public Choice Society Meeting (Riga), Workshop on Sanctions (Vienna).
2024	European Public Choice Society Meeting (Vienna), Political Economy of Democracy & Dictatorship Conference (Münster).

JOURNAL REVIEWER

International Economics, *Journal of Comparative Economics*, *PLOS ONE*, *Political Science*, *Zeitschrift für Psychologie*.

TEACHING EXPERIENCE

2026	Applied Time Series Econometrics (Graduate lecture)
2025 – 2026	International Macroeconomics (Graduate lecture)
2022 – 2026	Special Topics in International Macroeconomics (Graduate seminar)
2021 – 2025	Empirical Economic Research (Undergraduate seminar)
2019 – 2022	Integrated introduction to social science, economics, and business administration (Undergraduate tutorial)

PROFESSIONAL EXPERIENCE

2021 – Present	**Trier University** Research & Teaching Assistant
2020 – Present	**ViOffice**, Trier Co-Founder
2023 – 2025	**Leibniz Institute for Psychology**, Trier Data Scientist & UI/UX Designer
2018 – 2021	**Trier University** Student Assistant & Tutor
2018	**Ernst & Young (EY)**, Luxembourg Intern – Audit
2016	**European Parliament**, Brussels Intern – MEP's Office

GRANTS

2025 – 2026	Maintenance grant from *Jesus College Oxford*
2025 – 2026	Tuition waiver from *Jesus College Oxford*
2020 – 2021	*Erasmus+* scholarship for a graduate exchange
2016 – 2017	*Erasmus+* scholarship for undergraduate exchanges

SKILLS

Languages	German: Native. English: Advanced (C1–C2). French: Intermediate (B1). Latin: Latinum Certificate.
Programming	Statistical Programming: R, Python, Stata, SPSS. Web Development: R Shiny, HTML, CSS, JavaScript. Databases: SQL, Bash. Text Processing: MS Office, LaTeX.

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